TRANSACTIONS

OF THE

AMERICAN SOCIETY

OF

MECHANICAL ENGINEERS.

NOVEMBER, 1884, AND MAY, 1885.

XTH AND XITH MEETINGS.

VOL. VI.

NEW YORK CITY:
PUBLISHED BY THE SOCIETY,
AT THE OFFICE OF THE SECRETARY,
280 BROADWAY.

Press of J. J. Little & Co.,
Nos. 10 to 20 Astor Place, New York.

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REDMOND J. BROUGH .... July 21, 1883.
C. W. SIEMENS, Honorary Member .... Nov. 20, 1883.
HENRY F. SNYDER .... Nov. 25, 1883.
O. HALLAUER, Honorary Member .... Dec. 5, 1883.
WILLIAM ATWOOD .... Feb. 16, 1884.
WILMER G. CARTWRIGHT .... Feb. 23, 1884.
THEODORE H. RISDON .... May 19, 1884.
ISAAC NEWTON .... Sept. 25, 1884.
J. H. BURNETT .... Jan. 31, 1885.
HORACE LORD .... Feb. 28, 1885.
D. H. HOTCHKISS .... April 29, 1885.
HENRI TRESCA, Honorary Member .... June 24, 1885.
HENRY H. GORRINGE .... July 6, 1885.

RULES

OF THE

AMERICAN SOCIETY OF MECHANICAL ENGINEERS.

[*Adopted November 5th,* 1884.]

OBJECTS.

ART. 1. The objects of the AMERICAN SOCIETY OF MECHANICAL ENGINEERS are to promote the Arts and Sciences connected with Engineering and Mechanical Construction, by means of meetings for social intercourse and the reading and discussion of professional papers, and to circulate, by means of publication among its members, the information thus obtained.

MEMBERSHIP.

ART. 2. The Society shall consist of Members, Honorary Members, Associates and Juniors.

ART. 3. Mechanical, Civil, Military, Mining, Metallurgical and Naval Engineers and Architects may be candidates for membership in this Society.

ART. 4. To be eligible as a *Member*, the candidate must have been so connected with some of the above-specified professions as to be considered, in the opinion of the Council, competent to take charge of work in his department, either as a designer or constructor, or else he must have been connected with the same as a teacher.

ART. 5. *Honorary Members*, not exceeding twenty-five in number, may be elected. They must be persons of acknowledged professional eminence who have virtually retired from practice.

ART. 6. To be eligible as an *Associate*, the candidate must have such a knowledge of or connection with applied science as qualifies him, in the opinion of the Council, to co-operate with engineers in the advancement of professional knowledge.

ART. 7. To be eligible as a *Junior*, the candidate must have been in the practice of engineering for at least two years, or he must be a graduate of an engineering school.

The term "Junior" applies to the professional experience, and not to the age of the candidate. Juniors may become eligible to membership.

ART. 8. All Members and Associates shall be equally entitled to the privileges of membership. Honorary Members and Juniors shall not be entitled to vote nor to be members of the Council.

ELECTION OF MEMBERS.

ART. 9. Every candidate for admission to the Society, excepting candidates for honorary membership, must be proposed by at least three members, or members and associates, to whom he must be personally known, and he must be seconded by two others. The proposal must be accompanied by a statement in writing by the candidate of the grounds of his application for election, including an account of his professional experience, and an agreement that he will conform to the requirements of membership if elected.

ART. 10. All such applications and proposals must be received and acted upon by the Council at least thirty days before a regular meeting, when the Secretary shall at once mail to each member and associate, in the form of a letter ballot, the names of candidates recommended by the Council for election.

ART. 11. Any member or associate entitled to vote may erase the name of any candidate, and may, at his option, return to the Secretary such ballot enclosed in two envelopes, the inner one to be blank and the outer one endorsed by the voter.

ART. 12. The rejection of any candidate for admission as member, associate, or junior, by *seven* voters, shall defeat the election of said candidate. The rejection of any candidate for admission as honorary member by *three* voters shall defeat the election of said candidate.

ART. 13. The said blank envelopes shall be opened by the Council at any meeting thereof, and the names of the candidates elected shall be announced in the first ensuing meeting of the Society, and also in the first ensuing list of members. The names of candidates not elected shall neither be announced nor recorded in the proceedings.

ART. 14.—Candidates for admission as honorary members shall

not be required to present their claims; those making the nominations shall state the grounds therefor, and shall certify that the nominee will accept if elected. The method of election in other respects shall be the same as in case of other candidates.

Art. 15. All persons elected to the Society, excepting honorary members, must subscribe to the rules and pay to the Treasurer the initiation fee before they can receive certificates of membership. If this is not done within six months of notification of election, the election shall be void.

Art. 16. The proposers of any rejected candidate may, within three months after such rejection, lay before the Council written evidence that an error was then made, and if a reconsideration is granted, another ballot shall be ordered, at which thirteen negative votes shall be required to defeat the candidate.

Art. 17. Persons desiring to change the class of their membership shall be proposed in the same form as described for a new applicant.

FEES AND DUES.

Art. 18. The initiation fees of members and associates shall be $15, and their annual dues shall be $10, payable in advance. The initiation fee of juniors shall be $10, and their annual dues $5, payable in advance. A junior, being promoted to full membership, shall pay an additional initiation fee of $5. Any member or associate may become, by the payment of $150 at any one time, a life member or associate, and shall not be liable thereafter to annual dues.

Art. 19. Any member, associate or junior, in arrears may, at the discretion of the Council, be deprived of the receipt of publications, or stricken from the list of members, when in arrears for one year. Such person may be restored to membership by the Council on payment of all arrears, or by re-election after an interval of three years.

OFFICERS.

Art. 20. The affairs of the Society shall be managed by a Council, consisting of a President, six Vice-Presidents, nine Managers, and a Treasurer, who shall be elected from among the members and associates of the Society at the annual meetings, to hold office as follows:

Art. 21. The President and the Treasurer for one year; and

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no person shall be eligible for immediate re-election as President who shall have held that office for two consecutive years; the Vice-Presidents for two years, and the Managers for three years; and no Vice-President or Manager shall be eligible for immediate re-election to the same office at the expiration of the term for which he was elected.

ART. 22. A Secretary, who shall be a member of the Society, shall be appointed for one year by a majority of the members of the Council at its first meeting after the annual election, or as soon thereafter as the votes of a majority of the members of the Council can be secured for a candidate. The Secretary may be removed by a vote of twelve members of the Council, at any time after one month's notice has been given him by a majority of its members to show cause why he should not be removed, and he has been heard to that effect. The Secretary may take part in any of the deliberations of the Council, but shall not have a vote therein. His salary shall be fixed for the time he is appointed by a majority vote of the Council.

ART. 23. At each annual meeting, a President, three Vice-Presidents, three Managers and a Treasurer shall be elected, and the term of office of each shall continue until the end of the meeting at which their successors are elected.

ART. 24. The duties of all officers shall be such as usually pertain to their offices or may be delegated to them by the Council or by the Society. The Council may, in its discretion, require bonds to be given by the Treasurer.

ART. 25. The Council may, by vote of a majority of all its members, declare the place of any officer vacant, on his failure for one year, from inability or otherwise, to attend the Council meetings, or to perform the duties of his office. All such vacancies and those occurring by death or resignation shall be filled by the appointment of the Council, and any person so appointed shall hold office for the remainder of the term for which his predecessor was elected or appointed; *provided* that the said appointment shall not render him ineligible at the next annual meeting.

ART. 26. Five members of the Council shall constitute a quorum; but the Council may appoint an Executive Committee, or business may be transacted at a regularly called meeting of the Council, at which less than a quorum is present, subject to the approval of a majority of the Council, subsequently given in writing to the Secretary and recorded by him with the minutes. Absent mem-

bers of the Council may vote by proxy upon subjects stated in the call for a meeting, said proxy to be deposited with the Secretary.

ART. 27. The President on assuming office shall appoint a Finance Committee and a Publication Committee and a Library Committee of five members each. The appointment of two members of each Committee shall expire at the end of each year. The Secretary shall, *ex officio*, be a member of all three Committees.

ART. 28.—The Finance Committee shall have power to order all ordinary or current expenditures, and shall audit all bills therefor. No bill shall be paid except upon their audit. When special appropriations are ordered by the Society, they shall not take effect until they have been referred to the Council and Finance Committee in conference.

ART. 29. It shall be the duty of the Publication Committee to receive all papers contributed, to decide which shall be published in the *Transactions*, and which shall be read in full at the meetings.

ART. 30. It shall be the duty of the Library Committee to take charge of the collection of all material for the Library of the Society, and to supervise all regulations for its use.

ELECTION OF OFFICERS.

ART. 31. At the regular meeting preceding the annual meeting a nominating committee of five members, not officers of the Society, shall be appointed, and this committee shall, at least thirty days before the annual meeting, send to the Secretary the names of nominees for the offices falling vacant under the rules. In addition to such regularly appointed committee, any other five members or associates, not in arrears, may constitute an independent nominating committee, and may present to the Secretary, at least thirty days before the annual meeting, all the names of such candidates as they may select. All the names of such independent nominees shall be placed upon the ballot list with nothing to distinguish them from the nominees of the regular committee, and the Secretary shall at once mail the said list of names to each member and associate in the form of a letter ballot, it being understood that the assent of the nominees shall have been secured in all cases.

ART. 32. In the election of Vice-Presidents, each member and associate may cast as many votes as there are Vice-Presidents to be elected. He may give all these votes to one candidate, or dis-

tribute them among more, as he chooses.* Managers shall be voted for in the same way.

Art. 33. Any member or associate entitled to vote may vote by retaining or changing the names on said list, leaving names not exceeding in number the officers to be elected, and returning the list to the Secretary—such ballot inclosed in two envelopes, the inner one to be blank and the outer one to be indorsed by the voter. No member or associate in arrears since the last annual meeting shall be allowed to vote until said arrears shall have been paid.

Art. 34. The said blank envelopes shall be opened by tellers at the annual meeting, and the person who shall have received the greatest number of votes for the several offices shall be declared elected.

MEETINGS.

Art. 35. The annual meeting of the Society shall be held on the first Thursday in November of each year, in the City of New York, unless otherwise ordered, at which a report of proceedings and an abstract of the accounts shall be furnished by the Council. The Council may change the place of the annual meeting, and shall, in that case, give timely notice to members and associates.

Art. 36. Other regular meetings of the Society shall be held in each year at such time and place as the Council may appoint. At least thirty days' notice of all meetings shall be mailed by the Secretary to members, honorary members, associates and juniors.

Art. 37. Special meetings may be called whenever the council may see fit; and the Secretary shall call a special meeting at the written request of twenty or more members. The notices for special meetings shall state the business to be transacted, and no other shall be entertained.

Art. 38. Any member, honorary member or associate may introduce a stranger to any meeting; but the latter shall not take part in the proceedings without the consent of the meeting.

Art. 39. Every question which shall come before the Society shall be decided, unless otherwise provided by these rules, by the votes of a majority of the members and associates present, provided there is a quorum.

Art. 40. At any regular meeting of the Society thirteen or more members and associates shall constitute a quorum.

Art. 41. Unless otherwise ordered, papers shall be read in the

order in which their text is received by the Secretary. Before any paper appears in the *Transactions* of the Society a copy of the paper shall be sent to the author, and, so far as possible, a copy of the reported discussion shall be sent to every member who took part in the same, with requests that attention shall be called to any errors therein.

ART. 42. The Society shall claim no exclusive copyright in papers read at its meetings, nor in reports of discussions, except in the matter of official publication with the Society's imprint, as its *Transactions*. The Secretary shall have sole possession of papers between the time of their acceptance by the Publication Committee and their reading, together with the drawings illustrating the same; and at the time of such reading, or as soon thereafter as practicable, he shall cause to be printed, with the authors' consent, copies of such papers, "subject to revision," with such illustrations as are needed for the *Transactions*, for distribution to the members and for the use of technical newspapers, American and foreign, which may desire to reprint them in whole or in part. The policy of the Society in this matter shall be to give papers read before it the widest circulation possible, with the view of making the work of the Society known, encouraging mechanical progress, and extending the professional reputation of its members.

ART. 43. The author of each paper read before the Society shall be entitled to twelve copies, if printed, for his own use, and all members shall have the right to order any number of reprints of papers at a cost to cover paper and printing; *provided*, that said copies are not intended for sale.

ART. 44. The Society is not, as a body, responsible for the statements of fact or opinion advanced in papers or discussions, at its meetings; and it is understood that papers and discussions should not include matters relating to politics or purely to trade.

AMENDMENTS.

ART. 45. These rules may be amended, at any annual meeting, by a two-thirds vote of the members present; *provided*, that written notice of the proposed amendment shall have been given at a previous meeting.

TABLE OF CONTENTS, VOLUME VI.

LIST OF ILLUSTRATIONS, VOLUME VI.

PAPERS

OF THE

NEW YORK MEETING, 1884

CLII.

PROCEEDINGS

OF THE

AMERICAN SOCIETY OF MECHANICAL ENGINEERS.

NEW YORK, NOVEMBER 5, 1884.

Standing Committee on Regular Meetings.—Wm. Metcalf, M. C. Bullock, Jos. Morgan Jr., R. H. Soule, Harris Tabor, S. T. Wellman.

Local Committee for the New York Meeting.—ALLAN STIRLING, *Chairman*; F. R. Hutton, R. H. Thurston, Edward Weston, Fred'k M. Wheeler, Wm. H. Wiley.

THE Fifth Annual Meeting of the Society was called to order in the hall of the New York Academy of Medicine, No. 12 West Thirty-first Street, at 8 P.M., by President John E. Sweet. Mr. Horatio Allen, honorary member of the society, was present, and by invitation occupied a chair on the platform at the side of the President.

On motion of Mr. Oberlin Smith, the President appointed the tellers to count the ballots on the election of officers and the ballots on the Revision of the Rules. Messrs. Kent, Hand and Lipe were appointed such a committee to report at the session on the following morning.

President Sweet then delivered his annual address, prefacing it as follows:

"LADIES AND GENTLEMEN, AND FELLOW MEMBERS OF THE AMERICAN SOCIETY OF MECHANICAL ENGINEERS:

"As has before been announced, a resolution of the Council passed last April requires the presiding officer to deliver an address at the opening of the annual meeting.

"This resolution, had it been presented a year ago, or might it have come a year hence, would have received my hearty endorsement. It will, I believe, in future serve to bring out the best work of an over-modest president; it may serve to limit the less

modest chief to an annual address instead of permitting too frequent lucubrations, while to-night it may at least serve to shadow forth the egotism of the present incumbent.

"For reasons which will be prominently apparent, I have not attempted to imitate the presidential addresses, of which there have been given so many masterly examples by the presidents of both the English and the American Societies; and all I have to offer as a substitute is simply a collection of thoughts bound together not by strap or bolt, link or chain, tie or shackle-bar, but noontime thoughts bound together as we bound our toys in school-boy days, with bits of tangled twine.

"As the potter molds unpromising clay into beautiful forms, as the wood-carver whittles his rude blocks into graceful images, and as the smith shapes the ugly iron into useful forms, so the thoughts I have to present are but the iron, the wood, and the clay which, in other hands, might be shaped into the useful, the good and the beautiful."

At the close of the President's address, Prof. Egleston announced an important discovery of the existence in nearly all metals and alloys of a "critical temperature" for each material at which its strength and toughness were either impaired or destroyed, while the material preserves its usual qualities above and below that point. He promised to make more extended allusion to this matter hereafter.

At the close of this brief announcement, the Secretary made some references to the programme of the meeting, and a recess was taken till ten o'clock the next morning.

A supper was served in the dining-room connected with the hall, and the social reunion was much enjoyed.

SECOND DAY.

November 6.

The morning session was convened at 10 o'clock, in the Hall of the Academy of Medicine. The secretary's register included the following names of members in attendance:

Allen, Horatio, *Honorary Member*................Orange, N. J.
Almond, Thos. R...............................Brooklyn, N. Y.
Anthony, Gardner C............................Providence, R. I.
Babcock, Geo. H...............................New York City.
Bailey, Jackson...............................New York City.

Baldwin, Stephen W. New York City.
Bancroft J. Sellers. Philadelphia, Pa.
Barr, Wm. M. Brooklyn, N. Y.
Bayles, Jas. C. New York City.
Bergner, Theodore. Philadelphia, Pa.
Betts, Wm. Wilmington, Del.
Bigelow, Geo. W. New Haven, Conn.
Bond, Geo. M. Hartford, Conn.
Booraem, J. V. V. Brooklyn, N. Y.
Broadbent, Sidney. Scranton, Pa.
Burdsall, Elwood, Jr. Portchester, N. Y.
Burr, Jno. T. Brooklyn, N. Y.
Capen, Thos. W. Stamford, Conn.
Carr, C. A. Hoboken, N. J.
Cartwright, Robert. Stamford, Conn.
Chapman, Luke. Collinsville, Conn.
Christensen, Aug. C. Brooklyn, N. Y.
Churchill, Thomas L. Boston, Mass.
Clarke, Samuel J. New York City.
Colin, Alfred. New York City.
Collins, C. C. Newark, N. J.
Copeland, Chas. W. New York City.
Corbett, Chas. H. Brooklyn, N. Y.
Cotter, John. Norwalk, Conn.
Couch, Alfred B. Philadelphia, Pa.
Cowles, Wm. New York City.
Cullingworth, Geo. R. New York City.
Curtis, Gram. New York City.
Davis, D. P. New York City.
Davis, E. F. C. Pottsville, Pa.
Douglas, E. V. Philadelphia, Pa.
Du Faur, A. F. New York City.
Durfee, W. F. Bridgeport, Conn.
Edson, J. B. Brooklyn, N. Y.
Egleston, T. New York City.
Emery, A. H. Stamford, Conn.
Emery, C. E. New York City.
Fitch, Chas. H. Flushing, N. Y.
Fritz, John. Bethlehem, Pa.
Galloupe, F. E. Boston, Mass.
Gold, S. F. Englewood, N. J.
Good, W. E. Reading, Pa.
Grimshaw, Robert. New York City.
Hall, Albert F. Boston, Mass.
Halsey, F. A. New York City.
Hand, S. Ashton. Toughkenamon, Pa.
Harmon, O. S. Jersey City, N. J.
Hawkins, Jno. T. Taunton, Mass.
Hemenway, F. F. New York City.
Henthorn, Jno. T. Providence, R. I.
Hewitt, Wm. Trenton, N. J.

Hill, Wm .... Collinsville, Conn.
Hobbs, A. C .... Bridgeport, Conn.
Hollis, Ira N .... Schenectady, N. Y.
Holloway, J. F .... Cleveland, Ohio.
Hornig, Julius L .... Jersey City, N. J.
Hunt, Robt. W .... Troy, N. Y.
Hutton, F. R. *Secretary* .... New York City.
Illingworth, Joseph J .... Utica, N. Y.
Jones, Washington .... Philadelphia, Pa.
Kent, Wm .... New York City.
Kirchhoff, C., Jr .... New York City.
Le Van, W. B .... Philadelphia, Pa.
Lipe, Chas. E .... Syracuse, N. Y.
Mahony, Jas .... New York City.
Maynard, Geo. W .... New York City.
Miller, Alexander .... New York City.
Miller, Lebbeus B .... Elizabeth, N. J.
Moore, Lycurgus B .... New York City.
Morgan, Chas. H .... Worcester, Mass.
Morse, Chas. M .... New York City.
Murray, S. W .... Milton, Pa.
Neftel, Knight .... New York City.
Odell, Wm. H .... Yonkers, N. Y.
Pankhurst, Jno. F .... Cleveland, Ohio.
Parker, Walter E .... Lawrence, Mass.
Partridge, Wm. E .... New York City.
Pickering, Thos. R .... Portland, Conn.
Pitkin, A. J .... Schenectady, N. Y.
Porter, Chas. T .... New York City.
Porter, Geo. A .... Syracuse, N. Y.
Porter, H. F. J .... New York City.
Pusey, Chas. W .... Wilmington, Del.
Randolph, L. S .... Susquehanna, Pa.
Robinson, A. Wells .... Montreal, Canada.
Rowland, Thos. F .... Brooklyn, N. Y.
Rowland, Thos. F., Jr .... Brooklyn, N. Y.
Root, Jno. B .... Brooklyn, N. Y.
Schuhmann, Geo .... Reading, Pa.
Sinclair, Angus .... New York City.
Smith, Albert W .... Oswego, N. Y.
Smith, Chas. D .... Plantsville, Conn.
Smith, Geo. H .... Providence, R. I.
Smith, Oberlin .... Bridgeton, N. J.
Snell, Henry J .... Philadelphia, Pa.
Spies, Albert .... New York City.
Stearns, Albert .... Brooklyn, N. Y.
Stetson, Geo. R .... New Bedford, Mass.
Stirling, Allan .... New York City.
Stratton, E. Platt .... College Point, N. Y.
Sweet, Jno. E, *President* .... Syracuse, N. Y.
Sunstrom, Karl J .... Providence, R. I.

Thurston, R. H .... Hoboken, N. J.
Towne, Henry R .... Stamford, Conn.
Trautwein, Alfred P. .... Brooklyn, N. Y.
Van Winkle, Franklin. .... New York City.
Ward, W. E. .... Portchester, N. Y.
Waterman, Jno. S .... Ithaca, N. Y.
Watts, Geo. W .... Scranton, Pa.
Webb, J. Burkitt. .... Ithaca, N. Y.
Webber, Wm. Oliver .... Lawrence, Mass.
Weeks, Geo. W .... Clinton, Mass.
Weightman, Wm. H .... New York City.
Wellman, S. T. .... Cleveland, Ohio.
West, Thos. D .... Cleveland, Ohio.
Wheeler, F. M .... New York City.
Wheelock, Jerome .... Worcester, Mass.
White, Jos. J .... Philadelphia, Pa.
White, Maunsel .... Bethlehem, Pa.
Wightman, D. A .... Pittsburgh, Pa.
Wiley, Wm. H .... New York City.
Wolff, Alfred R. .... New York City.
Woodbury, C. J. H .... Boston, Mass.
Wright, Jno. Q. .... New York City.

Messrs. Andrews, Brown, Griffiths, Halsey, Jacobi, Lewis, Parsons, Philips, Schell and Sibley were present, with others as guests, at the sessions.

The Secretary read the report from the Council to the Society, as follows:

Report from the Council to the Society.

The Council would report to the Society the steady and normal growth of the membership of the Society. After the last annual meeting of 1883, the number on the register was 440. The ballot lists since that date have added 136 new members (including those becoming members at this meeting), and giving a total membership, deducting losses by death or resignation, of 576. This may be slightly reduced before January 1st, 1885, by failure of a few elected members to qualify within the allotted six months from date of election.

The Council has held three meetings for the transaction of Society business since the last report in May. Besides the routine detail of reports of standing committees and the scrutiny of applications for membership, the following subjects have been presented before it:

The special committee appointed to confer with committees of sister engineering societies as to joint invitation and courtesies to

foreign engineers visiting this country, reported that no conclusion has been reached in conference, and no report was to be made at that time.

The Committee on Revision of Rules reported a recommendation that the rules as proposed by them should be submitted to the voting membership by letter ballot, which was so ordered.

A communication from Mr. J. G. Briggs, member of the Society, was received, in reference to compiling results of tests of fuels for steam purposes. It was directed that his letter be read at the annual meeting.

The matter has been before the Council, of securing legislation for a series of tests of fuel to supplement the standards of Prof. Johnson, in view of the development since his work was prepared of extensive fuel beds for steam purposes. No action has been taken, however.

Communications from the International Inventions Exhibition in London, 1885, and from the American Exhibition Committee in London, 1886, have been received, and it was directed that brief reference be made to both at the annual meeting.

The question of founding the library for the Society, has been presented to the Council and has received favorable discussion. A Library Committee has been appointed, consisting of Messrs. Towne, Copeland, Hoadley, Hutton and Porter, to take the necessary steps for the establishment of such a library, and it has been recommended that this committee report at this meeting.

The Council would further report that under Rule XIII., it has counted the ballots cast for members coming up for election at this time. There were 304 votes cast, and the following gentlemen are declared elected to their respective grades:

HONORARY MEMBERS.

Bramwell, Sir Frederick, F. R. S., Vice-Pres. Inst. C. E., Past Pres. Inst. Mech. Engrs., 5 Great George St. Westminster England.

Bauschinger, Johann, Prof. App. Mech & Dir. of Techn. Laboratory of the Techn. High School, Munich, Germany.

Grashof, F., Prof. App. Mech. & Mach. Constr. at the Polytechnic in Carlsruhe, Germany.

Herrmann, Gustav, Prof. Mechan Technology, at the Technical High School, Aix-la-Chapelle, Prussia.

MEMBERS.

Bailey, Reade W. . . . . Pittsburgh, Pa.
Barnhurst, H. R. . . . . Erie, Pa.
Dixon, Robert M. . . . . East Orange, N. J.

Ewart, William D.	Chicago, Ill.
Francis, Harry C.	Philadelphia, Pa.
Grinnell, Frederick	Providence, R. I.
Hammond, Geo. W.	Boston, Mass.
Humphreys, Alex. C.	New York City.
Ide, Albert L.	Springfield, Ill.
Lee, Alex. Y.	Pittsburgh, Pa.
Pantalaoni, Guido	Pittsburgh, Pa.
Raynal, A. H.	Worcester, Mass.
Sharpe, Joel	Salem, O.
Stiles, Norman C.	Middletown, Conn.
Zell, Robert	Baltimore, Md

ASSOCIATES.

Beaman, Elmer A	Providence, R. I.
Porter, Geo. A	Syracuse, N. Y.
Roche, John A	Chicago, Ill.

JUNIORS.

Cromwell, J. Howard	Cranford, N. J.
Darlington, Frank G.	Pittsburgh, Pa.

PROMOTION TO MEMBERSHIP.

DuVillard, Henry A., Junior A. S. M. E.	Providence, R. I.

Respectfully submitted,

BY THE COUNCIL.

The following report was received from the Treasurer, was read by the Secretary, and on motion was ordered on file:

NEW YORK, *November 5th*, 1884.

To the American Society of Mechanical Engineers:

GENTLEMEN:—I have the honor of submitting the Fourth Annual Report of the finances of the Society, beginning at November 1st, 1883, and ending at this date.

The receipts and payments in cash during the period named have been as follows:

RECEIPTS.

Balance on hand Nov. 1st, 1883	$303.18
Life Membership	150.00
Initiation Fees	2,295.00
Annual Dues	5,057.00
Engraving	27.90
Paper Sales	230.45
Society Badges	229.12
Binding Volumes of Transactions	181.60
Volumes of Transactions	295.00
Total	$8,769.25

PAYMENTS.

Engraving	$659.79
Traveling	101.10
Expenses	1,147.72
Salary	2,515.00
Printing and Stationery	3,007.36
Postage	286.40
Binding	300.85
Total	$8,018.22

Thus leaving a balance in bank and cash on hand, of $751.03.

There is in my hands at this time a bill of J. J. Little & Co. for printing the transactions of the Pittsburgh meeting of the Society in 1884, for $860.38, which has been audited by the Finance Committee, but remains unpaid on account of the want of sufficient funds in the treasury for its payment.

There is due from the membership of the Society, for the following items, the sum of $1,257.80 a very small percentage of which was due previous to the last annual meeting:

Paper Sales and Volumes of Transactions	$222.37
Annual Dues	517.75
Binding Transactions	331.20
Initiation Fees	165.00
Society Badges	21.48
Total	$1,257.80

The financial condition of the Society is very much improved over that of one year since. From an examination of the books of the Society, I am of the opinion that the state of its finances is better by about $800 than it was at the date of my last annual report.

Besides paying the expenses incurred during the last society year, from November 1st, 1883, to the present annual meeting (with the exception of the unpaid bill of printing previously mentioned), there was paid on Jan. 10th, 1884, a bill for printing the transactions of the New York meeting of 1882, and the Cleveland meeting of 1883, amounting to $1,259.80.

Permit me to say that it appears to me, with a prudent and economical administration of the funds of the Society, consistent with its aims and objects, I see no reason why the Society should not be placed financially in a more firm and substantial position

year by year; this opinion, however, is based upon the supposition that the income shall be that due from the membership, or nearly so.

Respectfully submitted,

CHAS. W. COPELAND, *Treasurer.*

The Secretary.—I might perhaps call the attention of the members in more detail to a point which the Treasurer has referred to in his report, and that is, that in this year we have paid for *two* volumes of the Transactions, and have met all the expenses of their distribution, which has never happened before. That is one reason why the balance does not appear even more in our favor than it stands. The bill for Vol. IV. was paid in January, and this bill of $860 is for the last part of Volume V., which bills have both come in the same financial year. Of course, we hope that will never happen again.

The report of the Committee on Library was read by Mr. Towne, the chairman, who presented also letters from officers of foreign societies and extracts from their rules in connection with the report. The report is as follows:

REPORT OF THE COMMITTEE ON LIBRARY.

To the Council of the American Society of Mechanical Engineers:

Your Committee, appointed June 25th, 1884, to take steps for the foundation of a Library for the Society, has held sessions for conference upon the duties allotted to it, and for the consideration of suggestions which have been offered. Your Committee recognizes the difficulties incident to the establishing of a Library for a society which is national in its character, in a country of such vast extent as ours, and in which there are so many metropolitan cities possessing technical libraries available to members in their vicinity, and also the special difficulty in establishing a library which shall be of value to the Society at large, and not merely to those members residing in the vicinity of its location. The experience of a sister institution, however (the American Society of Civil Engineers), indicates that the membership at large of such a society can and will make use of a library, when once established, to an extent which fully justifies its organization. Your Committee believing, therefore, in the wisdom of establishing a library, and in the expediency of inaugurating at once

measures looking to the accomplishment of this, begs leave respectfully to report as follows:

1. That in establishing a permanent fund for Library purposes, and also in providing for the current expenses of the Library, no dependence upon the ordinary income and funds of the Society should be expected or desired, but that, on the contrary, provision for the establishment and maintenance of the Library should be based wholly upon independent and special contributions.

2. That contributions to the Library Fund should be solicited from the entire membership of the Society, in any of the three following forms:

(*a*). Special subscriptions to the Permanent Fund, in amounts of $10 and upwards, payable in installments if preferred.

(*b*). Annual subscriptions of $2, payable at the same time as the annual dues. The amounts thus received to be applied, one-half to current expenses of the Library, and the other half added to the Permanent Fund until the latter has attained a total of $10,000, after which one-fifth of all annual subscriptions to be added to the fund and four-fifths to be applied to current use.

(*c*). Direct contributions of books and papers relating to Mechanical Engineering.

Acknowledgments of all contributions to be made by the publication of an annual report to the members containing a list of donors to the Library, showing opposite each name the contributions made during the year.

3. The contents of the Library to be available for use by the entire membership of the Society. Valuable and rare books, and those of which there is but one copy in the Library, to be available for consultation in the place where kept, but not to be taken therefrom. One or more duplicate copies of standard books to be provided, so far as the funds available may permit, and these duplicate copies to be utilized for circulation among the membership by mail or express.

Rules for the governance of the Library to be made by the Council, and to provide for the circulation of duplicate copies, as above suggested, under such provisions as may be found expedient. The care of the Library for the present to be committed to the clerk or assistant now employed by the Secretary, under direction of the latter.

4. Accommodation for the Library to be provided in whatever

rooms the Society may occupy. In this connection, however, your Committee beg respectfully to call attention to the great desirability, for the advancement of the general interests of the Society, and especially for the adequate accommodation of the Library which it is hoped to create, of inaugurating early measures for the creation of a fund with which to provide a permanent building for the general uses of the Society.

5. That an appeal should be made by proper circular for contributions to the Library Fund from the entire membership, and that to aid in this a brief reference thereto should be incorporated in the annual bills for dues, and a space be provided thereon for the insertion of such amounts as the members may be willing to contribute for this purpose, so that the collection of subscriptions may be facilitated by incorporating them with the other payments to the Society.

6. That a special appeal be made for donations of books, charts, diagrams, and copies of professional reports from members and from publishers, and that the expenses of transportation on all such donations as may be accepted shall be defrayed out of the Library Fund. In this connection it may be stated that the list of exchanges on file in the office of the Society has already been enlarged, and can doubtless be still further advantageously extended whenever proper provision for the accommodation of the Library shall have been made. The present accommodations are already crowded to a point of discomfort, and with the material even now accumulating cannot much longer be made available.

7. Finally, your Committee begs respectfully to advise that the above recommendations, if approved by the Society, should be referred to the Council with direction and authority to give effect thereto in such manner as may to it seem best, and at as early a day as may be practicable.

HENRY R. TOWNE,
J. C. HOADLEY,
CHAS. W. COPELAND, *Committee.*
F. R. HUTTON,
CHAS. T. PORTER,

On motion, the report was accepted and referred to the Council.

The report of the Committee on Test Commission was read by Professor Egleston as follows:

Your committee beg respectfully to report that during the last

year they have been actively engaged in endeavoring to get the bill for the United States Testing Commission passed by Congress. It was introduced in the first week of the session, and was referred to the Committee on Manufactures, which reported favorably upon it. Your committee sent out circulars to a large number of persons who were interested in the matter throughout the United States, urging them to write to their members of Congress asking them to favor the passage of the bill. Almost every letter received was in favor of its passage, and many of the Congressmen wrote to the committee expressing a willingness to work for it when it was once introduced. By some of the political methods of Congress, the bill failed to get a hearing and consequently was not voted upon. Your committee have every reason to believe that could the bill get a hearing it would pass without opposition. Notwithstanding the failures, we do not feel discouraged, and think it very desirable that either the committee should be continued or a new one appointed who, perhaps, know more about the ways of Congress than your present committee does.

T. EGLESTON
C. J. H. WOODBURY,
OBERLIN SMITH.

On motion of Mr. Towne, the report was accepted and the committee continued.

Mr. Kent presented the report of the tellers, as follows:

The tellers appointed to count the ballots for officers, and on the Revision of Rules, would report that they have performed that duty, and having agreed in their count, have to report as follows:

Whole number of votes cast for officers	311
PRESIDENT.	
J. F. Holloway	308
Scattering	2
VICE-PRESIDENTS.	
C. W. Copeland	304
Coleman Sellers	313
H. R. Towne	307
MANAGERS.	
Wm. L. Church	301
Wm. Hewitt	312
Chas. H. Morgan	310
TREASURER.	
Wm. H. Wiley	309
Scattering	1

In the ballots on Revision of the Rules of the Society, the whole number of votes cast was 259.

Ayes	257
No	1
Blank	1

The ballots include the votes of more than two-thirds of the members present, as provided in Art. 43 of the old rules, and therefore the Revised Rules are adopted.

S. ASHTON HAND,
C. E. LIPE,
WM. KENT, } *Tellers.*

At the close of the report of the tellers—

The President.—One thing is apparent, that while they can not tell outside who is elected President of the United States,* we know for a certainty who is president of this Society. Allow me to add, that while I shall feel much pleasure in resigning the chairmanship to my successor, it will not be with anything like the pleasure with which I congratulate him on the honor of the office, which I believe to be the greatest honor that American mechanics can confer upon one of their number. I hope we shall be able to hear a few words from Mr. Holloway.

Mr. Holloway.—There have been so many people running for President this year, male and female, and none of them as yet know whether they are elected or beaten, that I am hardly willing to feel that I am elected. I do not know but that there may be some back district coming in yet which may upset the returns. I have felt that perhaps, under the circumstances, I might be called on to say something, and if you will allow me, I have a little extempore speech here which I will read. I would remind you that, if by any circumstance I should have enough votes not to be counted out, that the trouble will begin, so far as you are concerned, at the next meeting. I do not preside at this one. In speaking of presidents and elections, I trust you will also remember that anything I say at this time is entirely unofficial.

Mr. Holloway then delivered a humorous speech, which was received with laughter and applause.

The Secretary read communications from officers of the London Inventions Exhibition of 1885, and from those of a somewhat similar exhibition to be held in London, 1886, whose prospectuses were to be found at the rooms during the sessions. The com-

* The Presidential election of 1884 had just taken place.

munication from Mr. J. G. Briggs on fuels for steam purposes was also read.

New business being in order, Prof. Egleston presented the matter of appointing a committee of the Society on Uniform Standards in Methods of Testing Materials and of Test Specimens.

Prof. Egleston.—At the Cincinnati meeting of the Mining Engineers, a remarkable paper was read upon testing. As one of the results of that paper, the Institute appointed a committee on uniform methods of testing and test-pieces. At the first meeting of this committee, it was resolved that this work should be made an international one, and I was instructed to see what could be done in Europe. I immediately commenced to correspond with the different engineering societies of the Continent and England, and having occasion to go to Europe, I came personally in contact with the officers of these societies. The Germans, with their usual readiness, took up the matter at once, appointed their committee, and on the 23d, 24 h and 2 th of September, at Munich, they held a session at which very important results were achieved. The English and the French people have assented, unofficially, and I am expecting every day to hear of the appointment of their committees. I have news from nearly all the different countries of Europe, with the exception of Russia to which I have not written, that their committees will be appointed. As the matter is an American one, I shall be very sorry to have this society left out in so important a matter. I beg, therefore, to offer a resolution that this Society appoint a committee to consider this question of test and test-pieces, to form a part of the international committee, all of whose members will probably be appointed in a very short time. And as it will not be convenient that this committee be appointed at once, I wish to add to the resolution that it be appointed by the Council, as may be convenient, within a month or six weeks.

The motion was seconded.

Mr. Kent.—Before any motion is put, I would like to ask Professor Egleston if he would tell us what societies in this country have already appointed committees, and if he can tell us as near as possible the names of the members; also if those committees are representatives of different professions or arts in this country. I think if committees are appointed, they should include representatives from iron and steel manufactories and bridge builders.

Prof. Egleston.—The only committees appointed so far have been appointed by the Institute of Mining Engineers. I communicated at once with all the engineering societies whose official titles I could get in this country, but none of them so far have acted, except that I have an unofficial communication from the Council of this society; and unless some extra effort is made, the only societies that will be represented will be the Mining Engineers and this society, if this society takes action.

Mr. Wolff.—I should like to state that I read a report of the Munich Conference in the *Centralblatt der Bauverwaltung*, October 8th, 1884. There were at the general meeting seventy-three representatives from Germany, and I think some from other countries. As I understood it, this conference determined upon the main essentials, and left it to a committee of their own to decide definitely as to the methods of testing. There was no mention at all of any international action, but the committee, acting on the recommendations of the conference, was itself to decide finally and conclusively on the methods of testing.

Prof. Egleston.—The meeting, as I understood it, was called by the action of the Secretary of the Society of Civil Engineers of Great Britain. I have his letters to me, which were official, and I was also invited as a member to attend the conference at Munich, but I have not had time to read over their discussions. There were six or eight letters passed between us while I was in Europe.

Mr. Kent.—I think there may be some grave objections to having this question settled by an international committee. If a committee should be composed of Americans and Englishmen, I should say it would be all right, because we all use the same materials and have the same measures. But the Germans, and others on the continent of Europe, would wish us to express all our results of tests of material in metric dimensions, which would be a great objection to American and English engineers. I should think, therefore, that any committee which should be appointed would have to be representative of American industries, so as not to be led away by French or German metric system men.

Prof. Egleston.—Mr. Kent's objection is very much like objecting to a good principle because it is expressed in the German or French language.

Mr. Towne.—This matter seems to me of so much importance that I hope the resolution will be adopted; and I beg to differ from Mr. Kent in regard to the danger that we encounter in meeting our German or French friends. It is quite possible that no

2

basis of agreement could be arrived at by which the work done in our country and England could be made comparable with that done in those countries; but certainly it is worth the effort to harmonize the work that is being done in this most important direction, so that it shall be all available to all workers in metals. All of us who have to deal with these questions recognize our great need of better and more accurate knowledge of the behavior of materials, and we can all see that a great many people in a great many different places, under many conditions, are seeking the truth in this direction. If their work could only be reduced to some recognized standard, by which all that is being done in this country should be made available to all of us, certainly our information would grow more rapidly than otherwise; and while, as I said, it may be impossible to harmonize the work done under different standards of measurement, certainly it is worth the effort to accomplish that, and it is my judgment that the thing should be done. I hope the resolution will be adopted.

President Sweet.—My own impression is that to have the tests, from different nations, of special value, it is important that the test pieces should be of the same length. It seems to me unimportant whether we call the test piece 200 millimeters or 8 inches.

The question being put, was carried, the President subsequently to appoint such a committee.

After the conclusion of this business, arose

Mr. Oberlin Smith.—If it is in order, I want to say something about a subject which this Society and all engineers are vitally interested in, and that is, the condition of affairs in the United States Patent Office. I do not know that we as a society have anything to do with it, unless we can influence Congress in some way to pass some one of the bills that have been brought before it for the relief of the Patent Office. It is perfectly obvious to us all that it is necessary to do something if the engineering work of the country is to go on as it ought to. It is a work which is dependent in many cases upon the Patent Office. We have only to look over the history of mechanics and inventions in this century to know how they have been fostered by patents. None of us doubt their value. I do not know whether all the members of this Society know fully how badly things are in Washington in that respect. The fact is now that the business of the office is from three to twelve months behind. In the department of metal-work tools, in which machine tools and such things come, it is about eight months behind. So if any of us now hit upon a new

invention, we know we shall have to wait eight months before it can be reached, and then perhaps the claims may be returned and the thing have to be gone all over again. The Patent Office building is very much crowded with other departments of the government, as you know. Out of a thousand government employees in that building, only about 470 belong to the Patent Office. The other five hundred odd are employed by the Department of the Interior in its other business. It is called the Patent Office, but it is mainly used for other work of the government. The fact is that the employees are not nearly numerous enough. There are probably not more than half as many examiners as there ought to be to do the work rightly. We can remember when a patent could be put through within a month. There is no reason in the nature of the case why it could not be put through in a week or two. The work is very much behind, but it is put on file, and comes on in its order. The special rule they had of taking up some cases out of their proper order gave so much dissatisfaction that the Commissioner ordered cases to be taken strictly in their turn. I have in my hand a speech of Senator Platt, of Connecticut, made last March, before the Senate, which covers the ground very thoroughly. A committee of Congress went to the Patent Office, and were satisfied that it ought to be overhauled; that the other people ought to be cleared out and the building given to the Patent Office alone; that the force ought to be increased and the salaries ought to be increased. There is no question about there being money to do it with, for the Patent Office has over $3,000,000 to its credit in the treasury, which it is not allowed to use in any way. The only reason that the thing has not been put through is because Congress did not choose to attend to it. I have a scheme of my own, but I do not know whether I shall live to carry it out. It is that we have two Congresses in this country—one to attend to politics and one to attend to business. The only other way I know of is to kill all the present Congressmen and put members of the engineering societies in their places. There can undoubtedly be pressure brought to bear on Congressmen this winter at the coming session, if petitions are sent in. Whether this Society can appoint a committee which will make it its business to communicate to Congress the wishes of the Society, or whether that committee had better see that petitions from engineering people and manufacturers all over the country are sent in to Congress, I do

not know. I do not know what is the best form to put it in. I think that a society like this, which is represented all over the country by people engaged in all sorts of work so closely allied to the patent system, can do something, and perhaps without much expense. There is no doubt that there will be some bills introduced, for many Congressmen are interested in the matter, and the Patent Office people themselves are interested. The thing will come up undoubtedly at this session of Congress, but there needs to be some influence brought to bear from outside. I will read a short extract showing how much behind the different classes were last March. In the department of chemistry, eleven weeks; in textiles, seven months; in dairy, fences, tobacco, etc., five months; in hydraulics and pneumatics four and a half months; in harvesters, eight and a half months; in fine arts, etc., four months; in paper manufacture, seven and a half months; in civil engineering, three and a half months; in household furniture, five months; electricity, three months; metal works, three and a half months. But it is worse now than it was then. Now the metal-work department is eight months behind, while last March it was three and a half months only. Besides, there not being half enough examiners, and not half enough room in the building, the examiners do not receive the pay they ought to. I believe they receive $2,500 a year, which is a ridiculous salary for men of the accomplishments necessary for the place. An examiner ought to be a man of the best ability, and I should think he ought to have at least double that salary. I would move, therefore, Mr. President, that this Society appoint a committee to consider whether anything can be done in urging forward the reform of the Patent Office.

Prof. Hutton.—In rising to second that motion, I would suggest that the Society's office is a sort of head-quarters for the interchange of the sentiments of members, and it has had running through it a perpetual stream of complaint about the condition of the Patent Office. Particularly is this the case in the matter of electrical inventions. It has been urged upon me by several gentlemen, who said they would not be able to be present at the meeting, that, should this question come up, I should try to say what I could toward bringing about some action for remedying this state of affairs at Washington.

Mr. Edson.—It appears from the temper of the last Congress that we had better first look sharply to the *continued maintenance*

of the Patent Office. It required considerable effort here very recently for us to meet the encroachments that were being made on our patent laws just as they now are; and it does not seem to be exactly the time for us to complain of the Patent Office. The result might be to make it considerably worse. There is a strong tendency outside, in the lobby, which controls Congress to a very large extent sometimes, to change the patent laws. We must not lose an opportunity of doing what we can to sustain the patent laws in their *present* form, and then to bring them into as much better form and practice as we can. The question is how to give the inventor the quickest protection to which he is entitled; and it would seem that the committee would be unwise to have that the sole idea in their minds of remedying evils. We may have to admit that there are evils; but nevertheless it is the fact that we have plenty of money at our disposal to remedy those evils of delay in the way of increased force, increased salaries, increased room, and increased library facilities, too. But we want *first* to take care that we hold on to all that we have. There is one other point which, perhaps, will have to be looked into in the consideration of the subject which the gentleman has just referred to: that is possibly the *personnel* of the examiners. In doubling, as has been suggested, the compensation of these examiners, we should like to double their capacity. If we can say anything about their integrity, we should like to quadruple that, perhaps; and then we would like to have the impression created that they are there and will remain there. We do not want them to be there a short time and then go out and issue cards as patent solicitors, in which business they can make a much larger income than they can as examiners in the Patent Office. First look to the maintenance of what we have; then better the state of affairs if possible. Otherwise, " bear the ills we have, rather than to fly to those we know not of."

Mr. Towne.—I appreciate the importance of the subject that has been introduced; but I think it is rather too vast a one to attempt to get through with this morning. I would like to ask, what is the form of the resolution that has been offered?

Mr. Oberlin Smith.—Merely the appointment of a committee by this Society, to consider whether we can do anything to forward the proposed reform in the Patent Office. Perhaps it will consist only in sending a memorial to Congress, but it might consist in getting petitions from mechanical people all over the country.

Mr. Towne.—I quite agree with Mr. Smith in the propriety of the Society taking some action on this important subject. The need of reform in the Patent Office is crying. The fact that the Patent Office is threatened is no reason why we should not act on the other side. The flagrancy of the present condition of things there can be summed up in just this one brief statement: The Patent Office was organized on the basis of a recognition of the rights of ownership in the work of a man's brain, and on the supposition that, by giving him the exclusive right to use that work for a limited length of time, the proper compensation would be assured to him and justice be done to the public. The fees which were exacted from inventors were not regarded as a tribute of any kind, nor as a payment for the privilege conferred, but simply as a means of meeting the necessary expenses of the office. Those fees have been paid on that supposition by inventors, and \$3,000,000 have already been taken from them in that way in excess of what has been given in return in the shape of work. That \$3,000,000 has been diverted from its proper purpose and placed in the miscellaneous funds of the government. It does not belong there. It belongs to the inventors of the country, and should be restored to them by being used to increase the efficiency the Patent Office.

In seconding Mr. Smith's motion I would offer this as an amendment: That this subject be referred to the Council, with an expression of opinion from the Society, at this meeting, that it is expedient and proper that the Society should take some action of this kind, and should in some official manner convey to Congress an expression of the opinion of the Society that a reform of this kind is needed in the Patent Office, whereby its work may be accelerated, so that the delay now suffered by inventors shall be reduced or done away with; and that in referring this to the Council, it be accompanied by the request that it may have early consideration, and that whatever action the council takes be transmitted to Congress early in the session.

Mr. Oberlin Smith.—I accept the amendment of Mr. Towne provided the Council will attend to it before December, when Congress meets. I would just say in regard to what Mr. Edson says, that I do not agree with him at all about not stirring up this matter, because Congress threatened last winter to abolish the Patent Office altogether. I think they were partly led to that feeling because the office was such a puny, shabby thing. If

these petitions come in from the Societies and the country, showing the interest that is taken in it, it will show how important an institution it is considered to be.

Mr. Partridge.—I want to make a suggestion to the committee—to whomsoever has the matter in hand—that if we are going to stir this pool, as my friend on my left suggests, we had better use a very short stick. I have had reason, in the past, to go below the surface, and it was certainly, to say the least, unsavory. The delays and the inconveniences are the smallest portion of the fault which, as inventors, this Society would have to find with the Patent Office.

Mr. Edson.—I wish to offer an explanation in regard to what I touched upon. I did not wish to belittle the force of the resolution as proposed. I suggested as an amendment that we embody in the resolution the idea of protecting, and that the committee be instructed to protect what we have, as well as to remedy existing evils in the Patent Office as far as we can.

Mr. Oberlin Smith.—I accept that amendment. It certainly is appropriate that this Society, or its committee, should urge upon Congress the *maintenance* of the patent system as well as its *reformation.* There might be something put in the memorial in regard to last winter's work deprecating any future action of that kind.

Mr. Fitch.—Will not the action proposed be very ineffectual? The average Congressman is influenced by motives which affect the people, especially the lower classes, who have a very small appreciation of the needs of inventors; and the consequence is that this class is strongly inclined to encroach upon property rights, such as are represented in the value of patents. Now I think that, of course, all measures ought to be taken for the immediate remedying of the trouble in the Patent Office; but it seems to me that the best measure for this Society to adopt is to do what we can to insure the establishment of civil service reform, and then we can bring its influence to bear upon men who can appreciate that influence, and place examiners in a position to examine the examiners of the Patent Office.

Mr. Oberlin Smith.—I attempted to bring this same matter before the American Institute of Mining Engineers at their recent meeting in Philadelphia, but found that their constitution forbade any action of the kind; that their only function is to read their papers and print their transactions. I have not been at one of the

meetings of the American Society of Civil Engineers recently, and I do not know whether they would act in conjunction with us or not. I had intended offering a resolution at the last meeting, had I been present. It would be very desirable if some of our members here who can attend the meetings of the Civil Engineers, would bring the thing up and see if they would act.

Mr. Le Van. — I might suggest that if we are going to amend the Patent Office, we ought also to amend the Supreme Court.

The President.—My own opinion is that this resolution would put it off pretty late. There is not likely to be another meeting of the Council until nearly December, with the exception of a short meeting at the adjournment of the present Council and the organization of the new.

Mr. Le Van—Why not make a special committee?

The President.—If it is thought by the members that this matter ought to receive some attention, it would seem to me that if this Society could furnish the Commissioner of Patents with our opinion with regard to it, that might be as likely to do as much good as the attempt to deal with Congressmen, with whom all of us are pretty well disgusted, I suppose.

Mr. Kent.—I suggest that this question be left over till to-morrow, and meanwhile that this committee draw a resolution, and have that transmitted to Congress; and also that a special committee be appointed, of those members who are most deeply interested in the question, and, in addition to memorializing Congress, let them send a circular to their respective Representatives in Congress.

Prof. Egleston.—What is needed in this matter is to increase the average intelligence of the ordinary Congressmen.

Mr. Towne.—In view of the statement of the President that there will be no meeting of the Council until December, I beg to withdraw my amendment to Mr. Smith's motion, and to let it stand as originally offered, that a committee be appointed by the Society.

Mr. Davis.—It seems to me that Congressmen are most influenced by what will be acceptable to a large number of their constituents. I should think it would have probably more effect if the matter was put in a sort of popular form and circulars sent round, subscribed to by a large number of persons, and in that way lead Congressmen to suppose that their constituents were interested in

the matter; while they would not care at all for the good-will of a few scientific men.

Prof. Egleston.—About four years ago I was chairman of a committee to have something done by Congress in the interest of the Metrological Society. We were extremely anxious to have some measures carried out, and with a great deal of trouble I got a petition with four thousand signatures, embracing the faculties of nearly all the colleges and technical institutions of the United States. In this instance I did not keep a copy, and I expressed the hope to the chairman of the committee that, as this document was extremely valuable as autographic matter, if in no other way, that it should be preserved. They paid no attention to the matter at all, and I afterward discovered that it went into the waste-paper basket. The next time I sent a copy of the signatures, and kept the autographs myself.

The motion was carried, that the committee be appointed by the chair, and at a later session it was announced as consisting of Messrs. Towne, Egleston, Edson, Babcock, and Oberlin Smith.

There being no other new business presented, Mr. Kent, chairman of the committee of the Society, to prepare a report embodying standard rules for conducting boiler tests, read by abstract the report, the code of rules and the appendices of the members of the committee. The report being so elaborate, it had been decided not to attempt to discuss it, but to send a copy to every member before the next meeting, and to have it presented for discussion as the first business of the spring meeting.

The first paper on the docket was then read: "The Experimental Steel Works at Wyandotte, Mich.," by Mr. W. F. Durfee, of Bridgeport, Conn. This paper and that by Mr. R. W. Hunt, of Troy, N. Y., on "The Original Bessemer Steel Plant at Troy," were discussed together by Messrs. Holloway, Stirling and Barnes.

After some announcements by the Secretary as to details of the meeting, a recess was taken till two o'clock.

Afternoon Session.

2 P.M.

The Society being called to order, the paper by Mr. Wm. Hewitt of Trenton, N. J., was read by the Secretary in the absence of the author, and Messrs. Davis, Sweet and Hutton took part in the discussion.

Mr. A. C. Hobbs, of Bridgeport, Conn., illustrated his discussion of Locks and their Failings by elaborate wall diagrams in colors, and Messrs. Towne, Emery and Oberlin Smith followed in discussion.

The paper by Mr. F. A. Scheffler, of Erie, Pa., on "A New Method of Constructing a Horizontal Tubular Boiler," was read in abstract by the Secretary and was discussed by Messrs. Stirling, Collins, C. E. Emery, Stratton, Kent, Webb, Wellman, Partridge, Davis, Oberlin Smith, Porter, Barnes, LeVan and Sweet.

The next paper was by Mr. Thos. D. West, of Cleveland, Ohio, on "Sound Castings." It was illustrated by two cast balls which had been drilled and broken to show the fracture, and Messrs. Barnes, Randolph, Stetson, Stratton, Durfee, Barr, Webber and H. F. J. Porter took part in the discussion. Mr. Stratton referred to the observations of the recent gun-foundry board, as reported in the Proceedings of the United States Naval Institute, and urged upon the members to interest themselves in the work of the Institute.

At the close of the session opportunity was given to Mr. Cope Whitehouse to present, briefly, his views upon the topography of the plain in which the Egyptian pyramids are found, as affecting the theories of their construction. His remarks were illustrated by maps and sketches.

After announcement by the Secretary of details of the excursion planned on the following day, the meeting adjourned to eight o'clock on the following evening.

Friday Evening.

The concluding session was called to order at 8 P. M.

Mr. F. A. Halsey, of New York, presented a paper on "A New Rock Drill," which elicited no discussion.

A paper by Mr. C. J. H. Woodbury, of Boston, Mass., entitled "Measurements of Friction," and one by Prof. R. H. Thurston, of Hoboken, N. J., on "The Sliding Friction of Rotation," were read by abstract and discussed together, by Messrs. Thurston, Towne, Schuhmann, Holloway, Kent and Emery; and Dr. Arvine, chemist of the Standard Oil Company, who was present, took part also by invitation. At the close of the discussion, Mr. E. F. C. Davis, of Pottsville, Pa., exhibited by request a number of photographic lantern-slides of mine interiors, taken by Mr. Geo. M. Bretz of Potts-

ville, with the electric light. The views showed the exterior of the Kohinoor colliery, the arrangement of the $9'' \times 12''$ engine and the five-light Arnoux dynamo in the gangway and various aspects of a large coal chamber in the Mammoth Vein of the Indian Ridge colliery. The time of exposure was thirty minutes, with $\frac{7}{8}''$ diaphragm for a negative $8'' \times 10,''$ in some cases, and in others ninety minutes. These views were taken originally to supplement a collection of exhibits gathered for the Smithsonian Institute to present at the New Orleans Exposition.

At the close of Mr. Davis' remarks, Mr. Towne, on behalf of the Committee on Memorial in reference to the United States Patent Office, presented the following report:

Resolved, That in furtherance of the views of the Society on the important subject of protection by patents, as expressed in the discussion thereon during the present session, the President be and is hereby requested to embody said views in a memorial to Congress, to be prepared by him and such other members, if any, as he may request to co-operate with him therein, which memorial shall be duly forwarded to Congress on or before the date of its assembling next month; that said memorial shall recite briefly the importance of the interests involved, the fact that the patent system is based upon a recognition of the rights of the inventor to the fruits of his work; that the fees of the Patent Office were intended merely to cover the cost of conducting the same; that already some $3,000,000 have been collected from inventors in excess of such cost, and that by reason of the diversion of this large sum from its legitimate purpose the work of the Patent Office, by reason of the resulting inadequacy of its clerical force, is and has long been greatly in arrears, and, finally, as an inevitable consequence, inventors are subjected to unreasonable, and in many cases disastrous, delays in obtaining the protection to which they are justly entitled, and for which they are charged; and that said memorial shall be so presented as, if possible, to secure for it favorable and early consideration.

Further, that, to the same end, the President shall cause to be prepared a suitable form of petition, embodying substantially the same arguments, and shall cause a copy thereof to be forwarded to every member of the Society with the request that, if approved, each member will sign the same, and will procure other signatures thereto, and will thereupon promptly transmit the same to his Member of Congress or Senator, with his personal request that the measures advocated in the memorial may have his support and advocacy.

HENRY R. TOWNE,
JARVIS B. EDSON, } Committee.
OBERLIN SMITH,

The resolution proposed was duly seconded and adopted.

Mr. Theodore Bergner, of Philadelphia, Pa., read a paper on Recent Improvements in Drawing Boards, illustrating his remarks by exhibiting the capacities of a board upon the platform. He prefaced his remarks as follows:

"A great and distinguished man of the last century, Gaspar Lavater, who died in Zurich on the 2d of January, 1801, is now almost forgotten. Some only remember him as a theorist who judged the weight of a man's brain by the length of his nose. He was an acute observer, and many, perhaps, who have forgotten the man, are familiar with sentences of pregnant and deep meaning recorded by him with epigrammatic brevity.

"He left one saying about three sorts of men—those who only see the whole, those who see but the parts, and that numerically small class, who see and comprehend the whole in its parts and the parts in the whole.

"Convinced that you, gentlemen, here assembled belong in a body to that third class, the quick observers, I greatly fear that my attempt at a treatise on so simple a subject as the drawing board is apt to be estimated, will weary you.

"However, having committed myself to the attempt, I will try to contribute something useful to your information."

Messrs. Oberlin Smith and Towne also spoke.

Owing to the lateness of the hour, the paper by Prof. Thurston, of Hoboken, N. J., on Steam Boilers as Magazines of Explosive Energy, the two tabular papers by Mr. Wm. Kent, of New York, on Tables for Facilitating Calculations in Boiler Tests, and Table for Calculating Sizes of Chimneys, together with the concluding paper in the series by Prof. J. M. Ordway, of Boston, Mass., on Experiments on Non-Conducting Coverings for Steam Pipes, were read by title only. After the list of papers had been completed, arose

Mr. C. E. Emery.—I have a resolution to present.

Resolved, That the thanks of this Society be tendered to its outgoing Treasurer, Mr. Charles W. Copeland, for the faithful and efficient manner in which he has performed the duties of his office, and the care which he has exercised to place the Society on a sound financial basis.

The President.—You hear the resolution; and I think if you all had had occasion to realize the importance of Mr. Copeland's services as some of us have, you would pass the resolution with a good round of applause.

The resolution was carried, with acclamation.

Mr. Partridge.—I have a series of resolutions to offer—

Resolved, That the American Society of Mechanical Engineers, thoroughly appreciating the efforts made in its behalf with a view of making the occasion of

the Fifth Annual Convention pleasant and profitable to its members, hereby tenders its hearty thanks,—

To Mr. Sam. Sloan, Pres., Andrew Reasoner, Supt., and F. S. Griffiths, Asst. Supt. D. L. & W. R. R., for the favor of a special train to and from Paterson.

To Mr. John S. Cooke, the Cooke and the Rogers Locomotive Works, the Passaic Rolling Mill, and the various silk mills of Paterson, for attention when in that city.

To the New York Academy of Medicine, for its courtesy in allowing this Society the use of the comfortable and admirably arranged rooms in which our meetings have been held.

To Chas. Wager Hull, Esq., Superintendent of the American Institute Fair, to whose thoughtful kindness the Society was indebted for an invitation to visit the fair now in progress.

To the Faculty of the Stevens Institute of Technology for their visit to that institution, and the courtesies extended during that visit.

Last, but by no means least, we tender our thanks to the Local Committee for the manner in which the details of the meetings and excursions have been arranged and carried out, which have contributed so much to the pleasure and profit of our meeting.

These resolutions were put and carried unanimously.

The Secretary made brief allusion of thanks to the authors of papers for this meeting, by whose co-operation it had been possible to have all but two of the fourteen papers in print in advance for circulation among the members at the meeting, with all the advantages which flow from this plan.

Prof. Thurston.—I have a resolution here, Mr. President, which I will read—

Resolved, That the thanks of the American Society of Mechanical Engineers are due and are hereby tendered to our retiring President, John E. Sweet, for the efficient and satisfactory manner in which the duties of his office have been performed during his term of office.

The President will refuse to put that I know, for if there is any one personal excellence which is offensively prominent in his case, it is his modesty, and I will venture to put the question for him.

*Mr. Holloway.**—It is so good an opportunity for me to do something in the way of presiding, that I do not want to have Professor Thurston take it away from me. The Society may permit me to anticipate the functions of my office, and I trust Prof. Thurston will, in putting this motion. I am very glad indeed that the first official act of mine will be one in which you will heartily concur. Those in favor of the motion will say aye.

The motion was carried, with enthusiasm.

* President-elect.

The President.—In accepting with thanks, I can but say that if I have succeeded in satisfying you in any way, only part of the credit is due to me. It is mostly due to the able assistance which I have been able to command.

The meeting then adjourned.

EXCURSION DAYS.

THURSDAY EVENING, NOV. 6TH.

FRIDAY, NOV. 7TH.

THURSDAY evening was spent by a large number of the members in attendance at the fair of the American Institute, in session in the city. Passes had been supplied by the courtesy of the Superintendent of the fair, and were largely used.

Friday was spent in a visit to Paterson, N. J. A special train, on the Delaware, Lackawanna and Western R. R., had been tendered by the courtesy of Mr. Samuel Sloan, President of the road, and by the politeness of Mr. John S. Cooke, of the Cooke Locomotive and Machine Works, the privilege of visiting several points of interest was accorded to the Society.

The Passaic Rolling Mill, the Rogers and Cooke Locomotive Works and the silk industries, were all visited by the party, and after lunch had been served at the train, the party returned to Hoboken and visited the Stevens Institute of Technology, whose laboratories and workshops had been thrown open for the entertainment of its guests, by the courtesy of the trustees and faculty of the Institute. The day was much enjoyed by all.

CLIII.

PRESIDENT'S ADDRESS, 1884.

BY JOHN E. SWEET, SYRACUSE, N. Y.

It is generally thought by mechanics as well as by others that an essay or paper or lecture on purely mechanical subjects cannot be made of interest to a general audience. While I do not propose to prove by argument or to demonstrate by display that this is not true, nevertheless I do not believe it to be true.

Is it probable that Wendell Phillips, who could hold his audience spell-bound an hour and a half reciting scraps of history relating to the lost arts, could not have found enough of general interest in the world of live mechanical wonders to entertain people one brief half-hour? Is it possible that those who spend days and weeks at our exhibitions reviewing the wonders, ninety per cent. of which are purely the result of machine work and mechanical processes, cannot be interested in a vivid description of the inventors and their inventions, the machines, the processes, and the skill of their operators? If so, then the fault lies with them, or the failure is with the speaker, and I believe the fault is not with the people.

Ruskin, speaking to his Edinburgh audience on the subject of architecture, said: "You can and ought to be interested in the subject of architecture. You all have to live in and use buildings and have more or less to do with their construction." While Ruskin was looking at the matter, usually in an artistic sense, he never overlooked nor failed to enforce the principle and the importance of sound mechanical instruction.

The reason why the citizens of Edinburgh should be interested in architecture holds good for all of us, and the same reasons hold good why all ought to be interested in mechanics.

Can any one have an interest in mankind and civilization and overlook a class comprising one-fourth of the entire population of the United States and over one-third of Great Britain? Or can we go on advancing civilization without giving a thought to the agencies that have contributed so much toward making modern civilization possible? Modern science has mainly or almost wholly

grown up within the last century. Science began to flourish when it could show its use, and it has flourished in proportion to its usefulness. No one questions a scientist at the present day as to the use of this or that discovery or fact but finds him ready with his useful answer.

It has been the proving of the value of science that has made it possible for purely scientific lectures to command as fair audiences as those on any other subject. Now, blot out of science all that is mechanical, and what would remain? Nothing but mathematics and medicine and fragments of undemonstrated theory. The success of modern chemistry turns upon the perfect balance. It may be argued that the chemist often makes his own balance. True, but when he does he ceases to be a chemist, and becomes a mechanic.

The modern physical laboratory is a perfect museum of mechanical wonders. Though the philosopher believes and may prove mathematically that certain things are and must be true, he does not ask the student to believe it until he has given visible demonstrations, and this is done very largely through the agency of mechanical contrivances. It is true he makes these things or the most of them himself, but they are none the less mechanical because of their being made by a philosopher.

While mathematics and medicine are exceptions, surgery is half mechanical, and astronomy is built up and turns upon the perfection of the astronomical instruments; and the perfection of the instruments comes by the painstaking, sharp-sighted, delicate touch of the patient workman. It is not probable that many men in Asaph Hall's place would have discovered the moons of Mars—still, possibly, a dozen would—while half the number put in Alvan Clark's place could not have made the instrument by which they were discovered. And yet Asaph Hall has received medals of honor from learned societies, while Alvan Clark will not have his brow decked with a laurel wreath long enough before his death to have the laurels wilt before he fades himself. Science has flourished because of the good it has done: it has been popularized by showing its good to the world; while mechanics has never been popularized, and for its importance in the civilization of the world it has had few champions. It may not be fitting for us to draw comparisons between science and mechanics, but let us erect a balance—not a mechanical structure, but a purely ideal one. Place in one pan all the science and scientists

of all time, and in the other pan, not all of mechanics and mechanical achievements but simply the steam-engine, the locomotive, and the steamboat. Then say to the world, of these things you can have one but not the other. Which would it take? Who can tell?—not you, not I.

If it is presuming to draw comparisons between science and mechanics, what will it be to make reference to history and literature and art in the same connection? This I will not do, but I will venture to invite you to picture to yourselves a pendulum-rod of immense length, carrying at its lower end a cross-beam proportionately long and forming a great inverted T. At each end of the beam conceive a mammoth platform, and at the center an index and its graduated scale. All are infinitely strong, and infinitely light, and almost infinite in size. The pendulum is hung high in the heavens, and is set back three or four hundred years in time, and so far away in the background that you can take in the whole picture at a single glance. Now fancy placed upon one of these platforms all the ancient history and literature and art; and all the historians and scholars and artists; all the facts and fables, truths and falsehoods—superstitions, dogmas and creeds—all of these that existed three or four hundred years ago. The ponderousness of the mass would swing our great pendulum far out from the perpendicular. So far, in fact, that the center of our great intellectual weight should appear to be under the point of support.

Look at the index; it reads ninety-eight, ninety-nine and a half. Why not one hundred? Is it because of anything on the other platform? It has been thrown so high that we can hardly see it—but look again. There are two men working a rude screw-press machine. They are making what look like snow-flakes, one, two, three, four a minute. Some of these snow-flakes drift down among the wise men on the other platform, and a wave of wonder ruffles the surface of the throng. The men and manuscripts on the edge of the great platform go tumbling off. After a moment note again the index which has gone down to ninety. Why so great a change? Look again at the upper platform, there are now dozens of machines and scores of men turning out forty snow-flakes each instead of four. The machines do not seem to be making the snow-flakes, the men are making them, and the machines are putting on some color.

They no longer come floating down, but must be brought down

in bulk as burdens are borne in the arms of men and on the backs of beasts. They are gray instead of white, and are red as they fall into the hands of the people. Tumult follows wonder. Men and superstitions and manuscripts and works of art go tumbling off. When we note the index again it is down to seventy-five, and on the upper platform, that is now in plain sight, we see large factories making the snowflakes not in little pieces but in long continuous bands. The coloring machines are no longer worked by hand running back and forth, but go whizzing round and round, propelled by steam, while the snowflakes go streaming off like the snow banners from the crests of the Sierras.

As the snowflakes of the mountains drift down over the sandy plains, so those from the machines drift out to the people. As there is a flake for every grain of sand, so there is a paper for every person. As the damp snow soaks and swells the ground, so do the papers as they are absorbed expand the ideas and acts of the people, bringing life out of stagnation, turning thought into action. As the heat of the glistening sun melts and changes the moisture of the snowflakes into steam, every atom becoming self-repellent, seeking more room and swelling a thousand times, so too, the inert man aroused to action becomes self-repellent and seeks more room. The active crowd off the inactive. Scores of men, creeds, false ideas and dogmas, unsound philosophy, bad laws and tyrants all go tumbling off.

And as we look again the index has gone down to thirty. A net-work of wire is hung over the heads of the people. Their thoughts and their acts are gathered and conveyed across the feeble strand to the machine on the other side, and they are then stamped upon the next group of outgoing snowflakes. The thoughts of the few are known to the many, and the acts of the many are known to all. New life has been given to old industries, and new industries have been created. Our printing machines no longer deal in single flakes but take the paper from the mills in rolls of a ton's weight and send it through in single and double streams. Note the index; it is down to zero.

We have upon one platform what there is left of Ancient History and Literature and Art—the body and the soul of the past. Upon the other platform stand the paper-mill, the printing press and the telegraph—the life-blood of to-day. Let us set our picture in the dim distance where time may cast as brilliant a halo around the heads of those who made the one as that which now

shines around the heads of those who study and love the other; and may the time soon come when we will have a seer among us who can sift the mechanism out of this and hold it up to view before the picture fades away.

James Freeman Clark in one of his papers uses as an argument against the Darwinian theory, the fact that man is the only tool-making animal. A fair investigation of the subject will convince most men that the world is indebted to the tool-makers for more than a mere furnishing of an argument against the Darwinian theory. If the world—the literary world at least—were called upon to select the greatest man of modern times, Shakespeare would no doubt have a fair majority. If all the works of Shakespeare were blotted out of existence, the world would meet with an irreparable loss, or had he never lived, a great fountain of knowledge and amusement would have been left dry. Still the world would have gone on and civilization and religion and industries would have advanced about the same as they have advanced.

But suppose the hammer and the other tools working upon the same principle had never been invented, or were to-day to be blotted out of existence, what would be the result? The thought, if carried along for a single ten minutes, becomes appalling. A time as short as one-tenth of the length of our world's history would bring us back to living in mud hovels with nothing to eat but roots and herbs and nuts. Thus we have on the one hand the greatest of men, whom the world could have gotten along without, and on the other the simplest of tools, without which the world would be a desolate waste. The greatest of men and the least of tools may seem an absurd comparison, surely, though we may not all agree upon the direction from which the absurdity approaches.

I think I know the temper of an audience the most of whom have spent their lives in study, and who have had it taught to them and who in turn teach others the idea that there is nothing like education. The thought has long since come into your minds that he is comparing work with thought—hand-work with brain-work, and material with ideal, and does not comprehend the difference. Let us see. What is a book but the embodiment of thought or a train of thoughts? And what is a loom but the embodiment of a thought or a train of thoughts? The construction of the loom by the pattern-makers and the founder and the forger and the fitter, is what many choose to term mechanical. So, too, is the writing and the printing and the

binding of the book mechanical. The cost of the machine, like the cost of the book, depends upon the material and the labor put upon it. The merit of the machine, like the merit of the book, turns upon the genius of its author. The value of the machine, as well as the value of the book, should be gauged by its influence for good and the amount it contributes toward the comfort and happiness of mankind. If this is the true measure of value, then what is there in the world of science, and the wonders of art, or the wisdom of letters that can more than stand side by side with any one of a dozen of our leading inventions?

The educated portion of the world look upon a book not merely as so much paper and printing and binding, but as the thoughtful work of the author, while the same class almost universally look upon a machine as so much wood and iron, running their minds forward to what it does, and how much it will save, and what the patent is worth, rather than backward to the brain-work of its author.

How few or how many have the remotest conception of the amount of thought expended on the steam engine alone? More than one hundred years has this thinking been going on among from fifty to one hundred of those noble hard-fisted thinkers in the days of James Watt down to the not less than ten thousand hard- and soft-fisted thinkers of to-day. And the thought expended upon the mechanism of the printing industry has not been much less than that given to the steam engine. In the almost fruitless attempt to invent a type-setting machine, the brain-work invested, which has found a tomb in the scrap-heap, would make quite a hill by the side of that spent on the mountain of books which have found a sepulchre in oblivion.

To many of you the idea may have occurred that there is a deal of difference in the kind of thought or brain-work done by the mechanic and that done by the literary man or author—the kind of difference that exists between the mechanical and the artistic. In many respects this is the reverse of true. Among all designers of industrial art, the designers of machinery must of necessity be the greatest artists, because they are dealing with a thing of life, and they are limited by the most severe restrictions. Painters, modelers and authors are limited only by their own genius. The architect is more limited, but he can add a panel, or a niche, or a false window here and there to balance his design. But the best mechanical engineer denies himself the right to add the least thing

not essential to the necessities of the machine ; nor will he even admit embellishment in color, and notwithstanding these restrictions some of the recent designs of machinery by our best designers are as perfect works of art as anything done in industrial art in the country at the present time. Devoid of poetic sentiment, it may be true, but it is not art with the poetry left out. It is art with the poetry suppressed. Every good designer is an artist in spirit and one of the most successful machine-designers in this country is one of the best art connoisseurs, and possesses, I believe, a finer collection of art-work than any other private citizen residing in the same city.

In the final record of mechanical work there is no fear of any slight to the man who spirals the railway up the side of a mountain and pierces the hardest of rock a dozen of miles and tunnels down on the other side like a circular stairway. There is no fear of slight to the name of those who span the Hudson with the mightiest bridge yet completed, or have in hand the great work which is to step from land to rock and rock to land by two 1,700 feet arches. There will be no lack of due honor to the leading men who shorten the time from land to land and city to city ; who enable us to communicate across the continent in two hours ; who change the darkness into light and enable us to converse fifty miles away as well as if but fifty feet.

There is no doubt but fair credit will be given to all our steam, naval, and metallurgical engineers ; but there are two or more classes of our inventors who are likely never to meet with their just rewards. One, a very small class whose inventions fail from causes that the wisest of men could not foresee, as an example that recently came to my knowledge will best illustrate. All of us have realized the loss and waste of power in stopping and starting street cars. The most of us have heard of some man getting up a device by which the lost energy in stopping was to be stored and given out in starting, and the device was about to be tried, and that was the end of it. No one conversant with the mechanical achievements of our people can doubt the possibility of solving the mechanical part of the problem, and the conclusion is probably that the task has been too much for the man or the gain did not pay the cost.

Both these conclusions are wide of the mark. The thing has been done—the contrivance simple, the weight not excessive, and the operation as simple as operating the common brake—in fact

a perfect mechanical success, and yet condemned and thrown aside in less than a week—for the simple reason that the better it worked the more rapidly it made horses balky. The horses in this case seem to exhibit the same degree of intelligence as shown by the railroad brakemen who rebel against the introduction of safety appliances to car-couplers, on the plea that "any one can couple cars with them things." Such cases of unfruitful inventions, although not numerous, are not uncommon, and in this case I hope for the inventor finding his reward. The same wasted energy goes on in the cable and electric railways, the same device is applicable, and unless the different systems form themselves into an amalgamated association and the lightning goes off on a strike, the invention may yet find its field of usefulness.

A second class of inventors, and a very large class, who are not likely ever to receive due credit for the real good they have done, includes the inventors of agricultural machinery. Their work alone has doubled the comforts of the civilized world. Others have helped classes; they have helped all. With them difficulties have been overcome by surviving defeat, and the victory has been won in every case by facing the most adverse circumstances. A knowledge of the laws of friction gave but little aid to the man who found out how to keep the knives of a mowing machine from clogging up. And a knowledge of the strength of material failed to give security when the test strain was the pulling power of a runaway team arrested by the roots of a tree.

Gauging the value of the thing on the democratic principle of the greatest good to the greatest number, the inventors of agricultural machinery will have few rivals. This statement may be questioned, but in my judgment it will be only by those who have as little knowledge of the difficulties of the problems and the magnitude of the work, or of the simplicity and extent of the results, as they imagine the rural engineer to possess of the more pretentious works of the men who claim to be their superiors.

There is another class of men who are never likely to have their merits justly appreciated. I refer to the great army of lieutenants —the chief draughtsman of the professional engineer—the secretary of the society, the professor's associate, the manufacturer's superintendent, the second engineer in the navy, the iron-master's assistant, the inventor's model-maker, the machine-shop foreman the man we always turn to when we get in close quarters, the man who helps us out when we cannot find the way out our-

selves. How readily the mind of the superior absorbs the thoughts and hints of his assistant, and imagines them to have been his own!

What I have said thus far has been by way of reference to some interesting features in the direction of the value of thought, and now I wish to say a final word in regard to the execution and handicraft without which the most ingenious and valuable invention and the most persevering of mechanical thought would be valueless. Do we not pay thousands every year to see and admire those whose life has been spent in acquiring skill in a single direction, while we pass the skillful workman, whose works are equally marvelous, with but a passing thought? May the time come when we shall have a museum in which there shall be gathered the finest specimens of workmanship with the masterpieces of our great engineers; where the works of the men and the growth of industries shall be represented. May the time come when more of the mechanical branches of our educational institutes shall find their true position, and where the students shall be instructed by example of noble work rather than by the toy models abounding in confusing complication, which they cannot understand and which are constructed regardless of proportion and meaningless in design, and are pernicious in every sense of the term.

Let us hope that if the high tide of human progress is sweeping on toward a more useful education, that the day may not be far away when he who knows what to do and how to do it, will be regarded as the equal of him who only knows what has been done and who did it.

CLIV.

AN ACCOUNT OF THE EXPERIMENTAL STEEL WORKS AT WYANDOTTE, MICHIGAN.

BY W. F. DURFEE, BRIDGEPORT, CONN.

IN the month of June, 1862, the writer of this paper was invited by the late Captain E. B. Ward, of Detroit, to design and superintend the construction and working of the machinery and apparatus necessary to test, by an experiment on a large scale, the merits of a process for the production of steel invented by William Kelley, of Eddyville, Kentucky.

At the time of which I speak the late Z. S. Durfee, who was associated with Captain Ward in the new enterprise, had been for some months in England, endeavoring to purchase certain United States patents that had been secured by Mr. (now Sir) Henry Bessemer for a process essentially the same as that of Mr. Kelley, and for a time it was believed that he would be successful in so doing. I mention this at the outset of my paper, as it explains the adoption of certain forms of apparatus, invented by Bessemer, in the Wyandotte experimental works. In discussing with Captain Ward the general scheme for the proposed experiment, it was determined to build an engine of sufficient power and blowing capacity for use in an establishment for producing steel on a commercial scale, should the results in the experimental works justify such an enterprise, and a similar conclusion was arrived at with respect to the size and general character of the converting vessel. As to the rest of the apparatus, it was decided to design it without reference to any possibility of its use in another work, but as cheaply and as simply as it could be made for the purposes of the experiment only; and it was further determined to erect the experimental plant adjacent to, and partly in, the buildings of the Eureka Furnace at Wyandotte, some ten miles below Detroit, on the Detroit River, where Captain Ward had extensive rolling mills. It was also determined that the metal for the experiment should be taken direct from the blast furnace, and that the spiegeleisen should be melted in crucibles.

As soon as this general scheme of procedure was fixed upon, I

entered upon the work of preparing plans for carrying it into execution, and just here difficulties began to be encountered. I had never seen any apparatus for the manufacture of steel by the method proposed, and the description of that used by Mr. Kelley at his abandoned works in Kentucky satisfied me that it was not suited for an experiment on so large a scale as was contemplated at Wyandotte. I had no plans furnished me from abroad, and very little had been published of the details of the new process as conducted in Europe, and I very much doubt if, at the date of which I speak (June to July, 1862), any citizen of the United States, save the late Z. S. Durfee (then in England), had ever seen the inside of a works where steel was made by the apparatus invented by Bessemer. However, I possessed myself of all the information obtainable, and as it was confidently expected that Mr. Z. S. Durfee would be successful in his efforts to purchase the American patents issued to Bessemer for various forms of apparatus for the production of steel by the pneumatic method, it was thought only to be anticipating the acquisition of property rights in the premises to use such of his inventions as seemed suited to the purposes in view.

I accordingly procured copies of Bessemer's patents relating to the matter in hand, which, together with the descriptive account contained in the first edition of "Fairbairn's History of the Manufacture of Iron," embraced all the information then accessible to me, relative to the European practice of the new art.

Having therefore very little knowledge of an exact character as to what had been done by others, but a very clear idea of the rationale of the new process, supplemented by an absolute faith in the great future before it, I proceeded to evolve from my own internal sense of the fitness of things, apparatus and methods suited to the general idea and environment of the proposed experimental works.

Difficult as this task was, it was made almost insupportably burdensome by the pronounced antagonism and outspoken opposition of nearly every influential person in Wyandotte. Among these was an individual holding a responsible position, who seemed to be possessed of a mental capacity suited only to the appreciation of ideas prevalent at the beginning of the sixteenth century, for he believed that the world was flat. "For," said he, "if it was round, Detroit River would be running up hill, which it couldn't do, ye know." When questioned as to the structure of the moon, this worthy re-

plied that "he s'posed it was a sort of a reflector, like;" and on being asked what held this reflector up, he answered, with a great many profound shakings of the head, that "that is the thing of it."

I had been but a short time in Wyandotte before I came to know that I was regarded as little better than a mild sort of lunatic, or as a confirmed idiot, who might be tolerated but not for a moment encouraged, still less assisted.*

Notwithstanding the delays and annoyances caused by the multitudes of antagonisms encountered on every hand, the work progressed, so that on the return of Z. S. Durfee from England in September, 1862, I was enabled to show him the converter in a nearly complete state, and was very much pleased to hear him say that it "looked very like converters that he had seen abroad." In the winter of 1862-3 the construction of the blowing engine was commenced, but owing to various interruptions occasioned by the war, strikes, and the fact that part of my time was occupied in supervising work at Chicago, the engine was not completed until the spring of 1864. This engine embraced several novel ideas, which I trust will be found of interest even now.

Having thus sketched the commencement and early history of the works, I will now ask your attention to their general arrangement, and also to some of their details.

The plant was located in the old buildings belonging to the Eureka blast furnace, some small additions being made thereto in which to place the blowing engine and converter.

I never fully understood just why the blast furnace aforesaid was called Eureka; the only theory at all satisfying to my mind being, that some expert in blast furnace history, looking for an example of ancient practice that embodied the most faults in design, construction and management, and being satisfied when he found this plant that further search was useless, had suggested the word as a most appropriate name for a furnace that more than satisfied his ardent desires for the discovery of the most archaic of metallurgical structures.

The plan (Fig. 3) shows the general features of the arrangement adopted, save that over the center of the casting-pit was a single-

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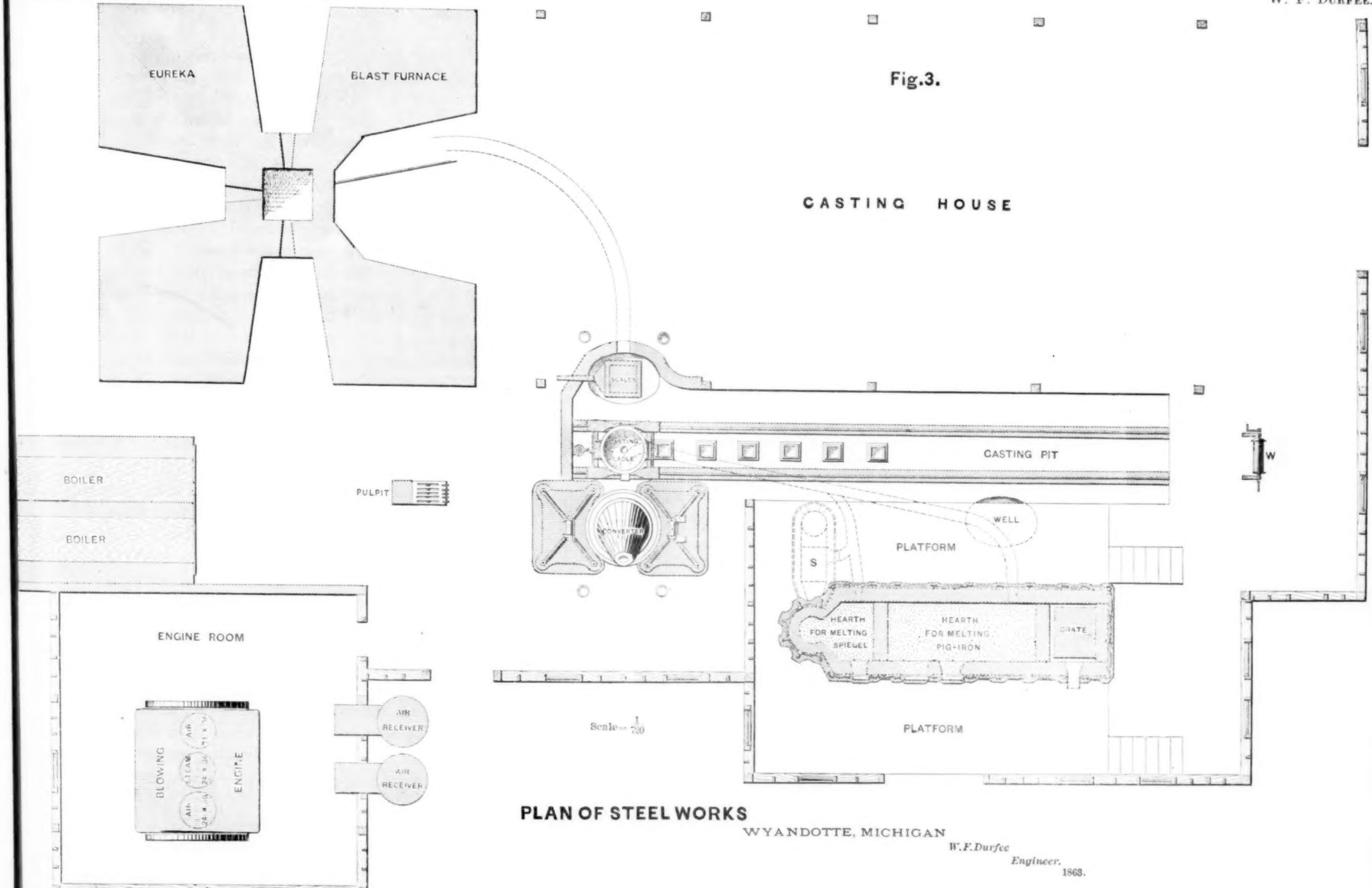
*** Indeed, the great Herr Unkunde Unheilschwanger, then the leader of metallurgical thought and practice in the vicinity, formulated the popular belief by openly declaring that "if that d——d Yankee expected to blow cold air through melted iron and not have it chill up, he *must be* a d——d fool."**

Fig.3.



PLAN OF STEEL WORKS

WYANDOTTE, MICHIGAN

W. F. Durfee

Engineer.

1863.

track traveling hoist for handling the ingots and moulds. This hoist was operated by a winch located at W. It was adopted not as the best, but as the only available expedient for the purpose, the space allotted me in the casting-house and the construction of the house itself not permitting the use of a crane of ordinary form.

The reverberatory furnace for melting pig iron was not included in my original programme of procedure, but was erected in the summer of 1864, for reasons that will be given in another place.

It will be observed that there is an elliptical well, or more properly a reservoir, located beneath the rear platform of the reverberatory furnace. A pipe conveyed water to this reservoir, and care was always taken (except in the following described instance) to have it filled with water before the commencement of a blow. The purpose of the well was to receive any steel that might remain fluid in the casting ladle, in case its tap-hole should chill. If the well was full of water it was a perfectly safe operation to turn two or three tons of fluid steel into it, thus cooling it in small shots and more or less irregular masses of manageable size, which could be utilized in various ways; whereas, had the metal chilled solid in the casting ladle, the result would have been a mass of such dimensions as was practically valueless at the time in that locality. With this well is associated a comedy, which came unpleasantly near being a tragedy. After several conversions had been made in the works, I was called to Chicago, and left Wyandotte with the intention of being absent about a fortnight. Having no expectation that the works would be operated until my return, I left my assistant for the time being charged with the supervision of certain repairs, among which was the relining of the converter, an event which occurred at unprofitably frequent intervals. The first knowledge that I received that my assistant had got into trouble at Wyandotte, was conveyed in a letter from Capt. Ward, which read as follows:

DEAR SIR—I wish you would come immediately to Wyandotte and look after that man X—— of yours. He will kill somebody by-and-by.

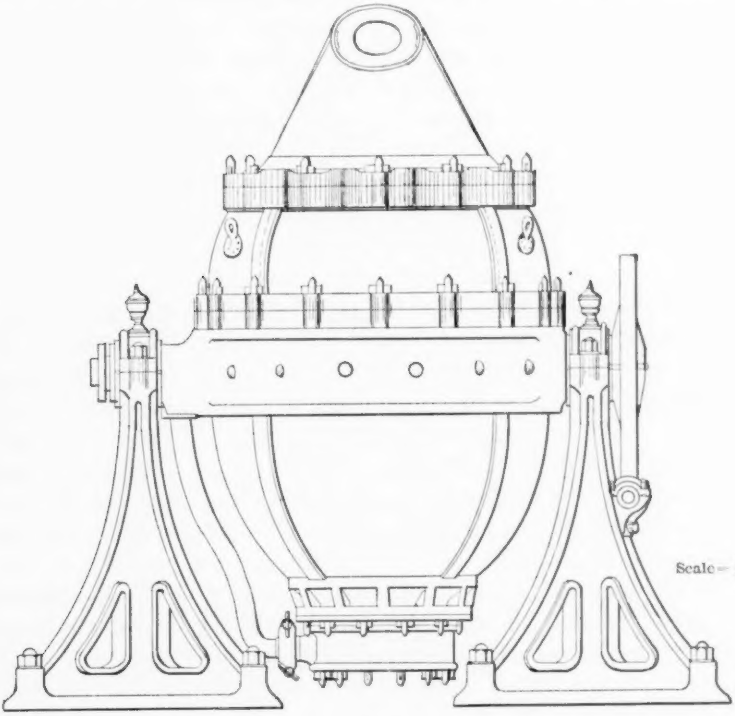
(Signed) Yours, truly,

E. D. WARD.

I returned at once to Wyandotte, and ascertained that my assistant, at the request of Capt. Ward, had attempted to run the works for the benefit of a large excursion party that he had brought down from Detroit on one of his steamers. With this party were

the late Senators Wade, of Ohio, and Chandler, of Michigan, together with sundry judges, bankers and merchants of repute.

I learned that my assistant had treated the captain's distinguished guests in rather an unceremonious manner, for having been unfortunate enough to have the tap hole of the casting ladle chill after successfully teeming two ingots, he ordered the ladle emptied into the well, which he had neglected to fill with water; and the result of turning two tons of fluid steel upon about a



Scale = $\frac{1}{40}$

Fig. 7*a*.

barrel of water which chanced to be in the well, was a terrible explosion, the metal flying in all directions. Senator Chandler was prostrated at full length in the pig-bed; Senator Wade was projected upon a pile of sand in a corner of the casting-house; others of the party were more or less burned, and otherwise injured, while Capt. Ward himself was blown bodily through the open doors of the building into the yard upon a pile of pig iron. For a time everything was confusion, but it was soon ascertained that by

great good fortune no one of the visitors was seriously hurt, and they all returned to Detroit thoroughly of the opinion that they did not care to see steel made by the "new process" again.

Returning from this digression and continuing the description of the works, Fig. 4 is a view of the machinery as it appeared to a person standing in the pulpit (Fig. 3), and looking toward the converter, V. This converter, the first in which steel was ever made in America, is also represented on a larger scale in elevation and

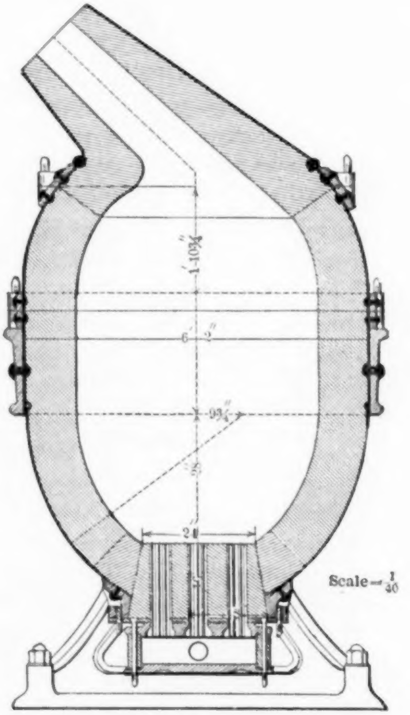


Fig. 7 b.

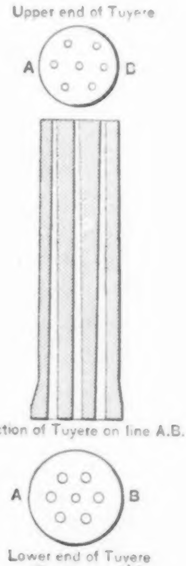


Fig. 7 c SCALE 1/12

vertical section by Figs. 7*a* and 7*b*, in which it will be seen that the upper part of the converter is composed of two separable sections, that its trunnions are supported by tall cast-iron standards and that it is turned by means of worm wheel gearing. This was so arranged that it could be driven either by hand or power.

I have been told that this converter was the first ever constructed with a cast-iron trunnion hoop.

The converter was provided with seven tuyeres, each having

seven holes five-sixteenths of an inch in diameter; these tuyeres are shown in detail in Fig. 7c. In Fig. 4 is shown the traveling crane which supplied the converter with metal taken direct from the blast furnace. The peculiar construction of this crane was forced upon me by the nature of the surroundings, the limited space allotted me in the casting-house, and the necessity for rapid work when work was required of the crane. The sustaining elements of the crane were two parallel cast-iron girders, each of which was supported by two posts. On one end of these girders was placed the converter stack, and on that part of the girders not thus occupied traveled a carriage having two transverse shafts on which were placed chain drums for hoisting. One extremity of each of the transverse shafts was provided with a worm gear engaging with a worm which was free to slide upon a shaft that extended along the outer side of each girder, said shaft giving motion to the worm by means of a groove that engaged a feather on the interior of the worm, thus allowing it to slide along the shaft and revolve with it at the same time. On either end of the worms before named was a brass collar, C, C (Fig. 6), beyond which was a rubber spring S, following which came two brass collars, the last of which abutted against the boxes in the hangers H, H, which finally received the thrust due to the load suspended from the crane. The purpose of the springs S S was to avoid shock when starting the load from a state of rest. Fig. 6 shows the general detail of the traveling carriage of the crane, an half section of one of whose hoisting drums is shown in Fig. 6b.

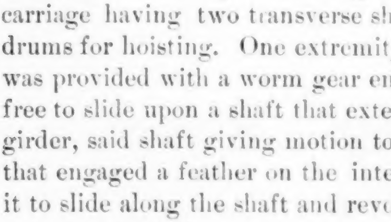

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Fig. 6b

On the transverse shaft nearest the converter were placed two chain drums, which were provided with hook-ended chains of equal length, one of which is seen at C (Fig. 4). The other transverse shaft was furnished with a single chain drum, which occupied a position midway between those on the first transverse shaft; this drum was furnished with a single hook-ended chain, seen in Fig. 4 at O.

The traveling carriage was drawn in either direction along the girders by a pair of pitch chains, one of which is shown at P (Fig. 4). These passed over rag wheels, R, on transverse shafts which turned in bearings at the end of the girders.

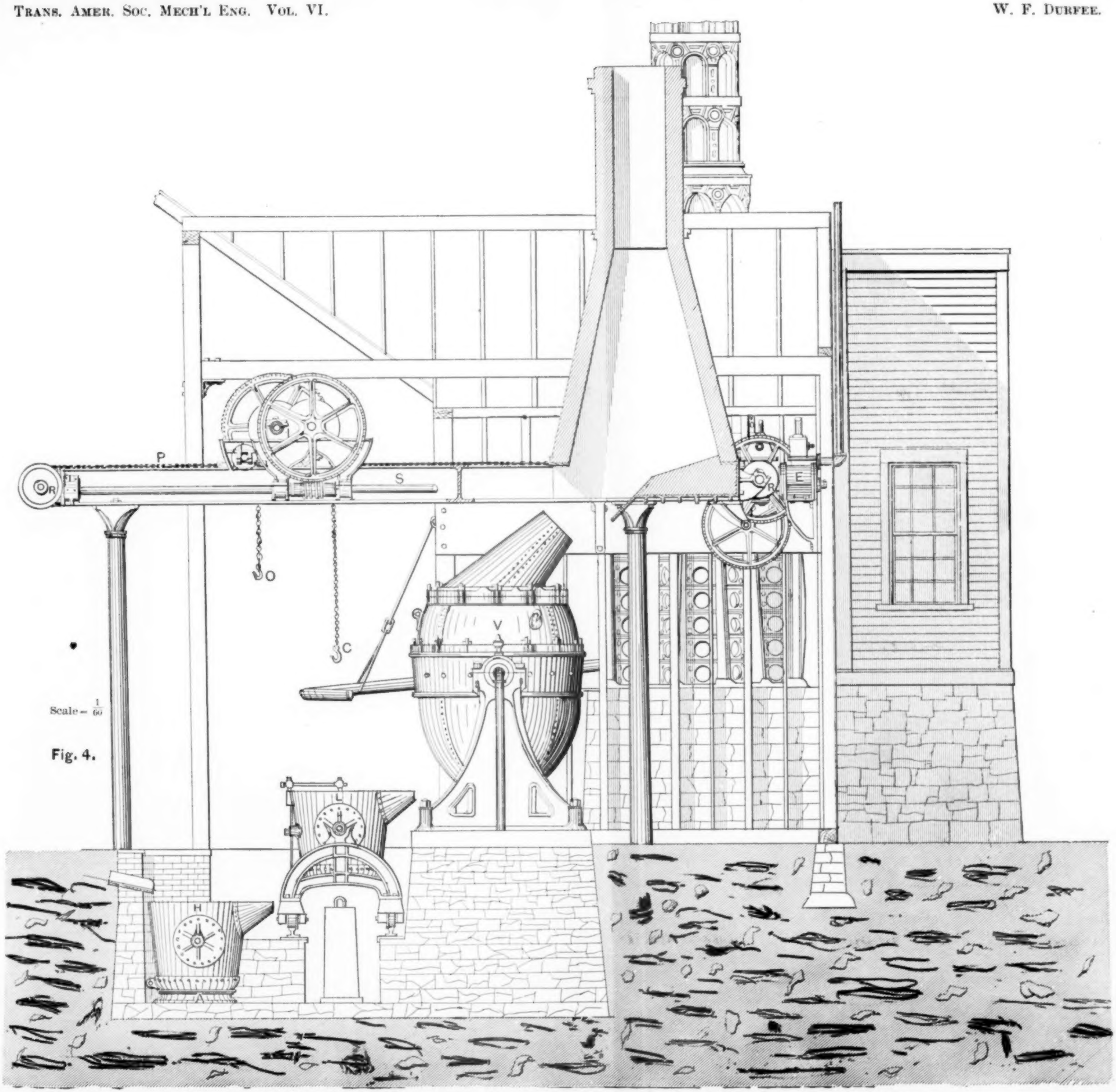
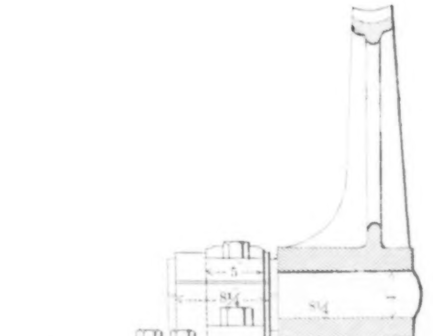


Fig. 4.

Section of CONVERTER HOUSE of Steel Works at Wyandotte, Michigan. W.F.Durfee, Engineer, 1863.



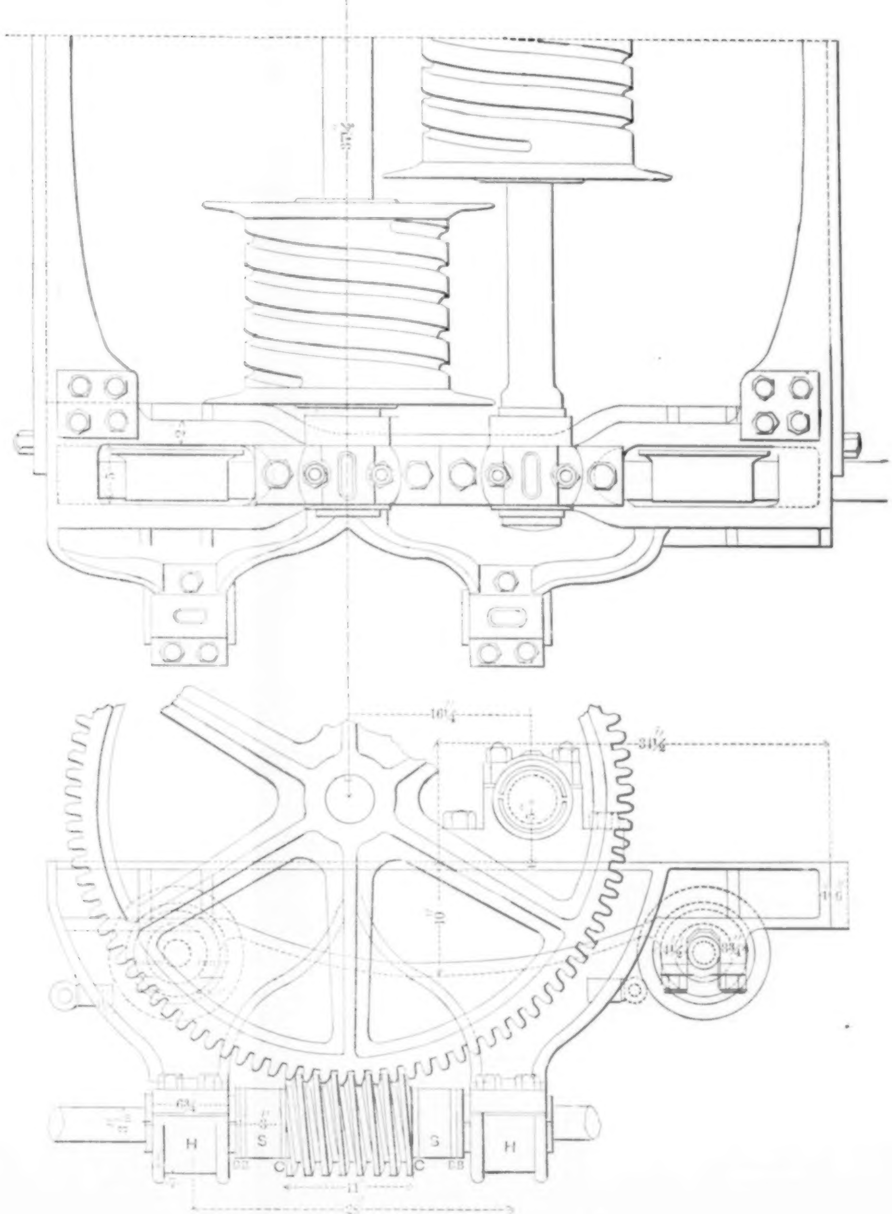


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Fig.6

The whole of the mechanism of the crane was actuated by three small quick working rotary engines, one of which is seen at E (Fig. 4). Each of these engines was capable of prompt reversal by means of a rocking shaft, on whose end was a weighted lever, connected by a small wire cord, R (Fig. 5), to one of the latch levers of the pulpit.

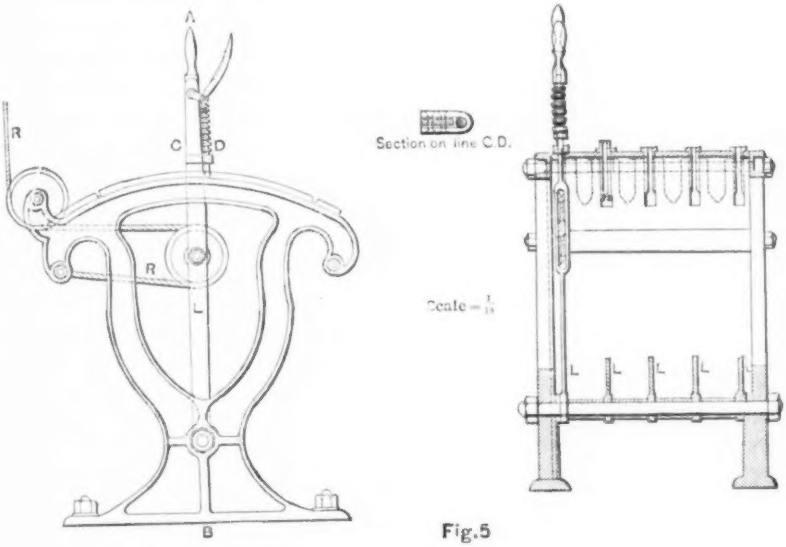

Fig.5

There was a fourth engine of the same description employed for turning the converter, which was controlled by a lever at the pulpit, as was also the valve which regulated the admission of blast to the same.

The lifting of the fluid cast iron delivered by a runner from the blast furnace into the hoisting ladle H (Fig. 4) placed upon the platform of the scales A, and the pouring of it into the converter, was accomplished as follows:

The carriage of the crane was run to the left-hand end of the girders on which it was placed, and the hooks on the double chain C were attached to the trunnions of the hoisting ladle, and the hook on the single chain was connected with the eye seen (in Fig. 4) on the left-hand side of its bottom. The converter having been turned into a proper position to receive the metal, the hoisting gear was put in action and the ladle H (Fig. 4) with its contents was raised and carried to the right until the end of its spout was over the mouth of the converter. The raising of the ladle was then stopped,

and it was turned upon its trunnions by continuing the hoisting on the chain O, until all the metal which it contained had been discharged into the converter.

From the standpoint of present practice the adoption of rotary engines for the various purposes named is doubtless open to criticism, but I will ask you to remember that at the time of which I write the best European practice with the converter was three "blows" in ten hours, and that if we did as well as that (we had no expectation of doing better) in our experimental works, the total time per day during which these engines would be called into action would not exceed three-quarters of an hour; thus the question of the economical employment of steam for the purposes of doing the work required of the rotary was of minor importance as compared with those of first cost and compactness of the apparatus employed.

The blast furnace was run by a contractor who was naturally and properly solicitous that no part of the iron made should be unaccounted for;—hence one of the reasons for weighing the iron before it went to the converter. But as simple a matter as this was, it did not escape the criticism of Herr Unkunde Unheilschwanger, who insisted that, because iron expanded when heated, "there must be more weight in a hundred pounds of melted iron, than in a hundred pounds of ordinary pig metal."

In the summer of 1864, before the first conversion was made, it was decided that, as we would be called upon to experiment with a variety of brands of pig metal, sent us by parties interested in the works, it was expedient to build a reverberatory furnace. This was accordingly done, and a hearth was made near the base of its stack for the melting of spiegel; and subsequently a small furnace (located at S, Fig. 3) was constructed for melting spiegel when the metal for conversion was taken direct from the blast furnace. In consequence of the erection of these furnaces, my original idea of melting the spiegel in pots was not even attempted.*

In the early part of the year 1864, the late Z. S. Durfee wrote me from England (whither he had returned in the fall of 1863 with a view of concluding the negotiations with Bessemer, and also of

* In connection with this matter of melting pig metal for the purpose of conversion, I will here state that it was at the Wyandotte experimental works in the summer of 1865, that the late Z. S. Durfee made the first attempt to melt pig metal in a cupola for use in the converting vessel, and I claim for him the origination of this practice which is now so universally employed, and which has contributed so much to the economy of production in all our steel works.—W. F. D.

purchasing the United States patent of R. F. Mushet for the use of *spiegeleisen* in the manufacture of steel) that it was possible that his efforts to purchase Bessemer's American patents would not be successful, and he sent me rough sketches of a stationary converter from which the steel could be tapped after conversion very much in the same way as iron is tapped from an ordinary foundry cupola, and he urged the advisability of losing no time in having such a converter built. I accordingly at once commenced the study of the matter, and prepared drawings for a stationary converter of which Fig. 8 is a vertical section. It is substantially of the same

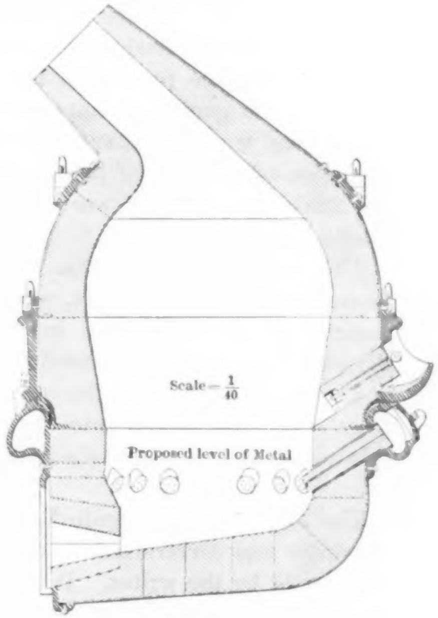

Fig 8.

form as the sketches sent me, but differed from them in the substitution of inclined side, for bottom tuyeres. This converter was built and erected in the works at Wyandotte, but no metal was blown in it until some time in the fall of 1865, some months after I ceased to be connected with the enterprise. In this converter it will be noticed that the upper and outer ends of the tuyeres are above the level of the metal in the converter, and that their lower ends are but a few inches below that level. Therefore the pressure of blast necessary to overcome the ferrostatic head at the inner

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ends of the tuyeres was much less than would have been required had the tuyeres been inserted in the bottom. This arrangement also permitted the blast to be stopped without fear of the metal in the converter running through the tuyeres. One of my reasons for thus placing the tuyeres, was the belief that such heavy pressure as was required for bottom-blown converters was not at all essential to the production of a good quality of steel. My reasoning was that to expel the carbon from the metal under treatment, it was only necessary to bring in contact with it the requisite number of atoms of oxygen, which I believed (and still believe) could be better accomplished by a large volume and low pressure, than by a small volume and high pressure of blast. I have had no opportunity of practically demonstrating this belief; but will here venture the prediction that one direction in which the genius of improvement will walk in our steel works, is that which leads to a great reduction of pressure of blast, and a corresponding diminution of the power required for its production.

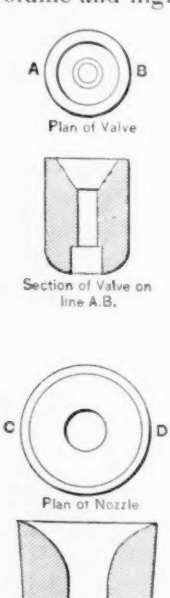


Fig. 8a SCALE 1/6

The casting ladle (L, Fig. 4) employed was mounted on an iron carriage, which bestrode the casting pit in which the ingot moulds were placed. This ladle was provided with a nozzle and valve in its bottom, through which its contents were discharged into the ingot moulds: this nozzle and valve are shown in detail in Fig. 8*a*.

The engine which supplied the blast for the converter was constructed from working drawings made by the writer. It was intended to produce a pressure of blast of sixteen pounds per square inch, which was about double the pressure used at that time for any metallurgical work, and was regarded as very heavy; in fact, I was informed at the time of commencing the plans for this engine (the winter of 1863) that the pressure used for blowing steel in England and Sweden was but eight pounds. I adopted the higher pressure with a view to shortening the time required for a blow, in the full belief that I was taking a decided step forward in the practice of the pneumatic process, though in this I soon became satisfied that I was in error. But whatever mistakes I made in this matter of blast

pressure, I had the comforting satisfaction of finding myself in most excellent company, for before my engine was finished, steel was blown in England with a blast-pressure of twenty-five pounds, a practice which has continued unto the present time. The blast

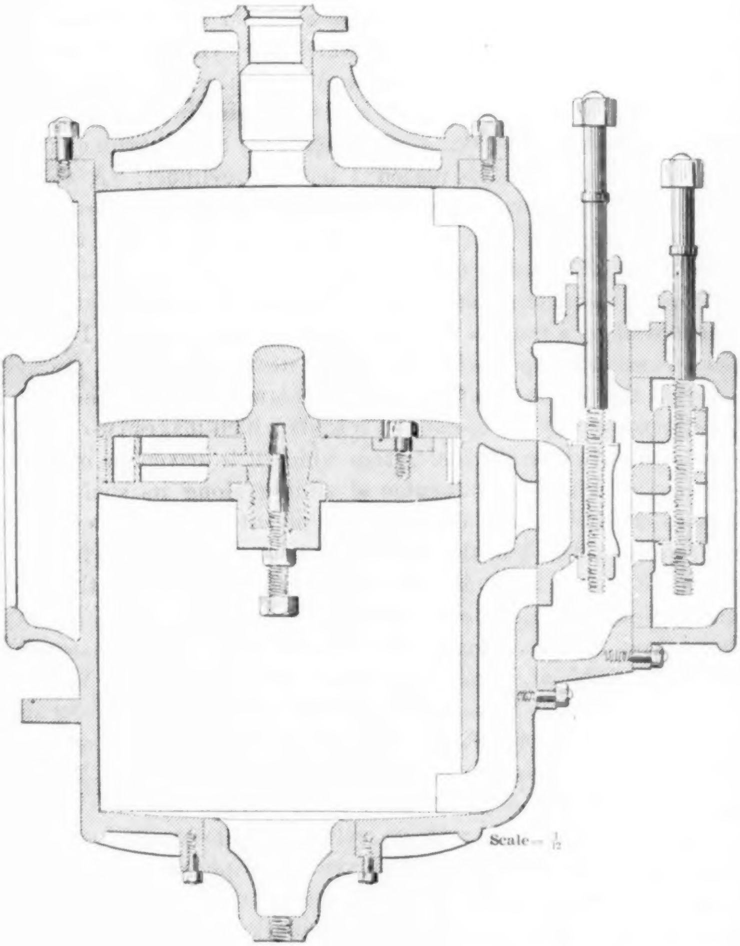
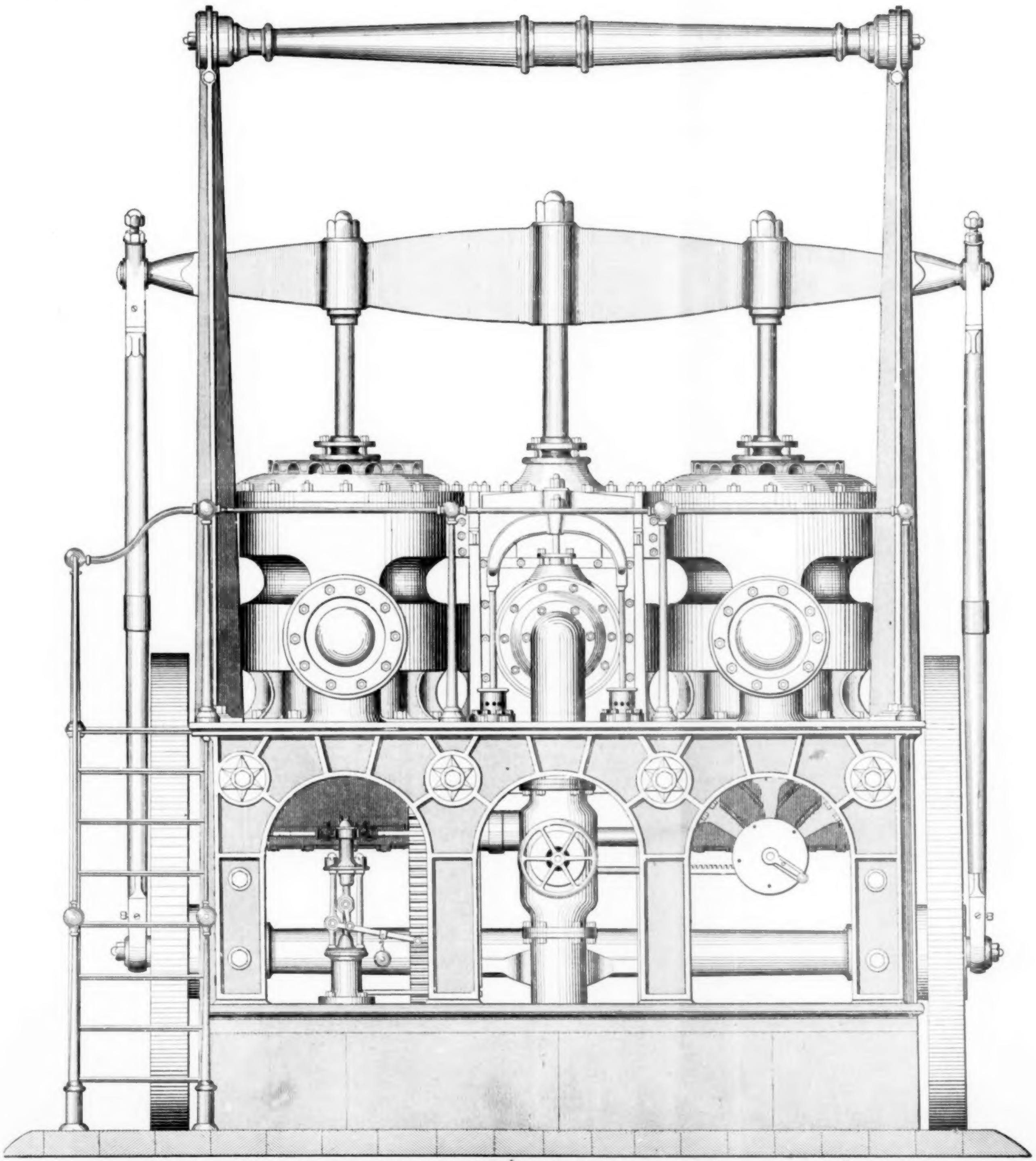


Fig. 12

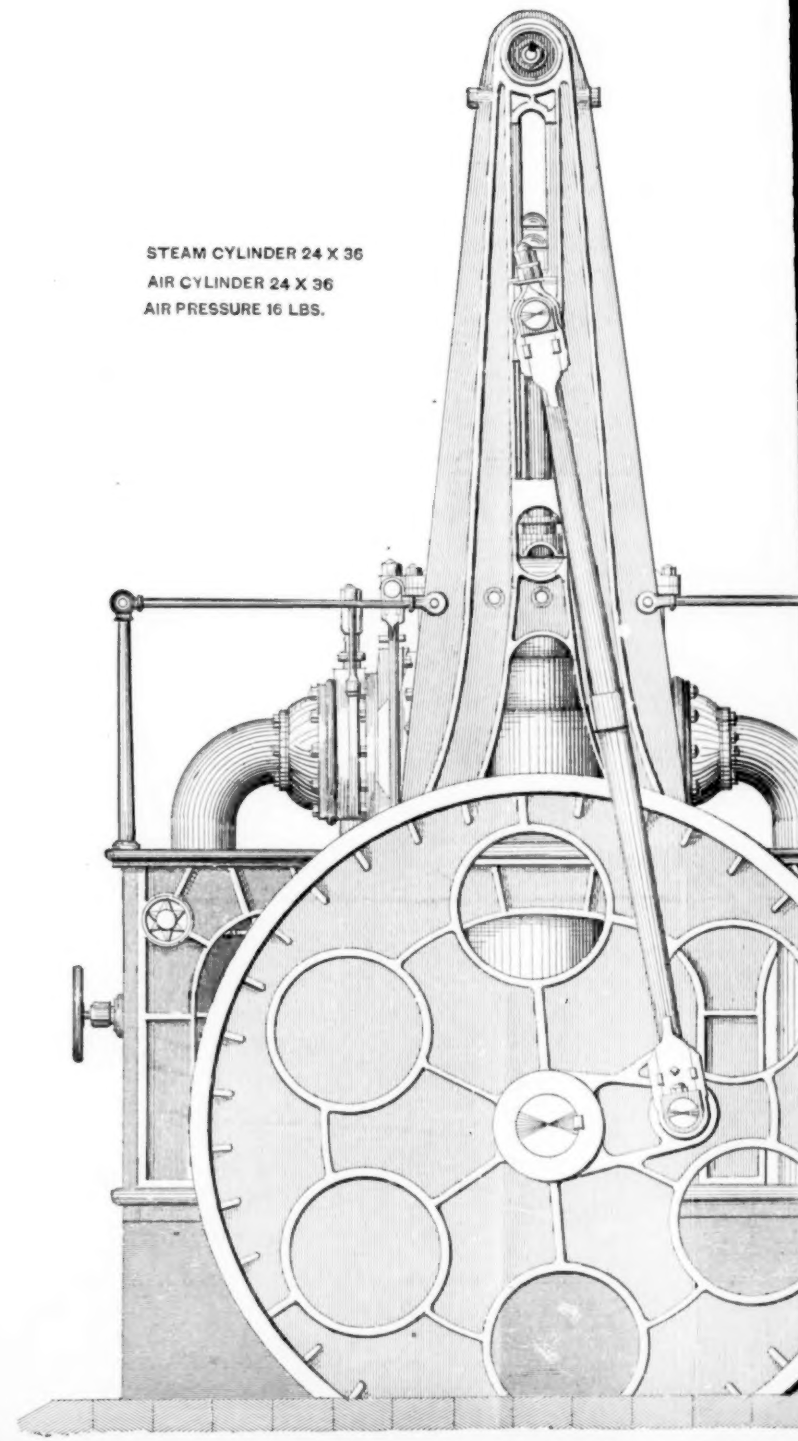
engine above named is represented in front and end elevation by Figs. 1 and 2. The construction consists of three cylinders of the same diameter (24 in.) and length of stroke (36 in.) whose axes are in the same vertical plane, and whose piston rods are connected to

a wrought-iron cross-head, whose vertical movement is insured by suitable guides near its ends. From the ends of the cross-head descend connecting rods to crank pins in the faces of fly-wheels on the extremities of a horizontal cast-iron shaft passing beneath the cylinders, and having its axis in the vertical plane before named. The above described combination is enclosed and sustained by a strong framework of cast iron, to which the cylinders are securely bolted; and the whole rests upon a substantial stone foundation, a part of which is seen above the floor of the engine-room. The central cylinder shown in the front elevation (Fig. 1) is the steam cylinder, on each side of which is placed a blowing cylinder. A vertical section of the steam cylinder taken at right angles to the cross-head is shown in Fig. 12. On the right of this section is seen the slide valve in the main steam chest, anterior to which is a gridiron cut-off valve in a smaller steam chest. The slide valve is actuated by a lever moved by a cam on the main shaft of the engine. This cam is so formed as to open wide the proper steam and exhaust passages as soon as the cranks pass their upper and lower centers. The cut-off valve is worked by a step-cam on a shaft which makes two revolutions to one of the main shaft, from which it is driven by means of spur gear. This cam is capable of sliding along its shaft, and is adjusted and held at any desired point of cut-off by means of hand gear shown in Fig. 1 in the right hand arch of the engine frame.

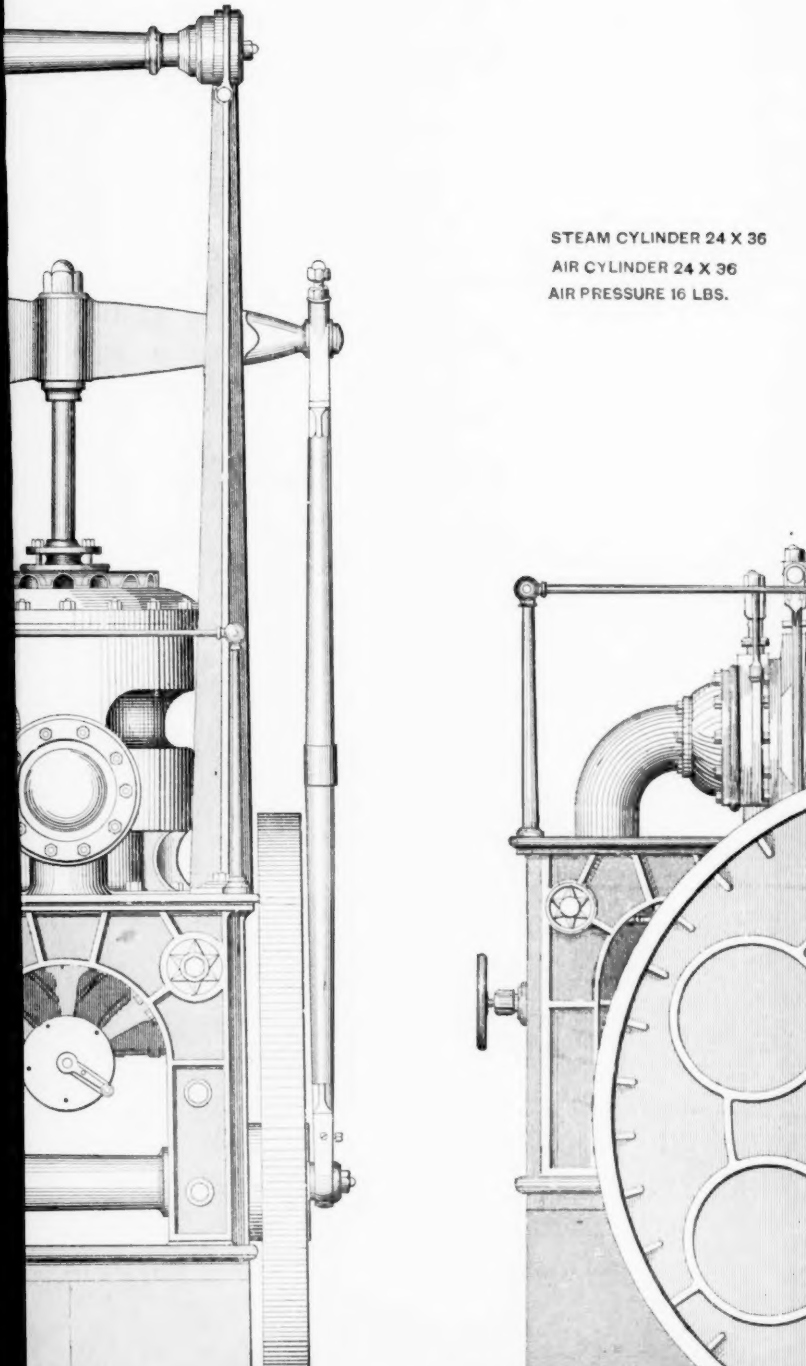
The main slide valve is very nearly balanced, and at the same time lubricated by the means shown in Figs. 9 and 10. Fig. 10 is a horizontal section through the exhaust port of the main slide valve, each flank of which is provided with a groove V, V, which is closed at its ends. Into this groove is introduced oil under such pressure as will nearly lift the valve from its seat when subjected to its minimum pressure of steam. The apparatus by which the required pressure of oil is obtained is represented in vertical section by Fig. 9, in which C is a steam cylinder containing a piston, the upper end of the rod of which serves as the plunger, R, of an oil pump, whose suction and discharge valves are shown at V and V′. At L, is seen a lever on which slides a block, having suspended to it a weight W, by which the apparatus is adjusted in conformity with the pressure of the steam admitted to the cylinder C, through the pipe P. The area of the piston in the cylinder C, is such, that when acted upon by steam of the standard working pressure, the total resultant pressure communicated by the plunger, R, to the oil in the oil pump is nearly sufficient to balance the main slide valve,



Front Elevation of Wyandotte Steel Works **BLOWING ENGINE**, Designed by W. F. Durfee Engineer, 1863.

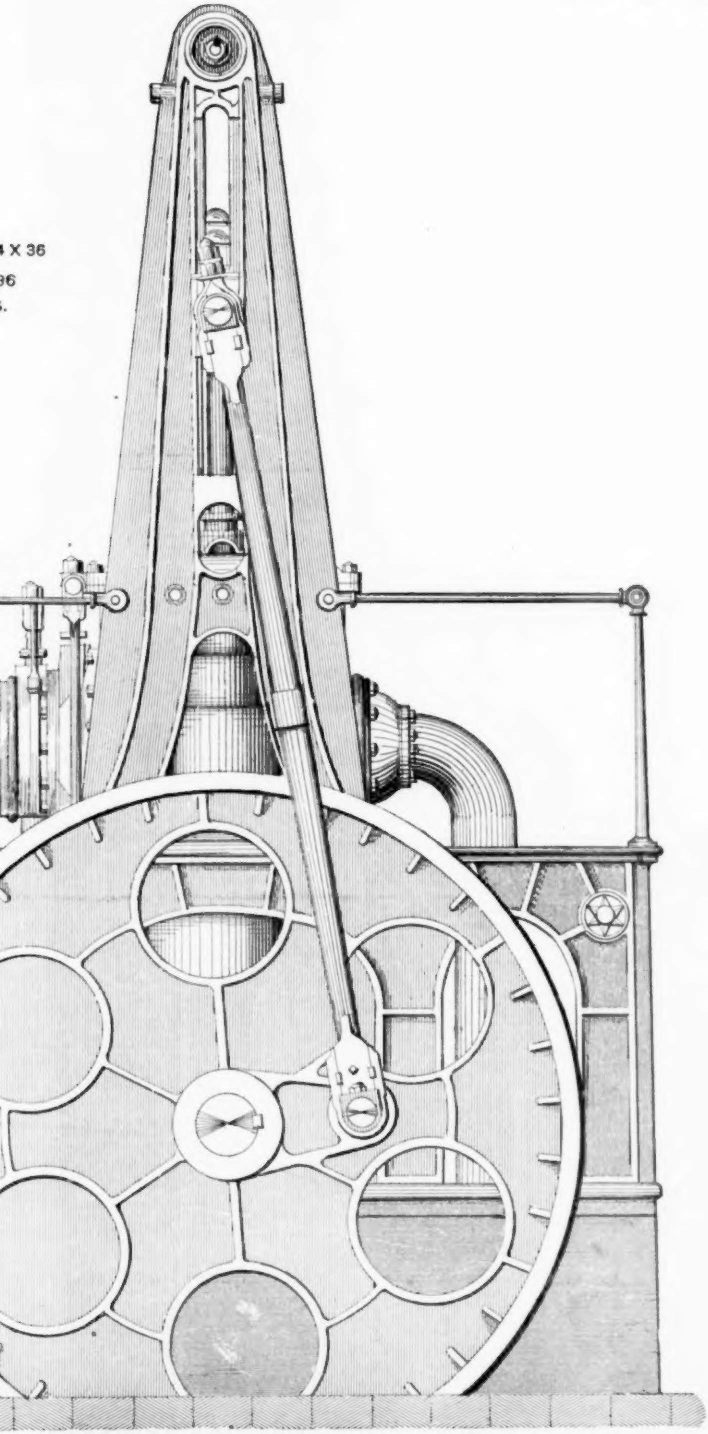


End Elevation of Wyandotte Steel Works **BLOWING ENGINE**, Designed by W. F.



F. Durfee Engineer, 1863.

End Elevation of Wyandotte Steel Works



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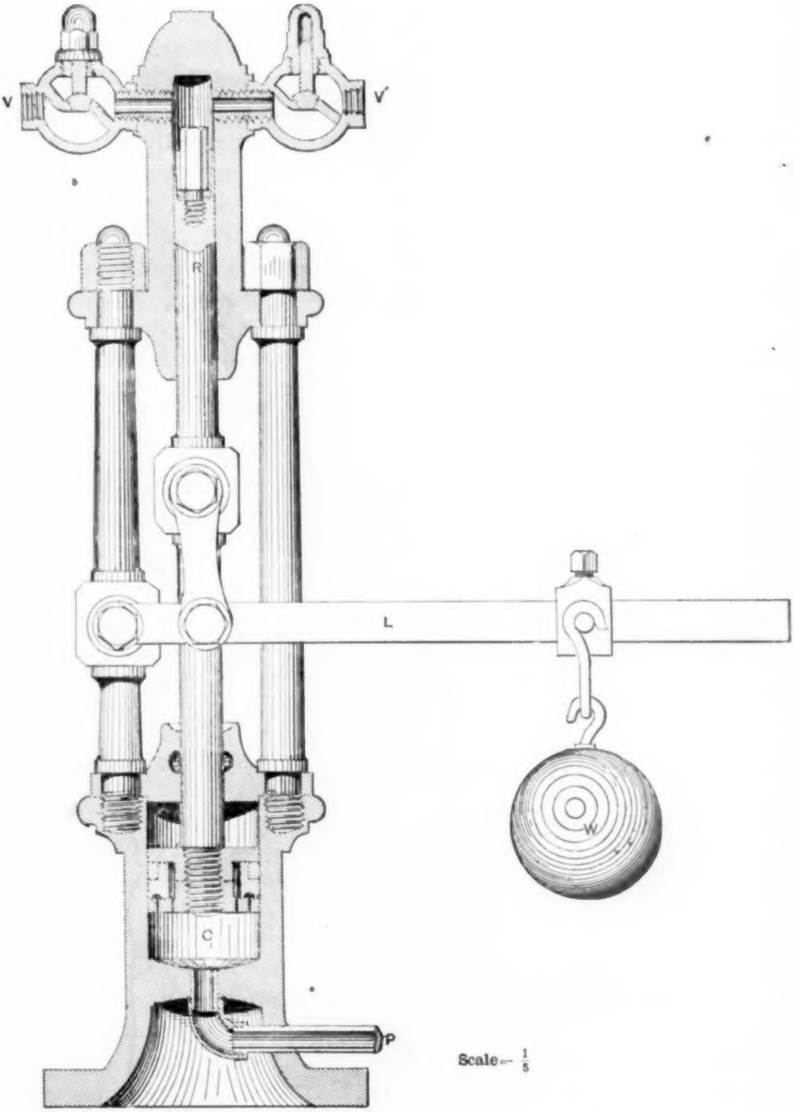


Fig. 9

through the medium of the oil in the grooves V V (Fig. 10), which are connected to the discharge valve V′ (Fig. 9) of the oil pump by means of prolongations of the pipes P P (Fig. 10).

Any slight deviation from standard steam pressure is compen-

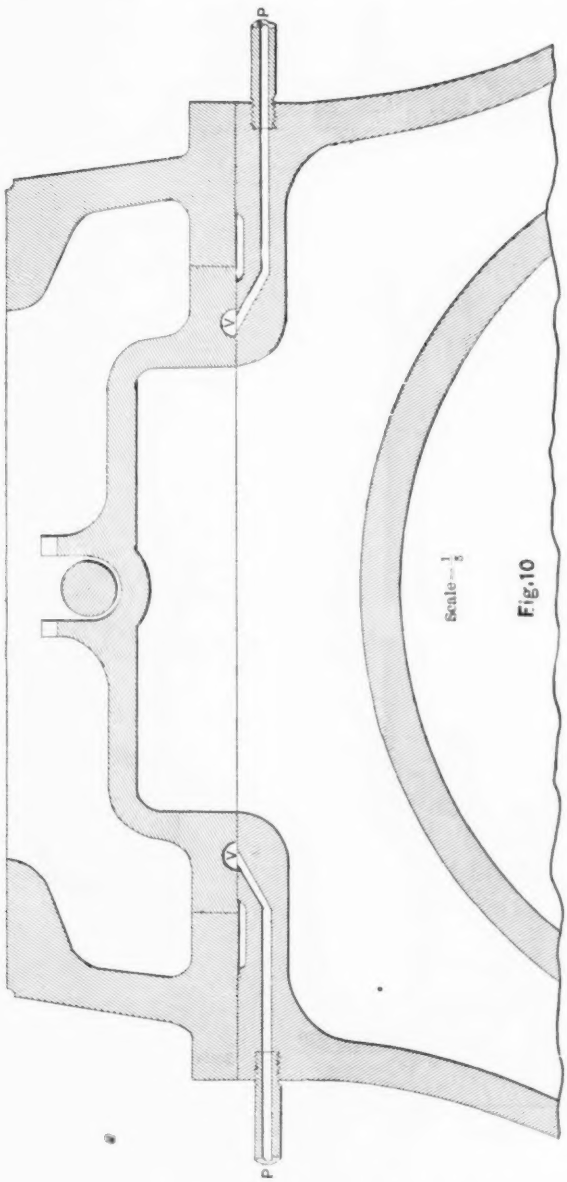


Fig. 10

sated for by a proper adjustment of the weight W (Fig. 9). If for any reason (such as leakage of oil from under the slide valve) the piston in the cylinder C (Fig. 9) rises to its top, the apparatus

will, so long as the piston remains in that position, cease to have any balancing effect upon the slide valve, but it can immediately be rendered effective by forcing the piston to the bottom of the cylinder C (Fig. 9) by means of the lever L. If the slide valve is properly fitted, and the oil pump correctly adjusted to the pressure on the valve, it will not be necessary to depress the lever L very frequently. The above described balancing apparatus was placed in the left-hand arch of the engine frame, as is shown in the front elevation. The construction of the steam piston is a somewhat peculiar one, which is well adapted to pistons moving vertically

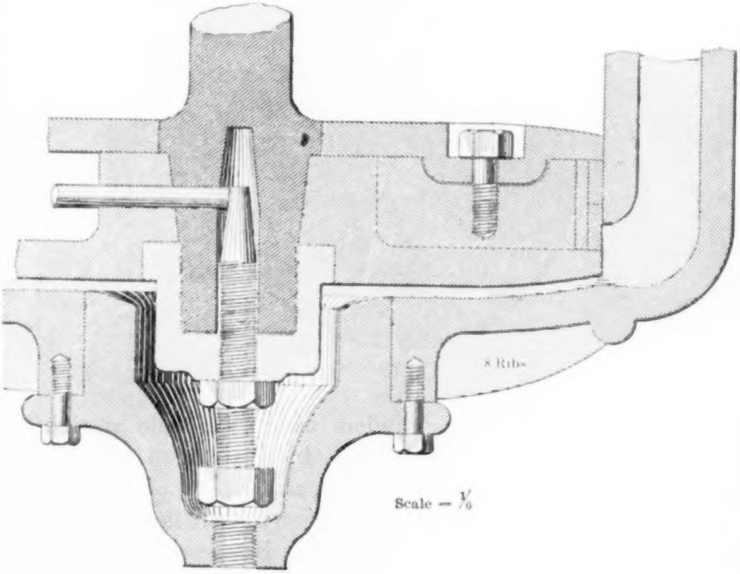
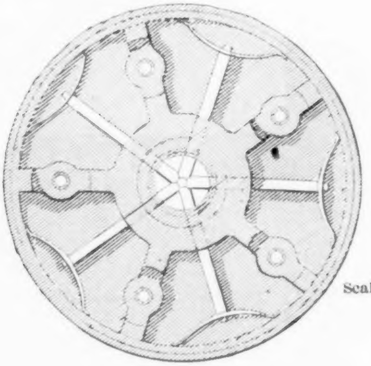


Fig. 11

under the conditions which existed in the Wyandotte engine, but would not, I think, be found satisfactory for those having a horizontal movement. The piston in question is provided with the usual pair of metallic packing rings backed by a junk-ring, whose interior surface is acted upon by the extremities of five curved blade springs, which are compressed simultaneously, by means of five radial cylindrical rods, whose inner ends abut on the conical upper end of a central adjusting bolt, access to which is had by removing the bonnet which closes the central opening in the bottom of the steam cylinder. The whole construction of this piston

is clearly shown in Figs. 11, 12 and 13, the last of which is a plan of the piston with its follower removed. The pistons of the blowing cylinders are constructed substantially the same as the steam piston just described, but instead of having metallic packing rings outside of the junk-ring, that ring is made somewhat smaller in diameter than it is in the steam piston, and outside of it are placed a series of segments of harness leather, well saturated with tallow. These segments break joints with each other and occupy all the space between the junk-ring and the interior surface of the cylinder, as will be seen in Fig. 14, which also shows a diametrical vertical sec-



Scale = $\frac{1}{12}$

Fig. 13

tion of one of the blowing cylinders made by a plane at right angles to the cross-head of the engine. In this section will be seen the elastic belt inlet and outlet valves, at each end of the cylinder. These valves were of the same construction as were in use on engines designed for English steel works by Mr. Bessemer. The section also shows one of the five supplementary inlet valves (which may with propriety be called *lip* valves) that were placed in each head of the cylinder. The position of these last named valves is more clearly shown in Fig. 15, which is a view of the upper cover (as seen from below) of one of the blowing cylinders. In the works and with the machinery described, was produced on one of the early days of September, in the year 1864, the first Bessemer steel made in America. This event was a great disappointment to all the enemies of the new enterprise; as they had filled the air with predictions of failure, and poisoned it with the miasma of discouragement; and they immediately turned their attention to a general depreciation of the results attained, and the persecution, with

renewed vigor, of all who were responsible for them. The great Herr Unkunde Unheilschwanger, seeing that "blowing cold air through melted iron" did *not* make it "chill up," suddenly de-

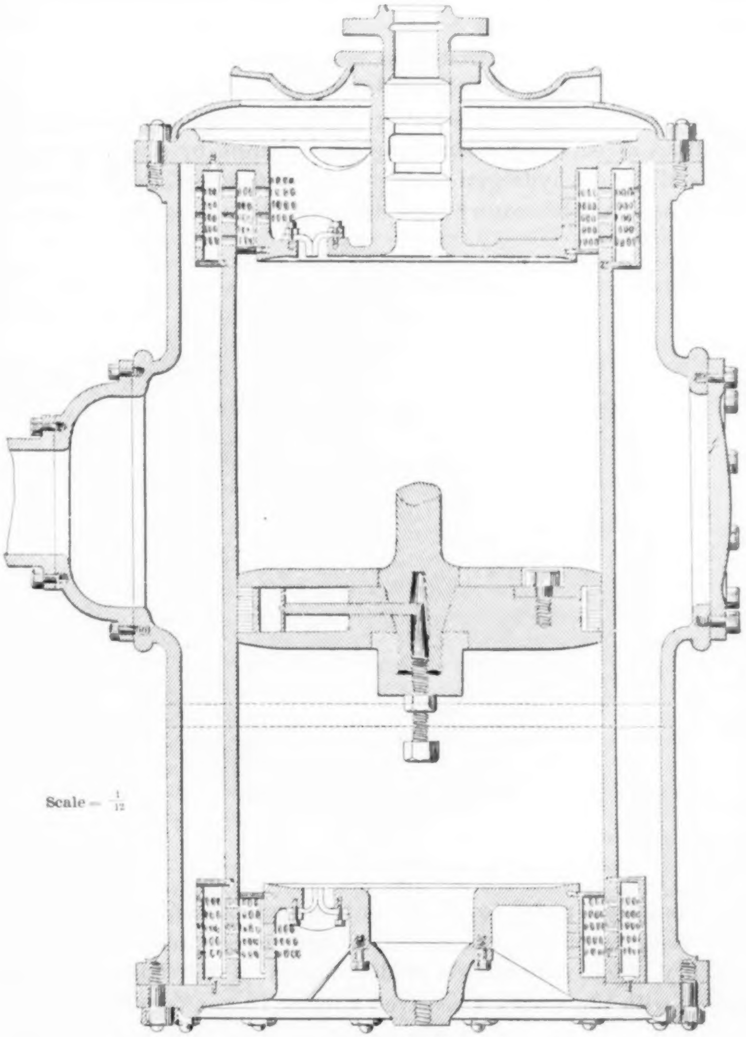


Fig. 14

clared that "'twas easy 'nuff to make steel! All ye'd gut ter du, was ter pore th' iron in that ere pot, und blow her awhile, und run in sum er that ere t'other met'l, und pore her out, und she's

steel, ye know." In a paper read at the Troy Meeting of the American Institute of Mining Engineers, I mentioned briefly some of the crimes against progress perpetrated by the ignorant and mischievous gang, of which Herr Unkunde Unheilschwanger was the recognized chief, who, like Satan,

"Exalted sat, by merit raised
To that bad eminence."

Not content with burglarizing the laboratory, and endangering the lives of those who were employed therein by plugging up the pipes of the oxyhydrogen blow-pipe, or with effecting the final destruction of the laboratory itself, they invaded the sanctity of private

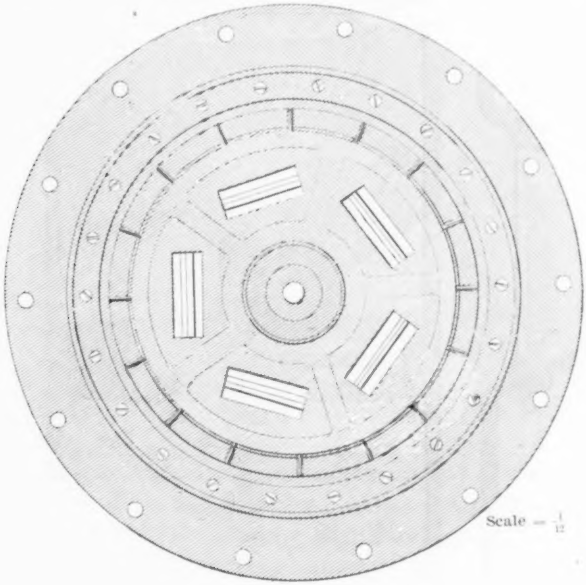


Fig.15

correspondence; and no person or thing was safe from the virus of their tongues or the penetration of their eyes. In the month of January, 1865, soon after the destruction of the laboratory connected with the works, I received a letter from a friend, whose opportunities for observing the secret (as they thought) operations of this syndicate of sin were much better than my own. He says: * * * "I am pleased to hear from you again, and yet was sorry, for I know what your feelings would be upon returning home; as I was unfortunate enough to be at Wyandotte at the time the

raid was being made upon your office and laboratory. I saw some things at Detroit in which Wyandotte men were concerned that sunk them in my opinion below the most contemptible of our race." * * * Then, after some more remarks, none too emphatic for the subject and occasion, he continues: * * * "Nothing in your vicinity *in writing* is safe from the perusal of *any one who wishes to read*, and anything you don't care to have *pirated*, destroy." After some words relative to other matters, he concluded with this advice, "*Take care of your letters*." This advice I took particular pains to heed.*

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* As a contrast to the behavior of Herr Unkunde Unheilschwanger and his sympathizers, it affords me pleasure to present the following letter from a gentleman whose friendship throughout my connection with the experimental works at Wyandotte I count as one of the treasures of my memory:

"OFFICE OF THE CHICAGO ROLLING MILL CO.,
May 26, 1865.

"MY DEAR DURFEE:

"The meeting of the Iron and Steel men adjourned yesterday to meet in Cleveland the fourth Wednesday in August. I regret very much that you could not have been here, particularly to see how well your steel behaved; and you must allow me to congratulate you upon its entire success. I assure you I was but too proud for your sake that everything we had to do with it proved so very successful. The hammer was altogether too light, of course, and it took more time than it otherwise would to draw the ingots down; yet all the pieces worked beautifully, and they have made six good rails from the ingots sent over, and not one bad one in any respect. The piece you sent over forged is now lying in state in the Tremont House, and is really a beautiful rail, and has been presented to the Sanitary Fair by Capt. Ward. We rolled three rails on Wednesday and three on Thursday. At the first rolling only your cousin and Geo. Fritz were present, at the rolling yesterday were Senator Howe, of Wisconsin, D. F. Jones, of Pittsburgh, R. H. Lamborn, of Philadelphia, Mr. Phillips, of Cincinnati, Mr. Kennedy, of Cincinnati, Mr. Swift, of Cincinnati, Mr. May, of Milwaukee, and three ladies, Mr. Scofield, of Milwaukee, Mr. Fritz, of Johnstown, Mr. Thomas, of Indianapolis, with four strangers, and everything went so well I really wanted you to see some of the good of your labors for so long a time and under such trying circumstances. You have done what you set out to do, and done it well, and I am glad to congratulate you and rejoice with you, for I can appreciate some of your difficulties, and wanted you to hear some of the praises bestowed upon your labors, as you richly deserve. I know this would make no sort of difference to you, yet we all have vanity enough (especially in such cases as this) to feel gratified at any little compliments we know we are entitled to. But I will not tire you with any more, as your cousin can tell you all and more than I can write, but, with kindest regards, allow me to remain,

"Your most ob't,
(Signed,) "O. W. POTTER."

This letter, coming to me at a time when I was worn and exhausted by both physical and mental toil, was like the "shadow of a great rock in a weary land," and I have preserved it carefully among my few pleasant mementos of the time.

In those early days the atmosphere in which I moved was heavy with the fog of discouragement. All the so-called practical iron men in the vicinity of Wyandotte were opposed to the new process. I well remember the sneers of contemptuous incredulity which greeted my statement that the time would come "when a steel rail could be made cheaper than an iron one;" and now that that time has arrived, as I look back upon my work at Wyandotte, with the added experience of twenty years to aid the retrospection, I do not hesitate to claim that it was as good a solution of the problem presented as was possible under the circumstances of time and environment.

Twenty years have elapsed since the first Bessemer steel was made in the experimental works at Wyandotte, and that time, improved by the labors of skillful men from among our engineers, metallurgists and chemists, has wrought wondrous changes in the construction and management of our steel works, rolling mills and furnaces. Practices which were twenty years ago condemned as criminal extravagances, are now regarded as essential economies. Things deemed impossible by men of little faith then, are but the common occurrences of to-day. Buildings, machinery, methods, have all felt the influence of the spirit of progress. Science has become better acquainted with Art, and Art has a better appreciation of Science; and their united forces are marching forever forward. Before their steady advance difficulties vanish, obstacles are surmounted, and seeming impossibilities are overcome; sound principles are established in place of empiricisms, and educated skill replaces laborious ignorance. Verily, "old things are passing away, and all things are become new."

(*This paper was discussed jointly with that of Mr. R. W. Hunt, of Troy, which follows.*)

CLV.

THE ORIGINAL BESSEMER STEEL PLANT AT TROY.

BY ROBERT W. HUNT, TROY, N. Y.

In calling the attention of the Society to a short description of the original Bessemer steel plant at Troy, N. Y., I cannot hope to present matter of much value; and can only crave your indulgence while placing on record the plans and practice of the first Bessemer plant that made a commercial success in America. My paper, taken in connection with that of Mr. Wm. F. Durfee on the Wyandotte works, may possess some additional interest by drawing your attention to the wonderful progress which has been made in this march of metallurgy during the last nineteen years.

As is well known, there were rival patents bearing upon the pneumatic process. The Kelley and Mushet patents were owned by the Kelley Process Co., who built the Wyandotte works; and the various Bessemer patents belonged to Messrs. Winslow, Griswold & Holley, the latter firm erecting the Troy plant. Of course this state of things caused great jealousy and rivalry. Mr. Durfee succeeded in starting his plant a few months before his Troy rival, and hence to him belongs the honor of having made the first heat of Bessemer steel blown in this country. Alexander L. Holley commenced the erection of the Troy works immediately upon his return from England in the spring of 1864, and made the first conversion of steel on February 16th, 1865. From the start, complete records of the works have been kept, and hence I am enabled to present the particulars of their early experience. But before so doing, let me call your attention to the plate, Fig. 16, showing the arrangement of the plant. Its location was determined by the existence of a water-power and wheel which had been used to run a grist mill. This opportunity for cheap power was too good to be neglected; and every other consideration sank into oblivion. Of how well this rewarded the owners, you can judge by the detailed history which follows.

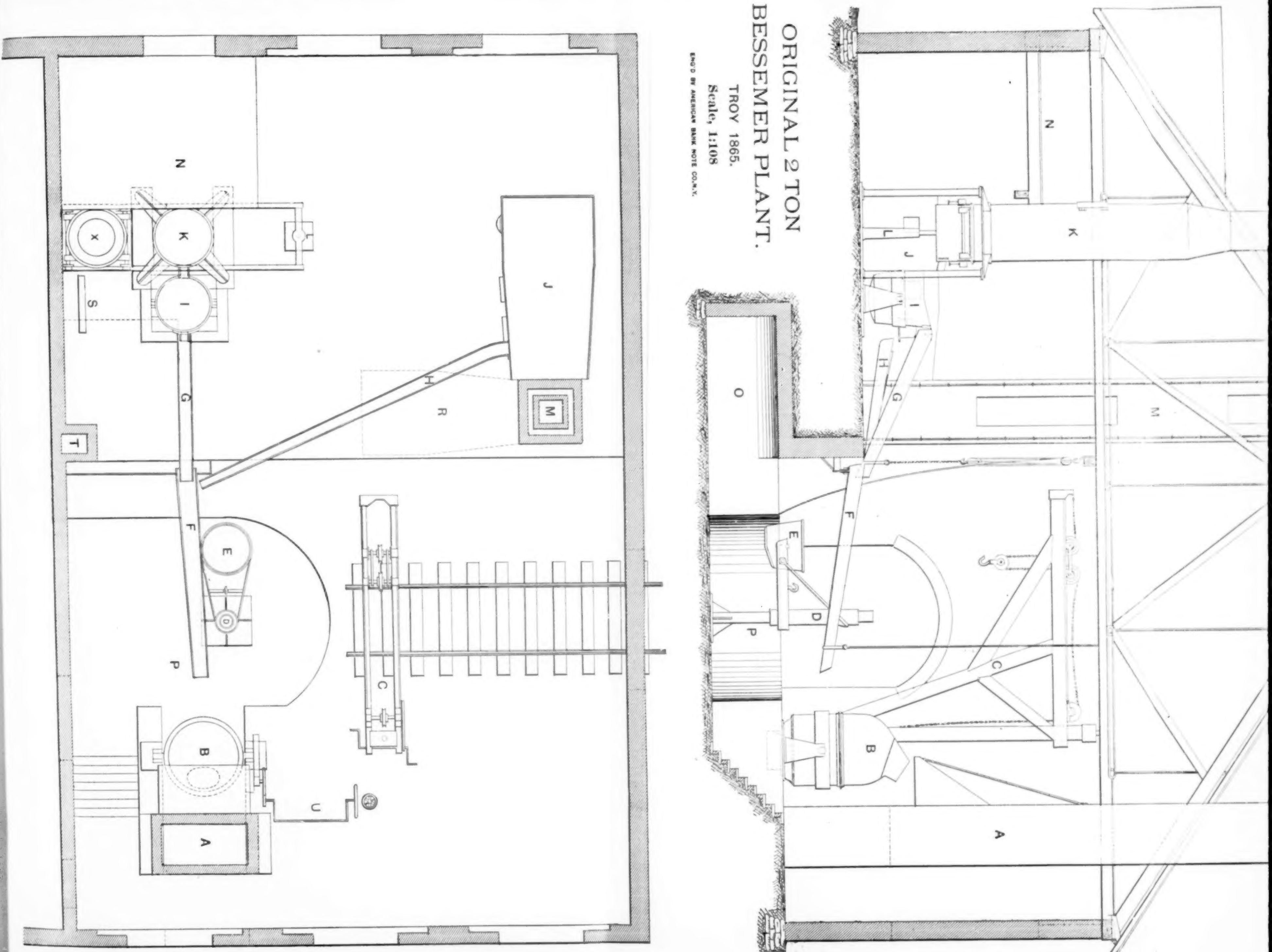
Two blowing cylinders, "48 x 48," were attached to this old wheel, and the rest of the plant placed in a building 64′ x 41′ 8″, built for the purpose. The pig iron was melted in the reverberatory furnace *J*, having a bed 7′ long by 4′ 9″ wide, from which it was

run through a gutter built in the floor to the wrought-iron runner *F*, and through it into the wrought-iron converter *B*, which was of course turned down to receive it. The runner *F* traveled on the rail *Y*, at its higher end and on a corresponding rail, to which its supporting rod was attached, at the lower or converter end. So that after the blow was finished it could be pushed over against the end wall of the building, and hence out of the way. *A* represents a brick stack with a brick hood, which carries off the flame of the conversions. The recarbonizing metal was melted in the furnace *R*, which is shown by the dotted lines and which worked into the stack *M*, in common with the furnace *J*. The resulting metal from the conversion was poured into the ladle *E*, which was supported by the cast-iron ram or crane *D*. This ram was controlled by attaching the chain of the wooden hand crane *C* to it at the hook *V*, and it was so swung over the moulds set in the pit *P*. These moulds and their ingots were subsequently taken from the pit by the crane *C*, and loaded on a car standing on the railroad track shown. The vessel was rotated by decidedly simple apparatus shown at *U*, which depended upon intelligent labor for its power.

Such was the original Troy two-ton plant. And it is not now surprising that many difficulties were encountered in its management. Mr. Holley's mind was soon impressed with the advantages of melting in a cupola and one was erected. This was built as shown by *K*. It was provided with duplicate bottom sections. The extra one being pushed to one side, as shown by *X*. He also advanced beyond the English method by placing the accumulating ladle *I*, resting on scales in front of the cupola. *G* is the wrought-iron runner conveying the metal to the runner *F*. When the cupola practice was adopted, the spiegel furnace *R* was torn down and the furnace *J* converted in one for melting the recarbonizer which was conveyed through the cast-iron gutter *H*. The cupola bottoms were raised and lowered by the screw *L*. *O* was an oven for drying stoppers, and *T* its chimney.*

As before stated, the first charge was made on February 16th, 1865, No. 2 Crown Point charcoal iron being used for the pig, and New Jersey Zinc Co.'s Franklinite for the recarbonizing metal. With this the record begins: One heat was blown, using 2,497 lbs. pig and 175 lbs. recarbonizer. From this there was cast three

* The removable converter bottom had not been invented, hence there was no bottom oven provided.



ORIGINAL 2 TON BESSEMER PLANT.

TROY 1865.

Scale, 1:108

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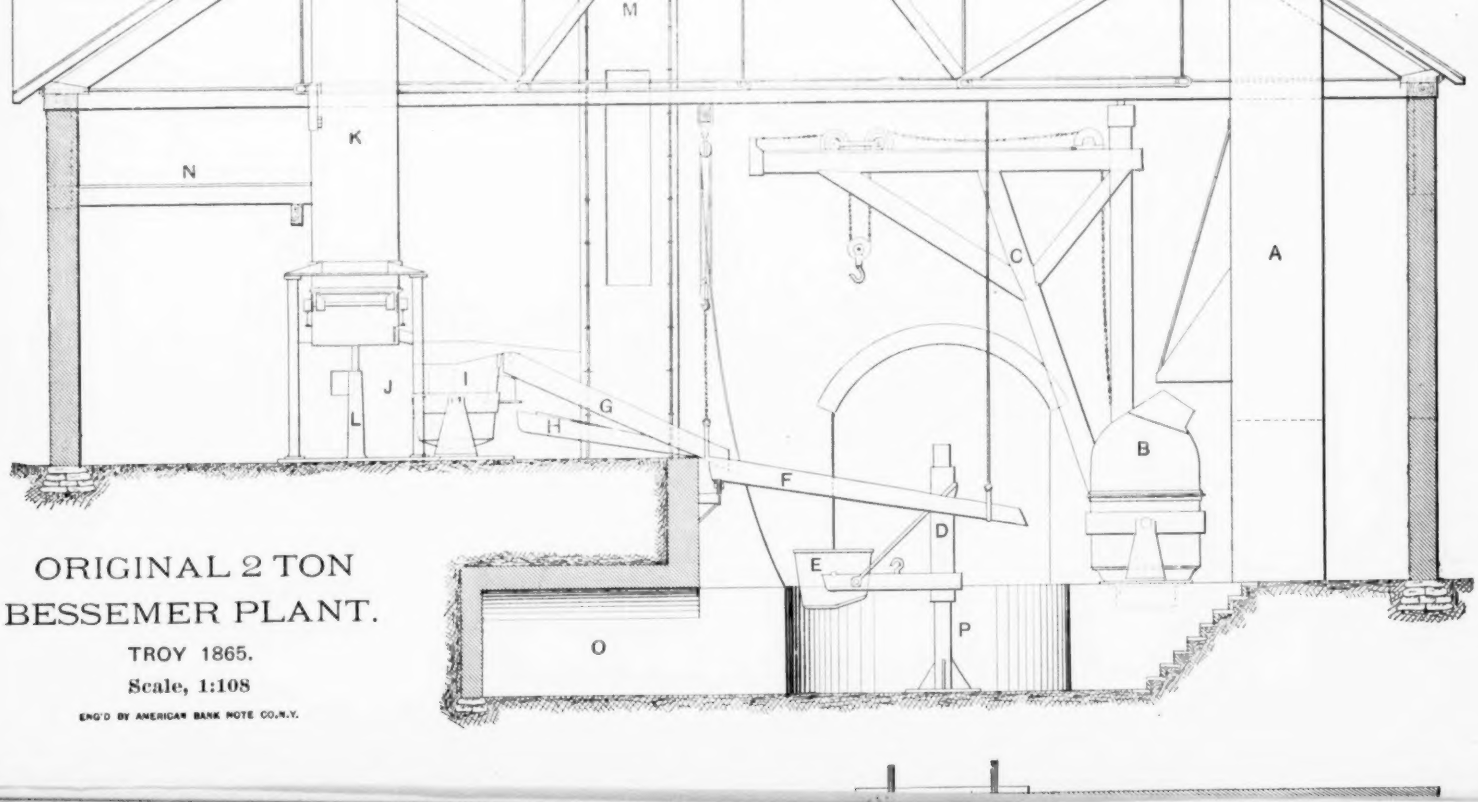

ORIGINAL 2 TON
BESSEMER PLANT.

TROY 1865.

Scale, 1:108

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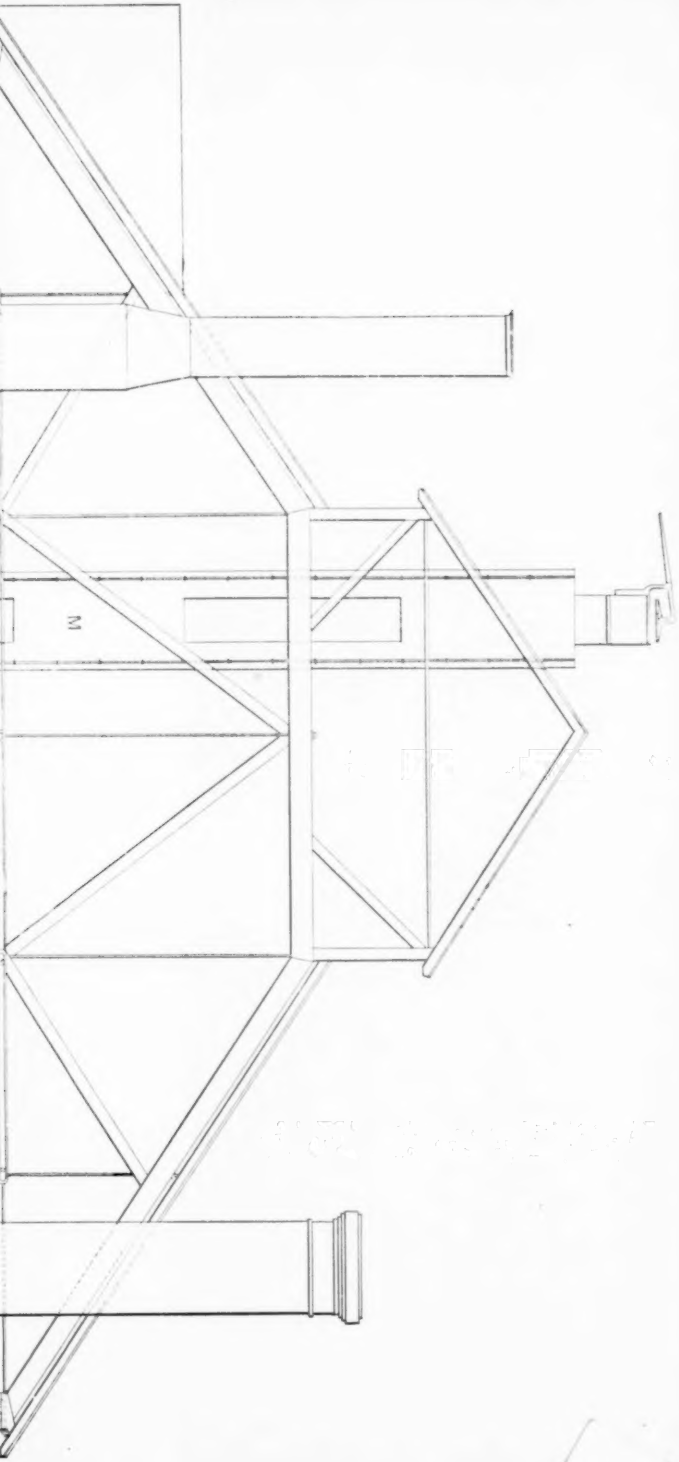

taper ingots weighing 482, 491 and 561 lbs. respectively, making 1,534 lbs. of castings and also 706 lbs. of scrap. Certainly a rather large percentage, and which, with steel rails at $27, we could hardly stand. But it was all figured out in the record, *i. e.*, "castings, 54.4%, scrap 26.4%, loss 16.2%. 1¼″ test piece bent double cold. Blast 5 to 9 lbs. Blew well and hammered as well as possible. Scrap mostly a large scull due to slow handling. Finer fracture than the charges made from the same brand of iron at Bessemer's works in England on November 30th, 1864. Welds pretty well, and hardens pretty well." This entry was evidently written in a hopeful spirit, and is in Alexander Holley's handwriting.

The next trial was made on February 27th, using the same grade of irons. But 50% of ingots were obtained; 29.8% scrap and 20.2% loss. The remarks being: "Blast 10 lbs. Blew 22 minutes. Vessel not hot enough. Ladle nozzle too small—1⅛″." On the following day the third trial from the same irons gave, "77% of ingots; 3.8% scrap and 19.2% loss. Vessel hot, and blew well with 8 to 10 lbs. of blast. Metal came through bottom by side of tuyere; stopped it with water. Steel all poured out of ladle. Nozzle 1⅞″ diameter." We can believe everybody went home happy that night.

So the record continues from day to day. Sometimes showing one heat to have been made, sometimes two, but always containing statements of greater or less difficulties encountered.

On April 4th, the product was increased to four heats. On April 27th, an ingot was made marked "Baldwin," with the following comment: "First tire ever made in America by this process. 'Bully Boy!'" On this same day a "Philadelphia delegation were present, and were much pleased."

Between May 17th and June 5th the following changes are noted as having been made. On the former date, the shaft of the water-wheel was found to be rotten, which necessitated a stop. During it the vessel was lengthened 18″, making it 10.6″ over all. "The pit was enlarged. New tuyeres and nozzles substituted, and the Franklinite furnace raised 6″, to get a better flow into the vessel."

On July 20th all was ready to try the new cupola. No. 1 Crown Point charcoal pig was used. "The iron melted in cupola in one hour after stopping tap hole—half the time blower making 88 revolutions, half 100 per minute; run out very hot from ladle. Coal consumed 1,226 lbs., iron melted and blown 2,997 lbs., result—ingots 85.4%, scrap 2%, loss 14.4%."

On July 25th, blows Nos. 132, 133 and 134 were made, and what was considered a great feat accomplished. It is thus told: "Melted in one hour after stopping tap hole, charged cupola in half-hour after first charge, it melted in three-quarters of an hour. These three charges were melted and converted into steel ingots in 3½ hours. Average loss of the three blows 20.6%."

I find it recorded, under date of July 28th, that the average consumption of coal in cupola was 1 lb. to 4.2 lbs. of iron melted.

About this time a small cupola was built and tried for melting the recarbonizer. I cannot find any drawing of it, and from the record of its failures I think it was soon abandoned, and the old reverberatory iron melting furnace changed into a spiegel furnace.

On November 25th, they made an ingot, cast in a loam mould of a shape suitable to be put on top of an iron rail pile to make a steel-headed rail.

The constant trouble from low-blast pressure led about this time to the abandonment of the water wheel. The last charge having been blown with it on December 8th. Work was resumed on March 10th, 1866, with blast from a steam engine. On the first day 11½ lbs. of blast is reported with 35 lbs. of steam pressure. On March 12th the pressure was good, but on the next heat the "engine worked badly, the fires low, and the vessel had to be turned down twice." However, from this on, the record is much cleaner. The product for the month of March is reported as:

Steel made................................	145,698 lbs. = 65 tons
Scrap made................................	5,390 "

I find that on November 13th, 1865, the experiment was made of using chromium ore in the vessel—the resulting metal was called "scrap." The experiment was repeated on the 22d. This time a triple compound of iron, chromium and carbon was used. The result remained the same.

On April 3d, 1866, the attempt was again made, and repeated on the 6th. While ingots were obtained which stood hammering, they cracked and crumbled badly. In those days "standing hammering" must have been somewhat different from our present idea. The 120 lbs. of chromium pig metal was heated to a red heat and thrown into the vessel when it was turned down after finishing blowing; it was turned up again for one quarter of a minute, and then the metal was poured into the casting ladle. This seems to have ended the chromium experiments.

An octagon ingot was cast on April 25th, 1866, which weighed 2,924 lbs. and was 15 inches in diameter. The mould was filled to within 12″ of the top, and an iron bar 5″ in diameter and 20″ long was lowered into the metal to the depth of 4″. This formed a handle for forging. Most probably this was the first large ingot made in this country. On the 26th another such ingot was made for a crank shaft forging.

May 2d, 1866, saw another experiment tried, by running 1,500 lbs. of metal into the converter, blowing it ten minutes, when it was entirely decarbonized. 3,000 lbs. more of pig was then run in and the vessel again turned up, the blast being on to mix the metal. It was then poured into the ladle and cast in the moulds. These are described as being "sixes and sevens."

80 charges were made in the month of May, yielding:

Ingots	81.5%
Scrap	1.8%
Loss	16.7%
Steel made	118 tons $\frac{1735}{2240}$.

The two-ton plant continued to run with increasing success. The patent difficulties were settled, and the firm commenced the erection of a two 5-ton converter plant. This was finished early in 1867. And I find from an old statement it was "confidently expected to produce from 20 to 30 tons of iron or steel ingots every turn of ten hours."

Before the final abandonment of the two-ton plant, Mr. Z. S. Durfee assumed charge of the works, Mr. Holley having severed his connection to finish building the Pennsylvania Steel Co's. plant at Harrisburgh, Pa., after seeing the 5-ton plant at Troy about ready for work. Among other changes, Mr. Durfee pulled down the spiegel furnace *J*, and put in a crucible or pot furnace, in which he melted his recarbonizer. It was then called ferro-manganese, and contained about 20% of manganese. This was melted in crucibles, and Mr. Durfee succeeded in producing some very good low carbon steel.

While we smile over these records of a past, that to some of us seems so long ago—yet in time is but as yesterday—let us realize what these trials meant to those conducting them. Let us not overlook their earnest endeavors, their high hopes, many disappointments, but never-failing courage. Strong faith was required, both by the capitalist and the engineer. Probably no industry ever

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made such gigantic strides, attained such advancement in the same number of years, as the Bessemer Process in America. But the fire that burnt away its crudities also consumed great spirits. The bold investor, E. B. Ward, the cultivated Z. S. Durfee, the perfect gentleman, the constant patriot, John A. Griswold, have passed away, while to those of us who have been in the thickest of the fight comes more closely the death of that daring engineer, always advancing, ever right, warm, uncompromising friend, George Fritz: and saddest of all, the loss of him whose hand recorded most of that which I have presented to you, records of the actual wearing away of his great heart. Applied science triumphed, but Alexander Lyman Holley died.

DISCUSSION.

Mr. Holloway.—I desire to express my thanks to the authors of both these papers for the manner in which they have been prepared and read before us, and for what they have told us about the early Bessemer steel works of this country. To those of us who are more or less familiar with it, it is even of greater interest, but over and beyond all, I think the presentation of such papers is of especial value, as transmitting to posterity the early history of one of the most important metallurgical processes which this country has ever seen. When the present generation shall have passed away, but for such a record as this in the Transactions of our Society, the memory would be lost and forgotten of the trials and disappointments which were undergone to bring this process to the perfection which it has now attained; and it is certainly due to those who took so active a part in this enterprise, that the record should be made an abiding one. There is no one we honor more highly than he who contributed so largely to its success at Troy, and it is quite fitting that in a society which remembers him with so much of pleasure and regard, there should be made a lasting record of the trials and discouragements which he encountered; and many of us realize what those disappointments must have been to him. Had it not been for those little injections of humor which crop out here and there, as we see, all through those dark days, and which are indicative of his hopeful spirit, I hardly know how Holley could have gone through it as he did. The experiments at Wyandotte, as related by Mr. Durfee, are quite a revelation to me, although living not far away from them, and I have listened to them with much interest. What he says awakens a

reminiscence of which I would like to speak. We had at the time in our vicinity one of the old-time furnace men, one who commenced his career years before with a small charcoal furnace, and who became afterward a well known and successful manager. He managed a furnace, as indeed all did in those days, without knowing much about it. In fact, a good furnace man was at that time looked upon as a sort of necromancer. His name was David Himrod, of Youngstown, and to him belongs, I believe, the credit of first using bituminous coal in a blast furnace with success. I was at that time engaged in the construction of the Bessemer steel plant for the Cleveland Rolling Mill Co., and, as Mr. Durfee says, the marvel of all about us was that so much money should be put into the machinery and apparatus. Meeting Mr. Himrod one day, who had heard of what was being done in Cleveland, and of the large sum of money that was being put into the new steel works there, he said to me: "It's all nonsense, this putting up such costly machinery just to make steel. It can all be done easily, simply, and much cheaper." Said I, "Have you ever investigated the matter to any extent?" "Oh, yes," he said; "I have gone through it in a simple way, just to satisfy myself that this great expense is all unnecessary." I replied that "it was certainly very strange that men should embark in an enterprise involving such large expenditures for machinery unless it was required." "It's all nonsense," he repeated; "all you want to do is to agitate the metal, and get the air into it; that's what changes it." "Well," I asked, "did you ever try any experiments in that direction?" "Yes, I did," said he. "I sent over to Homer Hamilton's and got one of his ladles, and filled it with iron from the furnace. I had made up my mind that all that was wanting was to agitate it a little, and get the air through it, and that would make it steel." "How did you manage to agitate it?" I asked. "Well," he said, "when I had filled up the ladle, I got a potato, and put it on the end of an iron rod, and stuck it into it." "Well, what did it do?" I asked. Throwing up both arms, he answered, "Oh, it made a hullabaloo!" The "Himrod process" for making steel, so far as I know, never came into general use.

Mr. Stirling.—Having been one of Mr. Holley's assistants in the early stage of his work, I thought that I ought not let this opportunity pass without making some statements. I went to Mr. Holley on the 4th of May, 1866. The two-ton plant was then running, and the building was up for the five-ton plant which was

being constructed. While matters were in this transition state, I remember working out for Mr. Holley a system of bottoms for the cupola which I have not seen illustrated anywhere. The plan will show Mr. Holley's mind in the direction of removable bottoms. Of course, the earlier plan of having the bottoms on a railway, running one out after the other, as described by Mr. Hunt, was somewhat inconvenient, for the reason that they could not pass each other. So Mr. Holley instructed me to work out a system by which we might have three bottoms on a turn-table, the idea being that one would come in after the other into place under the stack of the cupola. The center of the turn-table was located at one side of the cupola, and there was a system of braced construction below, the idea being that one bottom would swing right in after the other was used. The one that was finished and ready to use would go right into its place without having to pass the other. I remember, also, there was a turbine substituted for the overshot water-wheel, which was an improvement, though it was a mistake to work with water under the circumstances. I remember also having charge of making for Mr. Holley, models for the present Bessemer plant about September, 1866. The models were made up in the Troy machine shop.

Having been with Mr. Holley in the early stages of this process and having also visited the steel works of Bolckow, Vaughan & Co., at Middleboro', some two years ago, I can say that the contrast was very marked. While Mr. Holley made a few tons in a month—I think Mr. Hunt says sixty-five tons a month—the steel works at Middleboro' were turning out over twenty thousand tons in that time. At Troy there were no blast-furnaces in the neighborhood of the steel plant. The blast-furnaces were fifty or more miles from the steel works, while at Middleboro' the blast-furnaces were in the immediate vicinity, and they took the melted metal right to the converters and rolled the hot ingots right through.

I want to bear testimony, as one of Mr. Holley's assistants at that time, to his uniform kindness to the people that he employed; and sometimes I know he was kind under circumstances that would have irritated other men.

Mr. Barnes.—If I had known that these papers were to be read, I might possibly have arranged for the presentation of some additional matter which I think would have proven of interest.

I also was one of the early helpers in this Bessemer line, my first employment in the works at Troy having been in February,

1866. The only remark I have to make at this time is that I had to do with the copying of a lot of English tracings of machinery and fixtures that had been sent over from Mr. Bessemer's office; and it is an important fact that but very few of the features of the details of Mr. Bessemer's practice as thus given were embodied in the American work which was going on at that time and subsequently to it.

Mr. Hunt.—I had the fun of trying to make steel in that stationary converter which Mr. Durfee has shown, the object being to avoid patents on the Bessemer tumbling converter. I mention this from the fact of a little experience I had last week, which I wish to state informally. I am not prepared to say that it is going to be a constant thing, but I think it will be of interest to the metallurgists and engineers present. There are erected and in operation now at Pittsburgh two stationary converters owned by Oliver Bros. & Phillips. They are built under the Clapp-Griffith patents, and they have been producing some remarkably good low metal. I suppose you are all familiar with the character of the converters. The tuyeres are on the sides and close to the bottom. As the operator turns the main pressure of blast off, the blast enters behind a system of plugs and drives them into the tuyeres. These plugs have small apertures through their center, which remain open to keep the metal from chilling in the tuyeres. Hence the oxidization, while the heat is being tapped out, is reduced to the minimum. Another feature is the cinder tap. They have been making some most beautiful low metal, and it possesses an ever-constant property of welding. They have made some splendid specimens of boiler tubes that will stand all sorts of torture without showing failure. My special interest was to find out how much good the cinder tap did. So last week I started modestly by taking some iron that contained 0.9 phosphorus and using 50 per cent. of that and 50 per cent. of Bessemer iron. The analysis of the resulting metal was about this: 0.54 phosphorus, 0.0065 of silicon, .50 of manganese, 0.08 of sulphur, 0.12 of carbon. I expected, of course, that it would go to pieces in the rolls. It was cast into seven-inch ingots. They were rolled into a billet and that rolled into half-inch rods, and the physical test we received from the Pittsburgh testing laboratory was more astonishing than anything else. It showed 74,790 pounds tensile strength, 48.8 per cent. of reduction of area, 55,070 pounds elastic limit and 25¼ per cent. elongation. I thought that

must be an accident. So I made five more heats, but I have only received the report of one of the physical tests of these, and that was taken from the sprue of the ingots—the rough sprue from the bottom casting. It was taken and rolled into an half-inch rod, and that gave 76,760 pounds tensile strength, 55,580 pounds elastic limit, 28 per cent. elongation, and 51.4 per cent. reduction of area. In the language of Troy, which you will remember is always classical, I am at present "knocked out."

CLVI.

A NEW ROCK DRILL.

BY FREDERIC A. HALSEY, NEW YORK.

In the invention and design of this machine, it was the writer's object to obtain a better steam distribution than had before prevailed in machines of this class. The chief resulting differences between this machine and others are as follows:

I. In the machines in general use the motion of the piston is arrested at the conclusion of the return or inboard stroke, by a live-steam * cushion obtained by giving the valve a great degree of "lead." In this machine the piston is stopped (so far as is possible so to do) by an exhaust-steam cushion obtained by closing the exhaust port soon after the return stroke has commenced, and the steam thus compressed forms a portion of that used to effect the succeeding striking stroke.

II. In the machines in general use the steam is used without expansion. In this machine expansion is introduced to any desired extent.

III. The machines in general use strike a cushioned blow. This machine strikes an uncushioned blow.

The cushioned blow is a necessity with the valve gears heretofore usually employed—this necessity arising from the following circumstances: The length of stroke of a rock drill is not constant. As the drill hole progresses in depth, the cylinder must be correspondingly fed forward, but to effect this feed with perfect regularity is found to be an impossibility. The effect of this irregular feed of the cylinder is to vary the point marking the end of the stroke of the piston—the approach of the piston to the lower cylinder head varying from stroke to stroke. Moreover, in starting a hole, and under certain other circumstances, it is occasionally desirable to be able to feed the cylinder forward, so as to shorten the stroke still more than is actually necessary to accommodate the usual irregularity of feed. In brief, the machine must be able to

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* For the sake of brevity the word "steam" will be used throughout this paper to designate the driving medium. It will be understood that the devices described are equally adapted to use with compressed air.

take strokes of considerably less than normal length, without failure to trip its valve, in order to continue in uninterrupted action. This circumstance has usually been provided for by simply giving the valve a great degree of lead at the *lower* end of the cylinder,—tripping the valve at a point previously decided upon as the end of the shortest stroke to be allowed, and then submitting from necessity to the loss of power due to the cushion thus introduced into all strokes of usual length. In the machine about to be described, provision has been made for this irregular feed and length of stroke, but nevertheless, when full-length strokes are made, the valve does not move nor is steam admitted below the piston, until the actual delivery of the blow. There is a wide-spread impression that the cushioned blow has been introduced designedly, in order to prevent the piston from striking the lower cylinder head. This is an error. There is no rock drill now in successful use, that will not strike its lower head sharply if given the opportunity. The real cause for the cushioned blow is that given above—the necessity of providing for strokes of varying length. In one of its forms (the economizer described later) this machine strikes its head or the rock, as the case may be, with the same force as previous cushioned-blow machines of the same size, the difference being that in other machines the blow is the effect due to the difference between the driving steam behind the piston and the cushion steam in front of it, while in this machine the blow is the net effect of a smaller amount of driving steam.

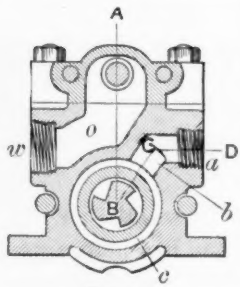

Fig.21.

Figures 17, 18, 19 and 20 are longitudinal sections taken on the broken line *ABCD* of Figure 21, the piston and valve being shown in a number of successive positions. Figure 21 is a cross section on the line *EF* of Figure 17.

In Figure 17 the piston has just completed its striking stroke and is ready to commence its return stroke. The steam which effected the preceding striking stroke has been exhausted through the opening *h*, which forms the only exhaust port for the upper or left-hand end of the cylinder. Steam enters at the supply nozzle *a*, flows through the longitudinal groove *b* * in the cylinder (seen also in

* The longitudinal groove *b* is of such length as to maintain constant communication between the nozzle *a* and the circumferential groove *c*. Its office is to

Fig.17.

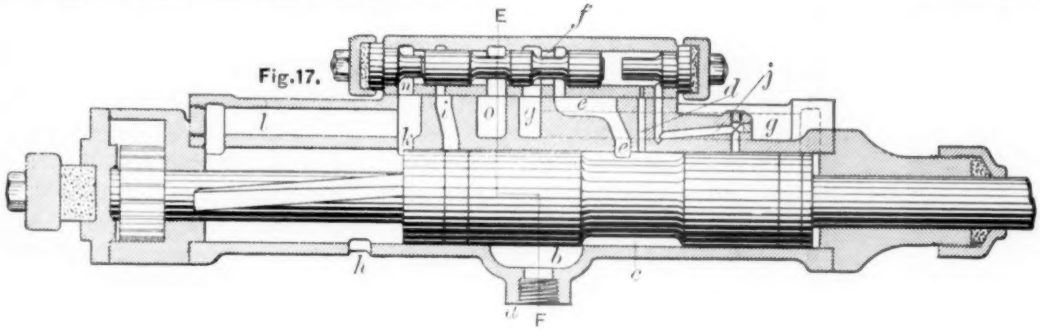

Fig.18.

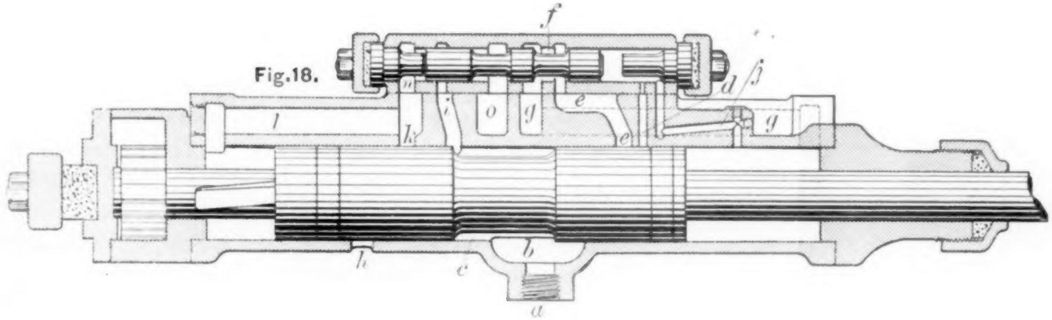

Fig.19.

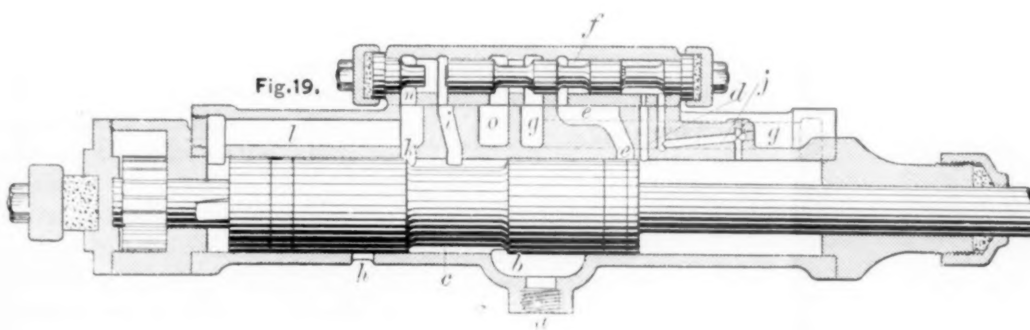

Fig.20.

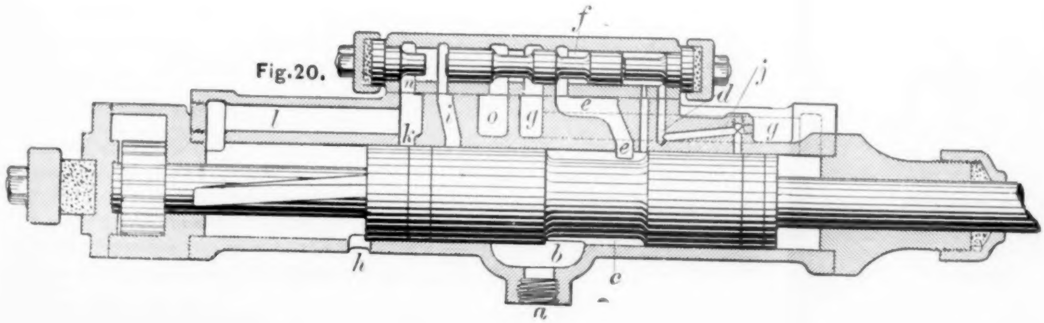

Figure 21), to the broad, shallow circumferential groove *c* in the piston. This circumferential groove *c* forms, in effect, the steam chest of the machine, and from it the steam is distributed alternately to the opposite ends of the cylinder. Through the passage *d* steam pressure is maintained in the lower end of the valve chest, firmly holding the valve in the position shown. Steam flows through the passage *ee*, and from this through the neck *f* of the valve, to the passage *gg*, which in turn leads it to the lower end of the cylinder. The piston now starts upward, and presently takes the position shown in Figure 18. In passing from the position of Figure 17 to that of Figure 18, the piston has closed the ports *d*, *e*, *h*, and opened *i*, *j*. Closing *h* confines the exhaust steam in the upper end of the cylinder, forming an exhaust cushion before the piston, and accomplishes the first improvement named above. Closing *d* merely isolates the steam already in the end of the valve chest. Closing *e* cuts off the supply of steam to the lower end of the cylinder, and for that end effects the second improvement aimed at. Opening *i* has no effect, as its upper end is still closed by the valve. Opening *j* establishes communication between the lower ends of cylinder and valve chest, and hence as expansion goes on from the cut-off, the pressure acting on the end of the *valve* will gradually fall. In Figure 19 the piston has ascended still further, and uncovered the port *k*, admitting steam through the passages *l* and *n* respectively, to the upper end of the cylinder and valve chest. The former completes the work of stopping the motion of the piston; the latter, being opposed only by expanded steam at the lower end of the valve, as just explained, shifts the valve downward, thus establishing communication between the port *gg* and the exhaust passage *o*. The piston now commences its descent, and closes and opens the various ports in the reverse order to that just explained. Closing *k* has no effect, as *i* being now open, the steam can pass through it to the upper end of the cylinder. Closing *i* effects the cut-off for the upper end of the cylinder, exactly as closing *e* did for the lower end. Opening *e* has no effect, its upper end being now closed by the valve. Opening *h* effects the exhaust.* In Figure 20 the piston has just uncovered the port

lessen the otherwise inconvenient length of the circumferential groove *c*. This in turn diminishes the length of piston and cylinder, and hence weight of machine.

* In the actual machine a covered passage leads the exhaust steam from the port *h* to the passage *o*, so that the exhaust from the two ends of the cylinder escapes to the air through a single outlet, *w*, of Figure 21.

d leading to the lower end of the valve chest, and it has thus established the condition which will reverse the valve, and insure the next upward stroke. As the port *d* is *just* uncovered, and no more, the piston is at the point marking the termination of its shortest working stroke. Should the piston stop short of the position shown (by reason of excessive feed), the port *d* would not be uncovered, the valve would not reverse, and the machine would stop. As will be seen, the piston is at some distance from the lower cylinder head, this distance representing the latitude of irregularity permitted in the feed. The piston may stop anywhere between the end of the cylinder and the position of Figure 20, and the action will continue. In order to effect the third improvement (the uncushioned blow), it is necessary to provide an arrangement, which, notwithstanding the passage *d* is always opened at the position shown in Figure 20, shall yet, when full-length strokes are made, permit the piston to pass on and complete its stroke without the movement of the valve actually taking place until the delivery of the blow. This is effected by simply constricting a portion of this passage *d*—making it of such small size that the passage through it of the steam necessary to move the valve, shall be delayed until the piston has had time to pass on and complete its stroke. In the machine as actually made, most of the ports opening into the cylinder are arranged in pairs, and diametrically opposite one another, to obviate side pressure on the piston.

Figures 22 and 23 are indicator diagrams* photographically reproduced from the original pencil lines, and being taken at working pressure, with wide-open throttle, unrestricted speed, and full-length stroke, illustrate the action of the machine. Figure 22 is from the upper end, representing the striking stroke, and Figure 23 from the lower end, representing the return stroke. At *p*, Figure 22, the piston is in the position of Figure 17. At *q* the exhaust port *h* is closed and compression begins; at *r* the port *k* is opened, full-pressure steam enters, stops the piston at *s*, and reverses the valve; at *t* the port *i* is closed and expansion begins; at *u* the port *h* is opened and exhaust takes place. At the lower end of the cylinder there is no gradual rise of pressure like that from *q* to *r* of Figure 22. At this end the rise of pressure is practically instantaneous, and the result is the undulations of Figure 23. While, however, the upper side of Figure 23 is about valueless, the lower side renders

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* Taken with the machine operated by compressed air.

clear the action which it is desired to show. As stated, the machine was running its full stroke—as near to its lower head as was considered safe—nevertheless, there is no lead whatever shown. At *v* the exhaust from the upper end of the cylinder occurs, and the crossing of the two exhausts produces the flutter shown. The port *d* is also opened at *v*, but it is clear that steam is not admitted until the end of the stroke is reached.

It will be observed that the point of cut-off depends upon the position of the ports *e*, *i*, lengthways in the cylinder, and can be varied

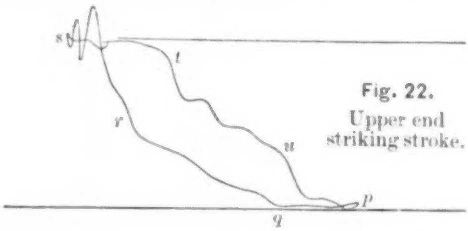

Fig. 22.
Upper end
striking stroke.

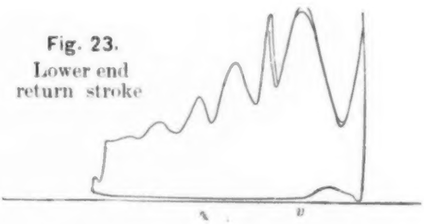

Fig. 23.
Lower end
return stroke

INDICATOR DIAGRAMS FROM THE ECONOMIZER ROCK DRILL.

Air pressure 55 lbs. (gauge pressure.)
Throttle valve wide open.
Speed approximately 400 blows per minute.
Scale of spring 60 lbs. per inch.
CROSBY INDICATOR.

at will in the design and in the two ends of the cylinder independently. The effect of the cut-off on the striking stroke is to diminish the force of the blow, while the effect of the absence of cushion is to increase it. The former may be adjusted to the latter so that the blow struck is precisely the same as in cushioned-blow machines, but of course obtained with a smaller consumption of steam. On the other hand, a late cut-off may be employed on the striking stroke, thus giving the full effect of the uncushioned blow to increased power. It is freely recognized that fuel is but one of many items of expense, and that in many situations speed of execution far out-

weighs any economy in fuel that might be realized through the use of the expansion principle. To meet both situations—those where economy and capacity, respectively, are leading objects, two classes of machines are being made—one having cut-off on both strokes, and the other on the up-stroke only. The first machine is named the "Economizer," and the second the "Slugger," and either is furnished as the situation requires.

CLVII.

A NOVEL HAMMER-HEAD AND DIE.

BY WM. HEWITT, TRENTON, N. J.

In the ordinary construction of steam hammers the head and die are usually secured to the piston-rod, which is slightly tapering at the end, by means of keys applied to a slot in the connection

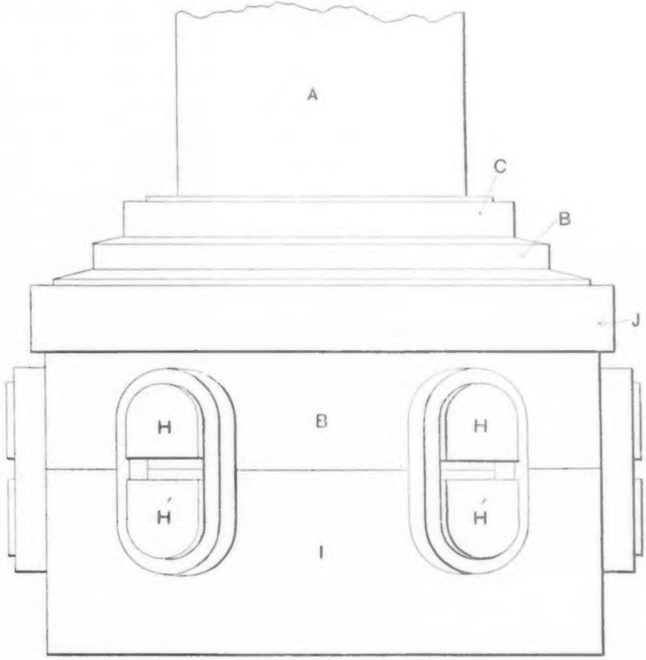

Fig. 24

between the piston-rod and head, and a dovetail joint in the connection between the head and die. The objection to this and all other modes of connection by means of keys is, that the unequal expansion and contraction of both the head and die continually knocks the keys loose—no matter how tight they may be driven—necessitating frequent stoppages to tighten up, which are sufficiently annoying in themselves even if they do not lead to more serious

consequences, such as the bursting of a head or cracking of a die; and although these parts are generally made of cast steel, such events are not of uncommon occurrence, especially in shingling, where foul blows are frequent.

The device which I present to your notice in this paper was applied a few years ago to a Sellers hammer in the works of the Trenton Iron Co., and has effectually overcome the difficulties mentioned, being a much cheaper as well as better arrangement.

Referring to the accompanying sketches (Figs. 24 and 25), *A* is the piston-rod; *B*, the head secured to the lower extremity of the

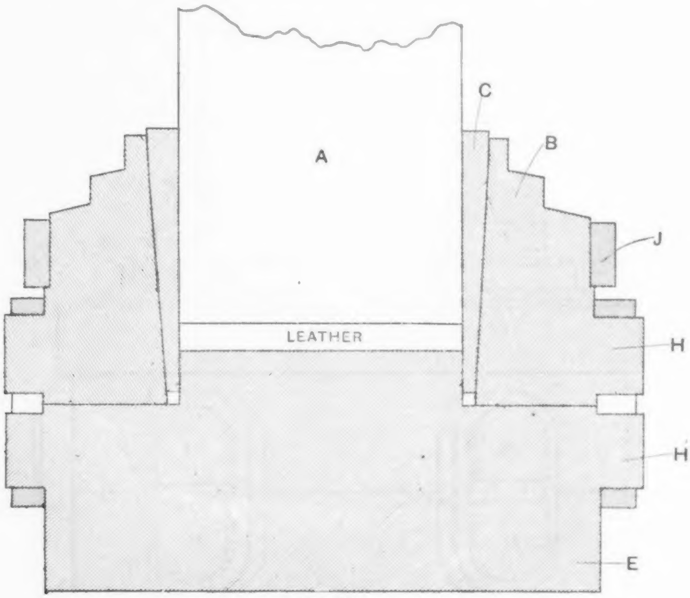

Fig. 25

piston-rod by means of a tapering split ring, *C*, and a circumscribing wrought-iron band, *J*. *E* is the die. *H*, *H* are a series of lugs projecting radially from the exterior face of the lowermost portion of the head, and *H'*, *H'* are a corresponding series of lugs projecting similarly from the exterior face of the uppermost portion of the die. The two series of lugs in the set of the parts are so disposed as to be vertically aligned in corresponding pairs.

The parts are joined in the following manner:—The wrought-iron band *J*, is first shrunk upon the head *B*, to keep it from fracturing, and the head, with the band shrunk upon it, is then heated

to a red heat. The tapering sleeve or ring *C*, which is split like a piston-ring, is now slipped over the end of the piston-rod; the head *B* while red-hot placed around this; and the die placed so that the lugs on it come directly opposite those on the head. The tapering ring *C* is then driven down tightly with a sledge; the head left to cool; and finally, the bands shrunk around the lugs, as shown in the sketches. The opposing faces of the lugs are faced off or beveled so that they do not touch in the contact of the die with the head in order to provide against the possibility of their receiving or sustaining any portion of the shock from the blows of the hammer, which otherwise would break or destroy them. The bands around the lugs in shrinking are of course subjected to an enormous tension, which retains them fixedly in position and holds the die immovably in place, until it is worn out, when it is removed by cutting the bands and replaced by a new die. The head *B* on cooling shrinks sufficiently, as practice has proved, to prevent the tapering ring *C* from slipping or shaking loose. A leather washer is placed between the end of the piston-rod and die, to provide for a slight amount of elasticity and ameliorate somewhat the force of the severe blows. The tendency of the blows, it is obvious, is to drive the tapering ring in tighter, and the force thus exerted outwardly against the head is so great that it would soon break it if it were not for the wrought-iron band *J*, which effectually prevents this. The head and die are made of cast iron.

DISCUSSION.

Mr. Hutton.—As Mr. Hewitt is not present to speak of them himself, I might mention one or two points which interested me when Mr. Hewitt was first showing me this arrangement of his. In the first place, the combined action of the heat and the blows on the leather packing between the end of the piston-rod and the top of the die is such that in the course of a few months, or whenever any inspection of the thing is necessary, the leather is found to have become charcoal. It is a fine pulverulent packed mass in that cavity. Mr. Hewitt also says that the life of this cast-iron head is from twelve to fourteen months of constant use, and shingling say about 10,000 tons of iron.

President Sweet.—It occurs to me, in looking over the drawings, that there is one piece more than is necessary. If the rod is tapered, the conical ring can be dispensed with, making the taper-

ing fit on the rod. It is not necessary to make any allowance for shrinkage. Simply put it in place, and let it shrink on.

Mr. Davis.—The taper-ended piston-rod would force itself down to its full extent every time, by the blows and the amount of momentum in the rod and piston. It would probably upset itself to such an extent as to loosen itself or split the hammer-head. This taper ring is very similar to the ordinary Sellers' method. The point about it that makes it such a good connection is that that ring is left a little above its seat, and, as the hammer strikes, the momentum of the ring itself drives it toward home. The ring is not heavy enough to do any damage; whereas the great weight of piston and rod pounding on a taper would burst the hammer-head or destroy the fit. I would like to ask how large the hammer is to which this was applied, because in our shops we have used hammers up to six hundred [and fifty pounds with the ordinary Sellers' connection constantly during the last five and a half years. One is a hammer that serves about twenty-five smiths' forges. The same head is in use to-day, and apparently as good as the day it was made. That tapering ring seems to answer every practical purpose. The piston-rod in every case comes right down solid against the hammer-head, and does not seem to upset itself at all from the usage. Our Sellers hammer-heads are easily removed at any time by warming them, and may be secured firmly on the piston-rod again by simply driving the taper ring in its place.

CLVIII.

TABLE OF SIZES OF CHIMNEYS.

BY WILLIAM KENT, M. E., NEW YORK.

THE accompanying table of sizes of chimneys for various horse-powers of boilers is based on the following data:

1. The draught power of the chimney varies as the square root of the height.

2. The retarding of the ascending gases by friction may be considered as equivalent to a diminution of the area of the chimney, or to a lining of the chimney by a layer of gas which has no velocity. The thickness of this lining is assumed to be two inches for all chimneys, or the diminution of area equal to the perimeter × two inches (neglecting the overlapping of the corners of the lining). Expressed algebraically, let D = diameter, A = area, E = effective area.

$$\text{For square chimneys, } E = D^2 - \frac{8D}{12} = A - \frac{2\sqrt{A}}{3}.$$

$$\text{For round chimneys, } E = \pi\left(D^2 - \frac{8D}{12}\right) = A - 0.592\sqrt{A}$$

For simplifying calculations, the coefficient of $\sqrt{A}$ may be taken as 0.6 for both square and round chimneys, and the formula becomes

$$E = A - 0.6\sqrt{A}.$$

3. The power varies directly as this effective area E.

4. A chimney 80′ high, 42″ diameter, has been found to be sufficient to cause a rate of combustion of 120 pounds of coal per hour per square foot of area of chimney, or, if the grate area is to the chimney area as 8 to 1, a combustion of 15 pounds of coal per square foot of grate per hour. This is fair practice for a boiler of modern type, in which flues, or tubes, are of moderate diameter, gas passages circuitous, and heating surface extensive in proportion to rate of combustion, so as to cool the chimney gases to 400° or 500°, and produce high economy.

5. A chimney should be proportioned so as to be capable of

giving sufficient draught to cause the boiler to develop much more than its rated power, in case of emergencies, or to cause the combustion of 5 pounds of fuel per rated horse-power of boiler per hour.

Conditions 4 and 5 being assumed, the 80′ x 42″ chimney, 9.62 square feet area, will cause the combustion of 9.62 x 120 = 1154.4 pounds of coal per hour, or at 5 pounds of coal per horse-power per hour, is rightly proportioned for 231 horse-power of boilers.

The power of the chimney varying directly as the effective area, E, and as the square root of the height, h, the formula for horse-power of boiler for a given size of chimney will take the form,—

$$\text{HP.} = CE\sqrt{h}, \text{ in which } C \text{ is a constant.}$$

For the 80′ x 42″ chimney, $E = A - 0.6\sqrt{A} = 7.76$ square feet.

$$\sqrt{h} = 8.944 \text{ feet.}$$

Substituting these values in the formula it becomes,—

$$231 = C \times 7.76 \times 8.944,$$
$$\text{whence } C = 3.33,$$

and the formula for horse-power is

$$\text{HP.} = 3.33\,E\sqrt{h}, \text{ or, HP.} = 3.33\,(A - .6\sqrt{A})\sqrt{h}.$$

If the horse-power of boiler is given, to find the size of chimney, the height being assumed,

$$E = \frac{0.3\text{ HP.}}{\sqrt{h}}$$

For round chimneys,—Diameter of chimney = Diam. of E + 4″.

For square chimneys, Side of chimney = $\sqrt{E}$ + 4″.

In the formulæ and table no account has been taken of the difference which is believed by some authorities to exist in the efficiencies of round and square chimneys of equal area, nor of the differences of friction and of rate of cooling of the gases in iron and in brick chimneys. Should experimental data of these differences, or of the effect of infiltration of air into brick chimneys, be obtained in future, the formulæ and table may be corrected accordingly.

Sizes of Chimneys for Steam Boilers.

Formula, HP. $= 3.33\,(A - 0.6\sqrt{A})\sqrt{h}$.

Diam. inches.	Area A. sq. ft.	Effective Area. $E = A - 0.6\sqrt{A}$. sq. ft.	Height of Chimney. 50 ft.	60 ft.	70 ft.	80 ft.	90 ft.	100 ft.	110 ft.	125 ft.	150 ft.	175 ft.	200 ft.	Equivalent Square Chimney. Side of square $\sqrt{E} + 4$ inches.
			Commercial Horse Power of Boiler.											
18	1.77	.97	23	25	27	...	...	...	....	....	....	....	....	16
21	2.41	1.47	35	38	41	...	...	...	....	....	....	....	....	19
24	3.14	2.08	49	54	58	62	...	...	....	....	....	....	....	22
27	3.98	2.78	65	72	78	83	...	...	....	....	....	....	....	24
30	4.91	3.58	84	92	100	107	113	...	....	....	....	....	....	27
33	5.94	4.48	..	115	125	133	141	...	....	....	....	....	....	30
36	7.07	5.47	..	141	152	163	173	182	....	....	....	....	....	32
39	8.30	6.57	..	...	183	196	208	219	....	....	....	....	....	35
42	9.62	7.76	..	...	216	231	245	258	271	....	....	....	....	38
48	12.57	10.44	..	...	...	311	330	348	365	389	....	....	....	43
54	15.90	13.51	..	...	...	...	427	449	472	503	551	....	....	48
60	19.64	16.98	..	...	...	...	536	565	593	632	692	748	....	54
66	23.76	20.83	..	...	...	...	...	694	728	776	849	918	981	59
72	28.27	25.08	..	...	...	...	...	835	876	934	1023	1105	1181	64
78	33.18	29.73	..	...	...	...	...	...	1038	1107	1212	1310	1400	70
84	38.48	34.76	..	...	...	...	...	...	1214	1294	1418	1531	1637	75
90	44.18	40.19	..	...	...	...	...	...	....	1496	1639	1770	1893	80
96	50.27	46.01	..	...	...	...	...	...	....	....	1876	2027	2167	86

CLIX.

TABLES FOR FACILITATING CALCULATIONS OF BOILER TESTS.

BY WILLIAM KENT, M. E., NEW YORK.

THE calculation of the results of a series of boiler tests involves a considerable amount of figuring and use of formulæ, and unless an engineer is very expert in arithmetical calculations, the correction of mistakes in calculation is often one of the most annoying parts of his work. The table of factors of evaporation for the purpose of reducing evaporation under actual conditions to equivalent evaporation from and at 212° herewith presented has been found of considerable service in the writer's practice, both in saving the labor of original calculation and in lessening the chances of errors in figures. It is much more complete than the table in the third volume of the American edition of Wiesbach, or the similar table in Appleton's Encyclopedia of Mechanics. The table was first calculated by a system of continuous additions and corrections to the sixth decimal place. Whenever the last two figures were between 47 and 53 inclusive, the calculation was revised for the purpose of determining whether or not in making a table of four places unity should be added to the fourth figure. The maximum error of any figure in the table is believed not to exceed .000052.* The table gives the factors for every 3° of temperature of feed water from 32° to 212° F., and for every two pounds pressure of steam within the limits of ordinary working steam pressures.

The difference in the factor corresponding to a difference of 3° temperature of feed is always either .0031 or .0032. For interpolation to find a factor for a feed-water temperature between 32° and 212°, not given in the table, take the factor for the nearest temperature and add or subtract, as the case may be, .0010 if the difference is .0031, and .0011 if the difference is .0032. As in nearly all cases a factor of evaporation to three decimal places is accu-

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* On the supposition that the steam tables used in making the tables are correct, and that 965.7 thermal units is the correct value of the latent heat of evaporation at 212° F. If 966.1, the value given by some authorities, had been used in the calculations, the factors would be .0004 or .0005 less than those in the table.

rate enough, any error which may be made in the fourth decimal place by interpolation is of no practical importance.

The tables used in calculating these factors of evaporation are those given in Charles T. Porter's Treatise on the Richards Steam Engine Indicator, and the formula is the well known one, Factor $= \frac{H-h}{965.7}$, in which H is the total heat of steam at the observed pressure, and h the total heat of feed water of the observed temperature.

In addition to the tables of factors of evaporation there are given tables of temperature and total heat units, reckoned from 32° F., of water and steam from 0 to 250 lbs. gauge-pressure, and of water from 32° to 212° F., all condensed from Mr. Porter's tables. Also a column showing weight of water per cubic foot according to Rankine's formula, as given in D. K. Clark's Rules, Tables and Data, but corrected for apparent errors, and interpolated for the degrees of temperature not given by Clark. As there is considerable difference in figures in the second decimal place of weights of water given by different authors, it is considered unnecessary to put figures beyond the second decimal place in the table, although the third decimal place was used in making the interpolations.

FACTORS OF EVAPORATION.

Feed wat'r tempera-ture.	Lbs. Gauge pressures ..0+ Absolute pressures 15	10+ 25	20+ 35	30+ 45	40+ 55	45+ 60	50+ 65	52+ 67	54+ 69	56+ 71
	Factors of Evaporation.									
212 F	1.0003	1.0088	1.0149	1.0197	1.0237	1.0254	1.0271	1.0277	1.0283	1.0290
209	35	1.0120	80	1.0228	68	86	1.0302	1.0309	1.0315	1.0321
206	66	51	1.0212	60	99	1.0317	34	40	46	52
203	98	83	43	91	1.0331	49	65	72	78	84
200	1.0129	1.0214	75	1.0323	62	80	97	1.0403	1.0409	1.0415
197	60	46	1.0306	54	94	1.0412	1.0428	34	41	47
194	92	77	38	85	1.0425	43	60	66	72	78
191	1.0223	1.0308	69	1.0417	57	74	91	97	1.0503	1.0510
188	55	40	1.0400	48	88	1.0506	1.0522	1.0528	35	41
185	86	71	32	80	1.0519	37	54	60	66	72
182	1.0317	1.0403	63	1.0511	51	68	85	91	98	1.0604
179	49	34	95	42	82	1.0600	1.0616	1.0623	1.0629	35
176	80	65	1.0526	74	1.0613	31	48	54	60	66
173	1.0411	97	57	1.0605	45	63	79	85	92	98
170	43	1.0528	89	36	76	94	1.0710	1.0717	1.0723	1.0729
167	74	59	1.0620	68	1.0707	1.0725	42	48	54	60
164	1.0505	91	51	99	39	56	73	80	86	92
161	37	1.0622	82	1.0730	70	88	1.0804	1.0811	1.0817	1.0823
158	68	53	1.0714	62	1.0801	1.0819	36	42	48	54
155	99	84	45	93	33	50	67	73	80	86
152	1.0631	1.0716	76	1.0824	64	82	98	1.0905	1.0911	1.0917
149	62	47	1.0808	55	95	1.0913	1.0930	36	42	48
146	93	78	39	87	1.0926	44	61	67	73	79
143	1.0724	1.0810	70	1.0918	58	75	92	98	1.1005	1.1011
140	56	41	1.0901	49	89	1.1007	1.1023	1.1030	36	42
137	87	72	33	80	1.1020	38	55	61	67	73
134	1.0818	1.0903	64	1.1012	51	69	86	92	98	1.1104
131	49	34	95	43	83	1.1100	1.1117	1 1123	1.1130	36
128	81	66	1.1026	74	1.1114	32	48	55	61	67
125	1.0912	97	57	1.1105	45	63	79	86	92	98
122	43	1.1028	89	36	76	94	1.1211	1.1217	1.1223	1.1229
119	74	59	1.1120	68	1.1207	1.1225	42	48	54	60
116	1.1005	90	51	99	39	56	73	79	86	92
113	36	1.1122	82	1.1230	70	88	1.1304	1.1310	1.1317	1.1323
110	68	53	1.1213	61	1.1301	1.1319	35	42	48	54
107	99	84	45	92	32	50	66	73	79	85
104	1.1130	1.1215	76	1.1323	63	81	98	1.1404	1.1410	1 1416
101	61	46	1.1307	55	94	1.1412	1.1429	35	41	47
98	92	77	38	86	1.1426	43	60	66	73	79
95	1.1223	1.1309	69	1.1417	57	75	91	97	1.1504	1.1510
92	55	40	1.1400	48	88	1.1506	1.1522	1.1529	35	41
89	86	71	31	79	1.1519	37	53	60	66	72
86	1.1317	1.1402	63	1.1510	50	68	84	91	97	1.1603
83	48	33	94	41	81	99	1.1616	1.1622	1.1628	34
80	79	64	1.1525	73	1.1612	1.1630	47	53	59	65
77	1.1410	95	56	1.1604	44	61	78	84	90	96
74	41	1.1526	87	35	75	92	1.1709	1.1715	1.1722	1.1728
71	72	58	1.1618	66	1.1706	1.1723	40	46	53	59
68	1.1504	89	49	97	37	55	71	78	84	90
65	35	1.1620	80	1.1728	68	86	1.1802	1.1809	1.1815	1.1821
62	66	51	1 1711	59	99	1.1817	33	40	46	52
59	97	82	43	90	1.1830	48	64	71	77	83
56	1.1628	1.1713	74	1.1821	61	79	96	1.1902	1.1908	1.1914
53	59	44	1.1805	52	92	1.1910	1.1927	33	39	45
50	90	75	36	84	1.1923	41	58	64	70	76
47	1.1721	1.1806	67	1.1915	54	72	89	95	1.2001	1.2007
44	52	37	98	46	86	1.2003	1.2020	1.2026	32	39
41	83	68	1.1929	77	1.2017	34	51	57	64	70
38	1.1814	1.1900	60	1.2008	48	65	82	88	95	1.2101
35	45	31	91	39	79	96	1.2113	1.2119	1.2126	32
32	76	62	1.2022	70	1.2110	1.2128	44	51	57	63

FACTORS OF EVAPORATION.

Lbs. Gauge pressures.. 58+ Absolute pressures. 73	60+ 75	62+ 77	64+ 79	66+ 81	68+ 83	70+ 85	72+ 87	74+ 89	76+ 91
Feedwat'r tempera-ture.	Factors of Evaporation.								
212° F	1.0295	1.0301	1.0307	1.0312	1.0318	1.0323	1.0329	1.0334	1.0339	1.0344
209	1.0327	33	38	44	49	55	60	65	70	75
206	58	64	70	75	81	86	91	97	1.0402	1.0407
203	90	96	1.0401	1.0407	1.0412	1.0418	1.0423	1.0428	33	38
200	1.0421	1.0427	33	38	44	49	54	59	65	69
197	53	58	64	70	75	80	86	91	96	1.0501
194	84	90	96	1.0501	1.0507	1.0512	1.0517	1.0522	1.0527	32
191	1.0515	1.0521	1.0527	33	38	43	49	54	59	64
188	47	53	58	64	69	75	80	85	90	95
185	78	84	90	95	1.0601	1.0606	1.0611	1.0616	1.0622	1.0626
182	1.0610	1.0615	1.0621	1.0627	32	37	43	48	53	58
179	41	47	52	58	63	69	74	79	84	89
176	72	78	84	89	95	1.0700	1.0705	1.0711	1.0716	1.0721
173	1.0704	1.0709	1.0715	1.0721	1.0726	32	37	42	47	52
170	35	41	46	52	57	63	68	73	78	83
167	66	72	78	83	89	94	99	1.0805	1.0810	1.0815
164	98	1.0803	1.0809	1.0815	1.0820	1.0825	1.0831	36	41	46
161	1.0829	35	40	46	51	57	62	67	72	77
158	60	66	72	77	83	88	93	98	1.0904	1.0908
155	92	97	1.0903	1.0909	1.0914	1.0919	1.0925	1.0930	35	40
152	1.0923	1.0929	34	40	45	51	56	61	66	71
149	54	60	66	71	77	82	87	92	97	1.1002
146	85	91	97	1.1002	1.1008	1.1013	1.1018	1.1024	1.1029	34
143	1.1017	1.1022	1.1028	34	39	44	50	55	60	65
140	48	54	59	65	70	76	81	86	91	96
137	79	85	91	96	1.1102	1.1107	1 1112	1.1117	1.1122	1.1127
134	1.1110	1.1116	1.1122	1.1127	33	38	43	49	54	59
131	42	47	53	59	64	69	75	80	85	90
128	73	79	84	90	95	1.1201	1.1206	1 1211	1.1216	1.1221
125	1.1204	1.1210	1.1215	1.1221	1.1226	32	37	42	47	52
122	35	41	47	52	58	63	68	73	78	83
119	66	72	78	83	89	94	99	1.1305	1.1310	1.1315
116	98	1.1303	1.1309	1.1315	1.1320	1.1325	1.1331	36	41	46
113	1 1329	34	40	46	51	57	62	67	72	77
110	60	66	71	77	82	88	93	98	1.1403	1.1408
107	91	97	1.1403	1.1408	1.1414	1.1419	1.1424	1.1429	34	39
104	1.1422	1.1428	34	39	45	50	55	60	65	70
101	53	59	65	70	76	81	86	92	97	1.1502
98	85	90	96	1.1502	1.1507	1.1512	1.1518	1.1523	1.1528	33
95	1.1516	1.1521	1.1527	33	38	43	49	54	59	64
92	47	53	58	64	69	75	80	85	90	95
89	78	84	89	95	1.1600	1.1606	1.1611	1.1616	1.1621	1.1626
86	1.1609	1.1615	1.1621	1.1626	32	37	42	47	52	57
83	40	46	52	57	63	68	73	78	83	88
80	71	77	83	88	94	99	1.1704	1.1710	1.1715	1.1720
77	1.1702	1.1708	1.1714	1.1719	1.1725	1.1730	35	41	46	51
74	34	39	45	51	56	61	67	72	77	82
71	65	70	76	82	87	92	98	1.1803	1.1808	1.1813
68	96	1.1802	1.1807	1.1813	1.1818	1.1824	1.1829	34	39	44
65	1.1827	33	38	44	49	55	60	65	70	75
62	58	64	69	75	80	86	91	96	1.1901	1.1906
59	89	95	1.1901	1.1906	1.1912	1.1917	1.1922	1.1927	32	37
56	1.1920	1.1926	32	37	43	48	53	58	63	68
53	51	57	63	68	74	79	84	89	94	99
50	82	88	94	99	1.2005	1.2010	1.2015	1.2021	1.2026	1.2031
47	1.2013	1.2019	1.2025	1.2030	36	41	46	52	57	62
44	44	50	56	61	67	72	78	83	88	93
41	76	81	87	93	98	1.2103	1.2109	1.2114	1.2119	1.2124
38	1.2107	1.2112	1.2118	1.2124	1.2129	34	40	45	50	55
35	38	43	49	55	60	65	71	76	81	86
32	69	75	80	86	91	97	1 2202	1.2207	1.2212	1.2217

FACTORS OF EVAPORATION.

Gauge Pressures lbs. / Absolute Pressures	78 + / 93	80 + / 95	82 + / 97	84 + / 99	86 + / 101	88 + / 103	90 + / 105	92 + / 107	94 + / 109	96 + / 111	98 + / 113
Feed wat' tem.	FACTORS OF EVAPORATION.										
212	1.0349	1.0353	1.0358	1.0363	1.0367	1.0372	1.0376	1.0381	1.0385	1.0389	1.0393
209	80	85	90	94	99	1.0403	1.0408	1.0412	1.0416	1.0421	1.0425
206	1.0411	1.0416	1.0421	1.0426	1.0430	35	39	43	48	52	56
203	43	48	52	57	62	66	71	75	79	83	88
200	74	79	84	89	93	98	1.0502	1.0506	1.0511	1.0515	1.0519
197	1.0506	1.0511	1.0515	1.0520	1.0525	1.0529	33	38	42	46	50
194	37	42	47	51	56	60	65	69	73	78	82
191	69	73	78	83	87	92	96	1.0601	1.0605	1.0609	1.0613
188	1.0600	1.0605	1.0610	1.0614	1.0619	1.0623	1.0628	32	36	40	45
185	31	36	41	46	50	55	59	63	68	72	76
182	63	68	72	77	81	86	90	95	99	1.0703	1.0707
179	94	99	1.0704	1.0708	1.0713	1.0717	1.0722	1.0726	1.0730	35	39
176	1.0725	1.0730	35	40	44	49	53	57	62	66	70
173	57	62	66	71	75	80	84	89	93	97	1.0801
170	88	93	98	1.0802	1.0807	1.0811	1.0816	1.0820	1.0824	1.0829	33
167	1.0819	1.0824	1.0829	34	38	43	47	51	56	60	64
164	51	56	60	65	69	74	78	83	87	91	95
161	82	87	92	96	1.0901	1.0905	1.0910	1.0914	1.0918	1.0923	1.0927
158	1.0913	1.0918	1.0923	1.0927	32	37	41	45	50	54	58
155	45	49	54	59	63	68	72	77	81	85	89
152	76	81	85	90	95	99	1.1004	1.1008	1.1012	1.1016	1.1021
149	1.1007	1.1012	1.1017	1.1021	1.1026	1.1030	35	39	43	48	52
146	38	43	48	53	57	62	66	70	75	79	83
143	70	74	79	84	88	93	97	1.1102	1.1106	1.1110	1.1114
140	1.1101	1.1106	1.1110	1.1115	1.1120	1.1124	1.1129	33	37	41	46
137	32	37	42	46	51	55	60	64	68	73	77
134	63	68	73	78	82	87	91	95	1.1200	1.1204	1.1208
131	95	99	1.1204	1.1209	1.1213	1.1218	1.1222	1.1227	31	35	39
128	1.1226	1.1231	35	40	45	49	53	58	62	66	71
125	57	62	67	71	76	80	85	89	93	98	1.1302
122	88	93	98	1.1302	1.1307	1.1311	1.1316	1.1320	1.1325	1.1329	33
119	1.1320	1.1324	1.1329	34	38	43	47	51	56	60	64
1.6	51	55	60	65	69	74	78	83	87	91	95
113	82	87	91	96	1.1401	1.1405	1.1409	1.1414	1.1418	1.1422	1.1426
110	1.1413	1.1418	1.1422	1.1427	32	36	41	45	49	53	58
107	44	49	54	58	63	67	72	76	80	85	89
104	75	80	85	89	94	99	1.1503	1.1507	1.1512	1.1516	1.1520
101	1.1506	1.1511	1.1516	1.1521	1.1525	1.1530	34	38	43	47	51
98	38	42	47	52	56	61	65	70	74	78	82
95	69	74	78	83	87	92	96	1.1601	1.1605	1.1609	1.1613
92	1.1660	1.1605	1.1609	1.1614	1.1619	1.1623	1.1628	32	36	40	45
89	31	36	41	45	50	54	59	63	67	72	76
86	62	67	72	76	81	85	90	94	98	1.1703	1.1707
83	93	98	1.1703	1.1707	1.1712	1.1717	1.1721	1.1725	1.1730	34	38
80	1.1724	1.1729	34	39	43	48	52	56	61	65	69
77	56	60	65	70	74	79	83	88	92	96	1.1800
74	87	91	96	1.1801	1.1805	1.1810	1.1814	1.1819	1.1823	1.1827	31
71	1.1818	1.1823	1.1827	32	36	41	45	50	54	58	62
68	49	54	58	63	68	72	77	81	85	89	94
65	80	85	89	94	99	1.1903	1.1908	1.1912	1.1916	1.1920	1.1925
62	1.1911	1.1916	1.1921	1.1925	1.1930	34	39	43	47	52	56
59	42	47	52	56	61	65	70	74	78	83	87
56	73	78	83	87	92	96	1.2001	1.2005	1.2010	1.2014	1.2018
53	1.2004	1.2009	1.2014	1.2018	1.2023	1.2028	32	36	41	45	49
50	35	40	45	50	54	59	63	67	72	76	80
47	66	71	76	81	85	90	94	98	1.2103	1.2107	1.2111
44	98	1.2102	1.2107	1.2112	1.2116	1.2121	1.2125	1.2130	34	38	42
41	1.2129	33	38	43	47	52	56	61	65	69	73
38	60	64	69	74	78	83	87	92	96	1.2200	1.2204
35	91	96	1.2200	1.2205	1.2209	1.2214	1.2218	1.2223	1.2227	31	35
32	1.2222	1 2227	31	36	41	45	49	54	58	62	67

FACTORS OF EVAPORATION.

Gauge Pressur's lbs. 100 + / Absolute Press. lbs. 115	105 + / 120	110 + / 125	115 + / 130	120 + / 135	125 + / 140	130 + / 145	135 + / 150	140 + / 155	145 + / 160	150 + / 165
Feed wat'r tem.	Factors of Evaporation.									
212	1.0397	1.0407	1.0417	1.0427	1.0436	1.0445	1.0453	1.0462	1.0470	1.0478	1.0486
209	1.0429	39	49	58	67	76	85	93	1.0501	1.0509	1.0517
206	60	70	80	89	99	1.0508	1.0516	1.0525	33	41	48
203	92	1.0502	1.0511	1.0521	1.0530	39	48	56	64	72	80
200	1.0523	33	43	52	62	70	79	87	96	1.0604	1.0611
197	55	65	74	84	93	1.0602	1.0610	1.0619	1.0627	35	43
194	86	96	1.0606	1.0615	1.0624	33	42	50	58	66	74
191	1.0617	1.0627	37	47	56	65	73	82	90	98	1.0706
188	49	59	69	78	87	96	1.0705	1.0713	1.0721	1.0729	37
185	80	90	1.0700	1.0709	1.0719	1.0727	36	44	53	61	68
182	1.0712	1.0722	31	41	50	59	67	76	84	92	1.0800
179	43	53	63	72	81	90	99	1.0807	1.0815	1.0823	31
176	74	84	94	1.0803	1.0813	1.0821	1.0830	39	47	55	62
173	1.0806	1.0816	1.0825	35	44	53	61	70	78	86	94
170	37	47	57	66	75	84	93	1.0901	1.0909	1.0917	1.0925
167	68	78	88	97	1.0907	1.0915	1.0924	32	41	49	56
164	1.0900	1.0910	1.0919	1.0929	38	47	55	64	72	80	88
161	31	41	51	60	69	78	87	95	1.1003	1.1011	1.1019
158	62	72	82	91	1.1000	1.1069	1.1018	1.1026	35	43	50
155	93	1.1003	1.1013	1.1023	32	41	49	58	66	74	82
152	1.1025	35	44	54	63	72	81	89	97	1.1105	1.1113
149	56	66	76	85	94	1.1103	1.1112	1.1120	1.1128	36	44
146	87	97	1.1107	1.1116	1.1126	34	43	51	60	68	75
143	1.1118	1.1129	38	48	57	66	74	83	91	99	1.1207
140	50	60	70	79	88	97	1.1206	1.1214	1.1222	1.1230	38
137	81	91	1.1201	1.1210	1.1219	1.1228	37	45	53	61	69
134	1.1212	1.1222	32	41	51	59	68	76	85	93	1.1200
131	43	53	63	73	82	91	99	1.1308	1.1316	1.1324	32
128	75	85	94	1.1304	1.1313	1.1322	1.1331	39	47	55	63
125	1.1306	1.1316	1.1326	35	44	53	62	70	78	86	94
122	37	47	57	66	75	84	93	1.1401	1.1409	1.1417	1.1425
119	68	78	88	97	1.1407	1.1415	1.1424	32	41	49	56
116	99	1.1409	1.1419	1.1429	38	47	55	64	72	80	88
113	1.1431	41	50	60	69	78	86	95	1.1503	1.1511	1.1519
110	62	72	82	91	1.1500	1.1509	1.1518	1.1526	34	42	50
107	93	1.1503	1.1513	1.1522	31	46	49	57	65	73	81
104	1.1524	34	44	53	62	71	80	88	97	1.1605	1.1612
101	55	65	75	84	94	1.1602	1.1611	1.1620	1.1628	36	43
98	86	96	1.1606	1.1616	1.1625	34	42	51	59	67	75
95	1.1618	1.1628	37	47	56	65	73	82	90	98	1.1706
92	49	59	68	78	87	96	1.1705	1.1713	1.1721	1.1729	37
89	80	90	1.1700	1.1709	1.1718	1.1727	36	44	52	60	68
86	1.1711	1.1721	31	40	49	58	67	75	83	91	99
83	42	52	62	71	80	89	98	1.1806	1.1815	1.1823	1.1830
80	73	83	93	1.1802	1.1812	1.1820	1.1829	37	46	54	61
77	1.1804	1.1814	1.1824	34	43	52	60	69	77	85	93
74	35	45	55	65	74	83	91	1.1900	1.1908	1.1916	1.1924
71	67	77	86	96	1.1905	1.1914	1.1922	31	39	47	55
68	98	1.1908	1.1917	1.1927	36	45	54	62	70	78	86
65	1.1929	39	49	58	67	76	85	93	1.2001	1.2009	1.2017
62	60	70	80	89	98	1.2007	1.2016	1.2024	32	40	48
59	91	1.2001	1.2011	1.2020	1.2029	38	47	55	63	71	79
56	1.2022	32	42	51	60	69	78	86	94	1.2102	1.2110
53	53	63	73	82	91	1.2100	1.2109	1.2117	1.2126	34	41
50	84	94	1.2104	1.2113	1.2123	31	40	48	57	65	72
47	1.2115	1.2125	35	44	54	63	71	80	88	96	1.2203
44	46	56	66	76	85	94	1.2202	1.2211	1.2219	1.2227	35
41	77	87	97	1.2207	1.2216	1.2225	33	42	50	58	66
38	1.2208	1.2219	1.2228	38	47	56	64	73	81	89	97
35	40	50	59	69	78	87	95	1.2304	1.2312	1.2320	1.2328
32	71	81	90	1.2300	1.2309	1.2318	1.2326	35	43	51	59

HEAT UNITS IN WATER, BETWEEN 32° AND 212° F. (reckoned from 32° F.) AND WEIGHT OF WATER PER CUBIC FOOT.

Temperature.	Heat Units.	Weight, lbs. per c. ft.	Temperature.	Heat Units.	Weight, lbs. per c. ft.
32 ° F.	0.	62.42	87 ° F.	55.05	62.16
33	1.	62.42	88	56.05	62.15
34	2.	62.42	89	57.05	62.14
35	3.	62.42	90	58.06	62.13
36	4.	62.42	91	59.06	62.12
37	5.	62.42	92	60.06	62.11
38	6.	62.42	93	61.06	62.10
39	7.	62.42	94	62.06	62.09
40	8.	62.42	95	63.07	62.08
41	9.	62.42	96	64.07	62.07
42	10.	62.42	97	65.07	62.06
43	11.	62.42	98	66.07	62.05
44	12.	62.42	99	67.08	62.03
45	13.	62.42	100	68.08	62.02
46	14.	62.42	101	69.08	62.01
47	15.	62.42	102	70.09	62.00
48	16.	62.41	103	71.09	61.99
49	17.	62.41	104	72.09	61.97
50	18	62.41	105	73.10	61.96
51	19.	62.41	106	74.10	61.95
52	20.	62.40	107	75.10	61.93
53	21.01	62.40	108	76.10	61.92
54	22.01	62.40	109	77.11	61.91
55	23.01	62.39	110	78.11	61.89
56	24.01	62.39	111	79.11	61.88
57	25.01	62.39	112	80.12	61.86
58	26.01	62.38	113	81.12	61.85
59	27.01	62.38	114	82.13	61.83
60	28.01	62.37	115	83.13	61.82
61	29.01	62.37	116	84.13	61.80
62	30.01	62.36	117	85.14	61.78
63	31.01	62.36	118	86.14	61.77
64	32.01	62.35	119	87.15	61.75
65	33.01	62.34	120	88.15	61.74
66	34.02	62.34	121	89.15	61.72
67	35.02	62.33	122	90.16	61.70
68	36.02	62.33	123	91.16	61.68
69	37.02	62.32	124	92.17	61.67
70	38.02	62.31	125	93.17	61.65
71	39.02	62.31	126	94.17	61.63
72	40.02	62.30	127	95.18	61.61
73	41.02	62.29	128	96.18	61.60
74	42.03	62.28	129	97.19	61.58
75	43.03	62.28	130	98.19	61.56
76	44.03	62.27	131	99.20	61.54
77	45.03	62.26	132	100.20	61.52
78	46.03	62.25	133	101.21	61.51
79	47.03	62.24	134	102.21	61.49
80	48.04	62.23	135	103.22	61.47
81	49.04	62.22	136	104.22	61.45
82	50.04	62.21	137	105.23	61.43
83	51.04	62.20	138	106.23	61.41
84	52.04	62.19	139	107.24	61.39
85	53.05	62.18	140	108.25	61.37
86	54.05	62.17	141	109.25	61.36

Temperature.	Heat Units.	Weight, lbs. per c. ft.	Temperature.	Heat Units.	Weight, lbs. per c. ft.
142 ° F.	110.26	61.34	178 ° F.	146.52	60.59
143	111.26	61.32	179	147.53	60.57
144	112.27	61.30	180	148.54	60.55
145	113.28	61.28	181	149.55	60.53
146	114.28	61.26	182	150.56	60.50
147	115.29	61.24	183	151.57	60.48
148	116.29	61.22	184	152.58	60.46
149	117.30	61.20	185	153.59	60.44
150	118.31	61.18	186	154.60	60.41
151	119.31	61.16	187	155.61	60.39
152	120.32	61.14	188	156.62	60.37
153	121.33	61.12	189	157.63	60.34
154	122.33	61.10	190	158.64	60.32
155	123.34	61.08	191	159.65	60.29
156	124.35	61.06	192	160.67	60.27
157	125.35	61.04	193	161.68	60.25
158	126.36	61.02	194	162.69	60.22
159	127.37	61.00	195	163.70	60.20
160	128.37	60.98	196	164.71	60.17
161	129.38	60.96	197	165.72	60.15
162	130.39	60.94	198	166.73	60.12
163	131.40	60.92	199	167.74	60.10
164	132.41	60.90	200	168.75	60.07
165	133.41	60.87	201	169.77	60.05
166	134.42	60.85	202	170.78	60.02
167	135.43	60.83	203	171.79	60.00
168	136.44	60.81	204	172.80	59.97
169	137.45	60.79	205	173.81	59.95
170	138.45	60.77	206	174.83	59.92
171	139.46	60.75	207	175.84	59.89
172	140.47	60.73	208	176.85	59.87
173	141.48	60.70	209	177.86	59.84
174	142.49	60.68	210	178.87	59.82
175	143.50	60.66	211	179.89	59.79
176	144.51	60.64	212	180.90	59.76
177	145.52	60.62			

Total Heat Units in Water and Steam.

(Reckoned above 32° Fahrenheit.)

Gauge Pressure. lb. per sq. in.	Absolute Pressure lbs. per sq. in.	Temperature Fahr. °	Heat Units in Steam. H.	Heat Units in Water. h.	Latent Heat of Evaporation. $H-h$.
0.	14.696	212.00	1146.60	180.90	965.70
0.304	15	213.03	1146.91	181.94	964.97
1 +	16	216.30	1147.91	185.25	962.66
5 +	20	227.92	1151.45	197.04	954.41
10 +	25	240.00	1155.14	209.31	945.83
15 +	30	250.25	1158.26	219.74	938.52
20 +	35	259.18	1160.99	228.84	932.15
25 +	40	267.12	1163.41	236.94	926.47
30 +	45	274.30	1165.60	244.27	921.33
35 +	50	280.85	1167.60	250.97	916.63
40 +	55	286.90	1169.44	257.15	912.29
42 +	57	289.11	1170.14	259.50	910.64

Gauge Pressure lb. per sq. in.	Absolute Pressure lbs. per sq. in.	Temperature Fahr. °	Heat Units in Steam. *H.*	Heat Units in Water. *h.*	Latent Heat of Evaporation. *H − h.*
41 +	59	291.43	1170.82	261.79	909.03
46 +	61	293.60	1171.49	264.02	907.47
48 +	63	295.71	1172.13	266.18	905.95
50 +	65	297.78	.76	268.30	904.46
51 +	66	298.79	1173.07	269.34	903.73
52 +	67	299.79	.38	270.36	903.01
53 +	68	300.78	.68	271.38	902.30
54 +	69	301.75	.97	272.38	901.59
55 +	70	302.72	1174.27	273.37	900.90
56 +	71	303.67	.56	274.35	900.21
57 +	72	304.62	.85	275.32	899.53
58 +	73	305.55	1175.13	276.28	898.85
59 +	74	306.47	.41	277.23	898.19
60 +	75	307.39	.69	278.17	897.53
61 +	76	308.29	.97	279.09	896.88
62 +	77	309.18	1176.24	280.01	896.23
63 +	78	310.07	.51	280.92	895.59
64 +	79	310.95	.78	281.82	894.96
65 +	80	311.81	1177.04	282.71	894.33
66 +	81	312.67	.30	283.59	893.71
67 +	82	313.52	.56	284.47	893.09
68 +	83	314.36	.82	285.33	892.49
69 +	84	315.20	1178.07	286.19	891.88
70 +	85	316 02	.33	287.04	891.29
71 +	86	316.84	.58	387.88	890.69
72 +	87	317.65	.82	288.72	890.11
73 +	88	318.45	1179.07	289.54	889.53
74 +	89	319.25	.31	290.36	888.95
75 +	90	320.04	.55	291.18	888.38
76 +	91	320.82	.79	291.98	887.81
77 +	92	321.60	1180.03	292.78	887.25
78 +	93	322.37	.26	293.57	886.69
79 +	94	323.13	.49	294.36	886.14
80 +	95	323.88	.72	295.14	885.59
81 +	96	324.63	.95	295.91	885.04
82 +	97	325.38	1181.18	296.67	884.51
83 +	98	326.11	.41	297.43	883.97
84 +	99	326.85	.63	298.19	883.44
85 +	100	327.57	.85	298.94	882.91
86 +	101	328.29	1182.07	299.68	882.39
87 +	102	329.01	.29	300.41	881.87
88 +	103	329.71	.50	301.14	881.36
89 +	104	330.42	.72	301.87	880.85
90 +	105	331.11	.93	302.59	880.34
91 +	106	331.81	1183.14	203.30	879.84
92 +	107	332.49	.35	304.01	879.34
93 +	108	333.17	.56	304.71	878.84
94 +	109	333.85	.77	305.41	878.35
95 +	110	334.52	.97	306.10	877.87
96 +	111	335.19	1184.17	306.79	877.38
97 +	112	335.85	.38	307.48	876.90
98 +	113	336.51	.58	308.16	876.42
99 +	114	337.17	.78	308.83	875.94
100 +	115	337.81	.97	309.50	875.47
101 +	116	338.46	1185.17	310.17	875.00
102 +	117	339.10	.37	310.83	874.54
103 +	118	339.74	.56	311.49	874.07
104 +	119	340.37	.75	312.14	873.61

Gauge Pressure lb. per sq. in.	Absolute Pressure lbs. per sq. in.	Temperature Fahr. °	Heat Units in Steam. H.	Heat Units in Water. h.	Latent Heat of Evaporation. $H - h$.
105 +	120	341.00	.94	312.79	873.16
106 +	121	341.62	1186.13	313.43	872.70
107 +	122	342.24	.32	314.07	872.25
108 +	123	342.85	.51	314.71	871.80
109 +	124	343.47	.70	315.34	871.36
110 +	125	344.07	.88	315.97	870.91
111 +	126	344.08	1187.07	316.60	870.47
112 +	127	345.28	.25	317.22	870.03
113 +	128	345.88	.43	317.83	869.60
114 +	129	346.46	.61	318.45	869.17
115 +	130	347.06	.79	319.06	868.74
116 +	131	347.64	.97	319.66	868.31
117 +	132	348.23	1188.15	320.27	867.88
118 +	133	348.81	.33	320.87	867.46
119 +	134	349.38	.50	321.46	867.04
120 +	135	349 95	.68	322.06	866.62
121 +	136	350.52	.85	322.64	866.21
122 +	137	351.09	1189.02	323.23	865.79
123 +	138	351.75	.19	323.81	865.38
124 +	139	352.21	.37	324.39	864.97
125 +	140	352.77	.54	324.97	864.57
126 +	141	353.32	.70	325.54	864.16
127 +	142	353.87	.87	326.11	863.76
128 +	143	354.42	1190.04	326.68	863.36
129 +	144	354.96	.20	327.24	862.96
130 +	145	355.50	.37	327.80	862.57
131 +	146	356.04	.53	328.36	862.17
132 +	147	356.57	.70	328.91	861.78
133 +	148	357.11	.86	329.47	861.39
134 +	149	357.64	1191.02	330.01	861.01
135 +	150	358.16	.18	330.56	860.62
136 +	151	358.68	.34	331.10	860.24
137 +	152	359.20	.50	331.64	859.86
138 +	153	359.72	.66	332.18	859.48
139 +	154	360.24	.81	332.71	859.10
140 +	155	360.75	.97	333.24	858.73
141 +	156	361.26	1192.13	333.77	858.35
142 +	157	361.77	.28	334.30	857.98
143 +	158	362.27	.44	334.82	857.61
144 +	159	362.78	.59	335.35	857.24
145 +	160	363 28	.74	335.87	856.87
146 +	161	363.77	.89	336.38	856.51
147 +	162	364.27	1193.04	336.90	856.15
148 +	163	364.76	.19	337.41	855.78
149 +	164	365.26	.34	337.92	855.42
150 +	165	365.74	.49	338.43	855.07
155 +	170	368.16	1194.23	340.93	853.29
160 +	175	370.51	.95	343.38	851.57
165 +	180	372.82	1195.65	345.78	849.87
170 +	185	375.08	1196.34	348.13	848.21
175 +	190	377.29	1197.01	350.43	846.58
180 +	195	379.45	.67	352.68	844.99
185 +	200	381.57	1198.32	354.89	843.43
190 +	205	383.66	.95	357.06	841.90
195 +	210	385.67	1199.58	359.18	840.40
205 +	220.44	389.84	1200.84	363.51	837.34
220 +	235.14	395.43	1202.55	369.34	833.21
235 +	249.83	400.76	1204.17	374.91	829.26
250 −	264.53	405.84	1205.72	380.25	825.48

CLX.

SOUND CASTING.

BY THOMAS D. WEST, CLEVELAND, OHIO.

THE term *sound* is of far more importance than any other which can be applied to designate a good casting.

A sound casting can seldom be judged by its outward appearance.

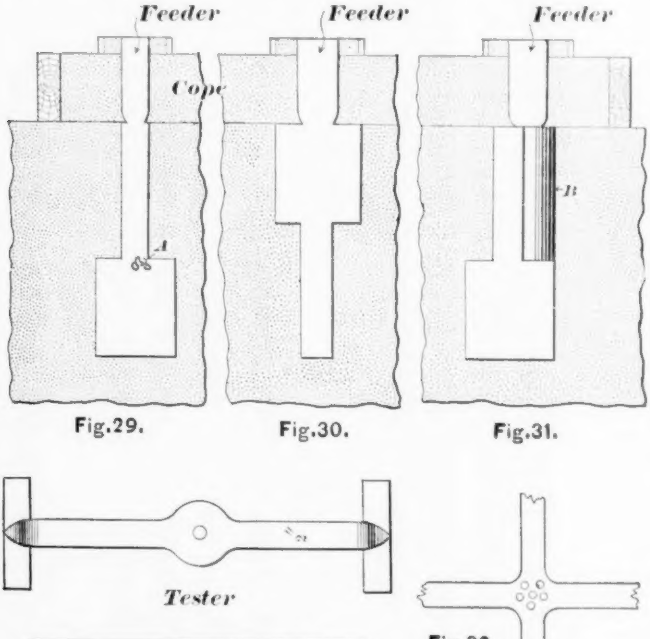


Fig.29. Fig.30. Fig.31.

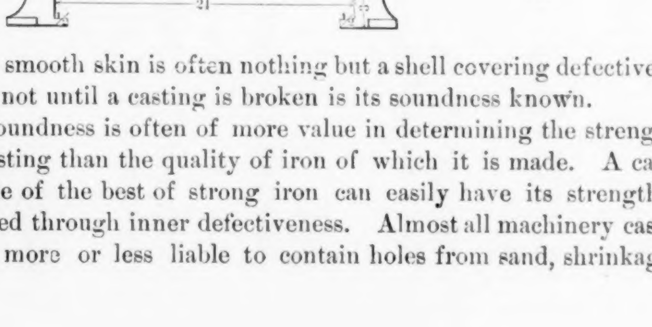


Fig.32.

The smooth skin is often nothing but a shell covering defectiveness, and not until a casting is broken is its soundness known.

Soundness is often of more value in determining the strength of a casting than the quality of iron of which it is made. A casting made of the best of strong iron can easily have its strength annulled through inner defectiveness. Almost all machinery castings are more or less liable to contain holes from sand, shrinkage, or

blow-holes. Often castings are so constructed that even were the moulder to turn them out free of sand or blow-holes, the shrinkage-hole would show up, were the casting to be broken, despite all the feeding he could do; the reason for this is best shown through an explanation of Fig. 29. Here we have, as is often the case, a heavy and light part connected. Now, were it always practicable to have the heaviest part the uppermost, as seen at Fig. 30, so as to be accessible for feeding, then the moulder could justly be blamed were the casting not sound.

No doubt every engineer will at a glance perceive the difficulty attending obtaining the perfect soundness of such a section as Fig. 29. Here we have the heaviest portion surmounted by a light body, which will become set much the soonest. The light part having frozen, any feeding-head that may be over it cannot be of any further benefit in supplying the lower heavy portion to feed its solidifying crust—which, by the way, in many cases, may not have begun to set until after the upper light part has solidified. This lower body having nothing now left to draw from, will draw metal from its uppermost liquid portion, which in such a section as shown, would leave cavities which would weaken the casting at *A*.

In practice when such sections as at Fig. 29 are thought to be required to stand much strain, it is best generally when practicable to have an enlargement made as seen at *B*, Fig. 31. This gives a body which by means of a feeding-rod and by occasionally pouring hot iron in the feeding-head will remain in a fluid state as long as the heavy portion. This accomplished, it can be readily seen that a cavity as at *A*, Fig. 29, is prevented from being formed.

Now it is by no means practicable to attain soundness in all castings by this means, for there are many moulds in which the intended form of the casting would be made almost unrecognizable were they to have all their heavy sections thus reached and fed by risers. Attending this is often the impracticability of placing over three or four feeders upon a mould, for often the bars of the cope, chaplets, binders and weights will not permit of using any more. Then, again, were it practicable to have a cope filled with feeding-heads, there are many castings which, in order to be sound would require that more men be taken off from the work of "running off the heat" than foundries at casting-time can generally spare.

It is very evident from the shapes of existing patterns and castings, that but little thought has been given to this element involved in obtaining an entirely sound casting. The best place to study

this error is at the "scrap pile." There one can find the shrink-hole in many forms. Often fillets which were intended as factors for strength will be found to be exactly the reverse. The greater part of machinery castings made, are more or less filleted, and some designers have the idea that the larger the fillet the greater the strength given. In cases where the fillet is fed by other metal than that contained in its central body this may be true. Often fillets are not accessible to be fed by other than the metal contained within its own body, and therefore, as illustrated by Fig. 32, a large fillet in such cases may often be a source of unsoundness.

A well-proportioned casting should not always be considered only from the standpoint of the strains which its respective parts have to stand. While it is often true that some part may be very light in comparison with others, it is more often better that the light part be made heavier in excess of what its strength requires in order that strains may be avoided as well as "draw holes," caused through unequal thickness of parts.

To give some data as to what extent ordinary cast iron will shrink, I have lately been experimenting with round balls of different diameters. The sizes of these were respectively about 4″, 5⅜″, 6¾″, and 10⅜″. Two of each size were cast at three different heats, thus making altogether 24 balls, and of these 12 were cast without any feeders, while 12 had them. The feeding heads for 4″ balls were 2½″ diameter; for 5⅜″ balls, 3½″ diam.; for 6¾″ ball, 4″ diam.; for 10⅜″ ball, 5″ diam.

For the first three sizes, the height of head from the flask joint up to the top of gate was 9″, and for the 10⅜″ balls the head was 12″. The gates which admitted the metal into the moulds were cut broad and very thin, in order that they should freeze a few moments after the mould became full, thereby insuring that metal did not enter through the pouring gates to supply any shrinkage. In pouring these balls, the iron was medium hot, and the gates were filled up to the heights given. The balls having the feeding heads were "churned" until they solidified.

In cleaning the castings, the feeding heads were chipped off so as to preserve the spherical form of the balls as much as possible.

This statement with reference to the manner of moulding and casting the balls is simply given to show the conditions under which the tests were made.

The following is a table giving the weights of the balls and the difference between the fed and the unfed balls:

FIRST HEAT.

Mixture of iron 200 lbs. ordinary No. 2 pig and 400 lb. scrap.

DIAMETER OF BALLS.	FED.	UNFED.	SHRINKAGE FOUND.	PERCENTAGE OF SHRINKAGE.
4″	8 lbs. 12 oz.	8 lbs. 10 oz.	2 oz.	1.428
5⅜″	20 " 11 "	20 " 8 "	3 "	0.906
6¾″	39 " 10½ "	39 " 4 "	6½ "	1.024
10⅜″	150 "	147 " 15 "	33 "	1.375

SECOND HEAT.

Mixture of Iron.

100 lbs. No. 1, Bessemer. A strong coke iron.
100 " " Hubbard. " " "
100 " " Pine Grove. " charcoal iron.
300 " Machinery scrap iron.

DIAMETER OF BALLS.	FED.	UNFED.	SHRINKAGE FOUND.	PERCENTAGE OF SHRINKAGE.
4″	8 lbs. 13½ oz.	8 lbs. 12 oz.	1½ oz.	1.060
5⅜″	20 " 13 "	20 " 9 "	4 "	1.201
6¾″	39 " 11½ "	39 " 6 "	5½ "	0.865
10⅜″	149 " 12 "	148 " 7 "	21 "	0.876

This second heat was poured with middling fluid iron.

THIRD HEAT.

Mixture of Iron.

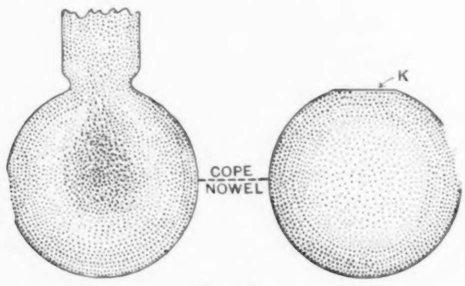
400 lbs. No. 1, Hubbard. A strong coke iron.
200 " Machinery scrap iron.

DIAMETER OF BALLS.	FED.	UNFED.	SHRINKAGE FOUND.	PERCENTAGE OF SHRINKAGE.
4″	8 lbs. 14¾ oz.	8 lbs. 12½ oz.	2¼ oz.	1.576
5⅜″	20 " 14 "	20 " 9¾ "	4¼ "	1.272
6¾″	39 " 12½ "	39 " 7½ "	5 "	0.785
10⅜″	149 " 8 "	148 " 6 "	18 "	0.752

In this third heat, with the exception of the $10\frac{3}{8}''$ balls, they were all poured with a more fluid metal than was used in the two upper heats. This I would assign as the reason for the $6\frac{3}{4}''$, $5\frac{3}{8}''$, and 4″ feed balls being heavier than any of the others, as shown.

In classing one heat against another, the mixtures of the iron must be taken into consideration. Balls from each of the respective heats were split in order to learn, if possible, the cause of the dissimilarity of weight most noticeable in the smaller sizes.

The cuts at Fig. 32 partly illustrate the fracture of the split balls.



FED **Fig. 32a** UNFED

The three smallest sized unfed balls showed a very open grain at their centers, gradually increasing in density towards the shell. The unfed 10 balls were not only very porous at their centers but contained large holes as well. The flat place seen at K shows about how the top part of the unfed balls looked. This was of course formed while the crust remained fluid enough to supply shrinkage. After the crust became set, the balance of shrinkage was then drawn from the innermost fluid portion of the balls as proved by the porousness and holes found when the balls were split open. The fed balls were the most dense in the middle, the most porous part of them being about midway between the shell and center, as seen in the cut. The density of some of the fed balls at the center was remarkable, and was a clear explanation o the cause of their variation in weight. This center-density was, n doubt, mainly caused by the pressure exerted by the feeding rod, and the occasional supplying of the feeding heads with hot iron. When feeding a casting the feeding rod at the latter end, is more or less enlarged, caused by molten metal sticking to it. This may be knocked off or a new rod used, but whichever way is used there will exist variations in the manipulations of feeding sufficient to cause the dissimilarity in weights seen. It seems reasonable to

assert that a thick feeding rod should exert more of a pressure and disturbance than a thinner rod, and that the smaller the ball the more effect could be produced.

In moulding these balls I was very careful in all the manipulations performed. The ramming, venting, drawing of the pattern, and gating, were as near alike as study and care could make them. In feeding, attention was given to the procuring of solid castings. The $10\frac{3}{8}''$ ball would occupy from fifty to sixty minutes to be fed solid, and although these largest balls show about the lowest percentage in shrinkage they no doubt give the nearest approximation that it would be practical to assign to shrinkage in the general run of castings, which if called 1 lb. to the 1 cwt. of casting, would not be far out of the way.

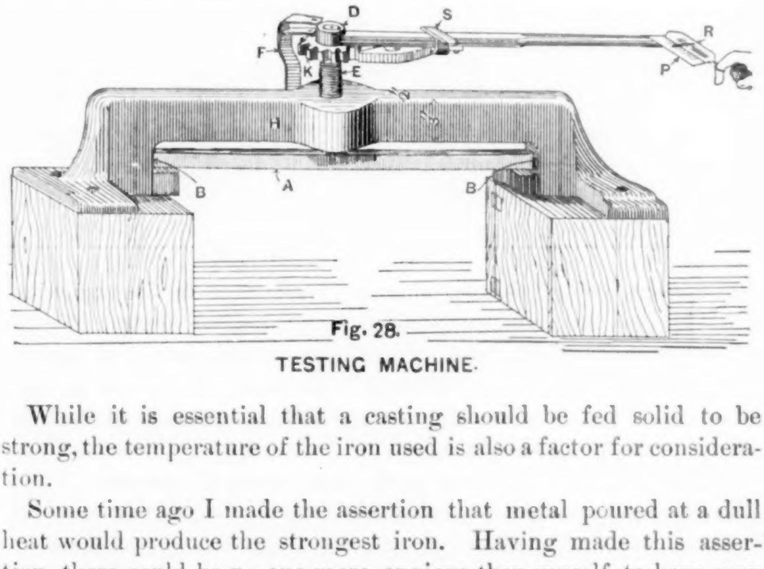


Fig. 28.

TESTING MACHINE.

While it is essential that a casting should be fed solid to be strong, the temperature of the iron used is also a factor for consideration.

Some time ago I made the assertion that metal poured at a dull heat would produce the strongest iron. Having made this assertion, there could be no one more anxious than myself to have seen this kept a maintained fact. Mr. Gardiner, foreman of Pratt & Whitney's foundry, at Hartford, Conn., has informed me that, through experiments which he had made with test bars, poured dull and poured hot, he found the hot-poured bars the strongest. Thinking that I might be in error, from the fact that the tests I had made were but few and crudely performed (as can be seen from the description then given), I desired to give the question a thorough test. Having no testing machine, I devised the simple affair shown in Fig. 28 for the purpose of dealing with the subject. In

using this machine, bars 1″ square × 24″ long were tested. In all tests the hot-poured bars stood the greatest load. To make sure that my machine was working correctly and to know what the results would be were heavier bars used than 1″ square, I had some patterns made, measuring 4¼″, 2¼″, and 1¼″ square by 24″ long. When cast, they were taken into the machine shop and accurately planed up to the respective sizes, 4″, 3½″, 2″, and 1″ square. The following table shows the strength of the dull and hot-poured bars as found by tests taken by an Olsen machine at the Ohio Steel Works, Cleveland, O.:

SECTION OF BARS. 24 INCHES LONG.		BREAKING LOAD.	SECTION OF BARS. 24 INCHES LONG.		BREAKING LOAD.
4″ square.	Hot	56,130	2″ square.	Hot	9,520
4″ "	Dull	49,830	2″ " .	Dull	6,400
3½″ "	Hot	38,470	1″ "	Hot	1,050
3½″ "	Dull	36,960	1″ "	Dull	1,020
2″ "	Hot	7,560	1″ "	Hot	1,139
2″ "	Dull	6,340	1″ "	Dull	960
2″ "	Hot	8,650			
2″ "	Dull	6,810			

The above bars all showed a perfect fracture, with the exception of the 3½″ dull bar, which showed a honey-combed center. These 3½″ bars were intended for 4,″ but as soon as the skin was broken when planing them "blow-holes" were seen, and thinking that were the bar planed down they might disappear, the machinist was instructed to make the bars 3½″ sq. As every cut revealed fresh holes it was found no cleaner at 3½″ than at the 4″ square.

These blow-holes were readily accounted for by the fact that the iron with which this bar was poured was so dull that it would hardly flow out of the ladle. It was purposely so poured in order to learn how it would stand for strength. The result as shown will no doubt be a surprise to many as it was to me, for although this bar showed such a bad fracture, we see that it stood within 1,510 lbs. as much as the hot bar whose fracture was perfect like all the others. It might be well to state that these test bars were cast vertical in order to insure their being sound and clean. There would be two bars of the same size moulded, and after one was poured with the hot metal direct from the cupola, the ladle would be allowed to stand until the balance of the metal was just dull enough to insure

that the casting should run up full and square. I have omitted the mixtures of which the respective bars were made, for the reason that a knowledge of them would be of no assistance in determining the end sought.

Since making the above tests it has occurred to the writer's mind, that his first experiments which showed dull iron to make the strongest bars were affected by the fact of the first test bars being poured with metal which was, as stated, agitated with wrought-iron rods.

The above bars were all poured with iron which was not in any way agitated; the metal being left to cool off naturally. Therefore the first test may not be in any error, and may simply go to show that it is beneficial to agitate hot metal with wrought iron-rods.

Before closing I would respectfully call *further* attention to the machine shown in Fig. 28.

This machine was first originated for the purpose of aiding me to determine the strength of the 1" sq. bars above mentioned. As some such machine would be found very useful to many, I studied to make it as presentable as possible. The weight of the whole machine is only about 80 lbs., and any one who may choose to give it a trial would, I think, be pleased with its workings, especially in view of the amount it would cost to make one (which should not exceed \$6.00). The machine is most adapted for testing foundry mixtures of iron, and new brands of pig iron. As seen it will record the three essential points which foundry men ought to know about their iron.

The first is the *contraction* of the iron.

The second its *deflection*.

The third its *strength*.

In obtaining the contraction, the pattern A from which the test-bars are to be made should be just the length of the distance between the standpoints B B. Then when the bars are cast, all that is necessary after one is set in place is to keep it tight to one end and the space at the other will give the contraction.

For obtaining the deflection, a piece at F has a slot through which a thumb set-screw binds it against the stand H. Before commencing to screw down upon the bar A, the piece F is set down upon the ratchet-wheel K, and being secured by means of the thumb-screw above mentioned, it will, of course, remain stationary. Then when the bar A breaks, its deflection can be told by the space between F and the top of the ratchet-wheel. The two arms which F is seen to have are for the purpose of holding a small 2" iron rule,

divided into fifty or 100 parts, and there are slots in the arms for the purpose of holding the rule.

To obtain the strength, the load is applied by means of the screw E, which is 1¼", having 9 threads to the inch. In the bottom of the screw there is a steel pin, having a bearing surface of about ¼". The ratchet-wheel K is of course secured to the screw E, and a part of the screw projects up above it so as to leave a pin for the ratchet lever D to work upon. The lever D is provided with a ratchet pawl, so that the operator can stand in the one place while working the screw. Behind the pawl is a spring so as to force it into the teeth of the ratchet. At S is a sliding band, which when pulled back releases the hold of the spring upon the pawl, thereby allowing the ratchet-wheel or screw to be turned back without removing the lever B.

At the end of the lever is a common 25 cent spring-balance scale. Across its face at R is fitted a thin piece of brass or copper plate. A wire is inserted in a small hole which is drilled through the little pin of the balance which indicates the pounds, this wire projecting out from this pin upon each side alike. Then when pulling the balance, this wire squarely pushes up the registering plate R, so that when the piece to be tested breaks the plate will register the load.

The length of this lever from the center of the screw to the point from which the balance pulls is 18". The reason for having the balance lying in the semicircular frame P is simply to insure that the pulling is always done in the one direction. The scale used with this is the 24 lbs. scale, and a load of 1,200 lbs. (which is about the strength of ordinary cast iron when tested in such sized bars as shown) exerted upon a bar to be broken will but show about 12 lbs. upon the scale.

In using this machine, were it desired to graduate the scale so as to know in actual pounds what load was being applied, all that is necessary is to set the machine upon some rolling platform scale which will weigh about 2,000 lbs. After the machine is bolted or clamped to the lower frame of the scales, and the weight of the machine noted, then turn down the screw, and as the beam of the platform scale rises, mark off upon the face of the spring balance at every hundred a straight mark. Then after going as high as is desired, the hundreds can be subdivided if preferred. Now I know that many will object to the use of the screw as a feature of this machine. The machine is certainly one that could not be used as a standard, but it will answer to let a shop know the relative strength

of its irons. If the screw is an easy fit, kept clean, and well lubricated, the machine should for such a cheap wrinkle give good approximate results. At least the deflection and shrinkage are two things which could be counted upon as positive.

When making the test bars, they should be run by means of skimming gates, and in moulding them care must be exercised in order to have the bars come all alike. The bars I used were made in a flask which had a flat iron bar mortised into each end of the nowel, just as far apart as the pattern is long. By this means the moulds could not be lengthened through any rapping of the pattern.

To know the strength of iron and the amount which it will shrink, is certainly a point of value in aiding to make strong, sound castings; and while it is often impossible to know whether a casting is sound until it is broken, we may, through a knowledge of the mode adopted in making it, often be guided in placing confidence as to the strength and soundness of the casting produced.

It should not be always looked upon as the culmination of skill to make a casting "peel" and be smooth. Many castings are more easily produced smooth than sound, and the skill and experience generally required to make sound castings will often rank far above that required to make them smooth.

DISCUSSION.

Mr. Barnes.—It is a matter of great interest to many of us who use castings to learn in this way from Mr. West that there are those among foundrymen who seek with constant and well-directed effort to make something that is thoroughly good in itself. There is such a thing as merely making something that shall sell, and it is another and a more important thing to make that which shall be good in itself.

Mr. Randolph.—I would like to ask Mr. West if he has noticed any great difference in the shrinkage of different qualities of cast iron. A few days ago I noticed some driving-wheel centers; one or two were of very soft iron, the others very hard. I found they were cast from the same pattern, by the same man, in green-sand moulds. The hard wheels were seven-sixteenths of an inch smaller, being forty-four inches in diameter.

Mr. West.—That would about accord with the experience of almost every foundryman, that hard iron, as a general thing, shrinks more than soft iron.

Mr. Stetson.—I supposed the only ones who knew anything about cast iron were those who used it. I am exceedingly annoyed by poor castings, and am glad to see it recognized that the foundrymen do know something about it. I would like to inquire if there is any progress being made in the method of casting under pressure. This method (I mean air pressure) was invented in this country and applied to casting car-wheels. I have been informed that Whitworth used this method in casting his larger gauges, and on their being broken they show a very fine fracture—the grain is fine and looked strong. I should like to know if there is any possibility of making a sure thing of cast metal by the introduction of this method.

Mr. West.—I believe they have cast cannon under hydraulic pressure in some parts of Europe; but in this country there is nothing that I know of that is any way modern in reference to it.

The only way that ordinary foundrymen have of applying pressure to moulds is by the height of gates or heads. I believe that somewhere in Europe they had some large pipes to make that had to stand enormous pressure, and they were to be cast of a mixture of wrought iron, steel and cast iron. This was melted in a cupola, and in order to make that metal sound it is said they put about from nine to ten feet of a head upon the mould. Pressure may to an extent be created by feeding. As shown in that ball there, the metal in the one that is fed is very close and dense, and in the working of the rod, as stated in the paper, it sometimes gets very thick and acts like a plunger, thereby in a sense exerting a pressure upon the cooling metal. That, probably, is to a great extent the cause of the fineness of the iron seen in the fed balls.

Mr. Durfee.—In churning a casting as large as that, does not the feed rod waste away to some extent and get incorporated with the substance of the iron forming the center of the mass of the casting?

Mr. West.—It does at the beginning, when the iron is very fluid. At first it will burn the rod away.

Mr. Durfee.—There will be a number of inches of the rod which will actually disappear?

Mr. West.—In some cases; if it is a very heavy body of iron, it will eat it up.

Mr. Durfee.—That wrought iron must be incorporated with the iron that is fed and modify the composition of the center of the casting. It would be an interesting investigation to have some samples taken out of the centers of those castings and analyzed,

to ascertain if the proportion of carbon is the same as exists on their outsides beyond the influence of the churning-rod. The combination of the wrought iron with the soft cast iron would tend to harden the cast iron.

Mr. West.—It may to an extent, but I think the influence is very slight. A great many castings are ill-fed because of the moulders' carelessness in not watching their rod.

Mr. Durfee.—The point I raised may not be an important one in large castings, but in a casting of the size of that before us, there might be, and I think would be, a slight difference in the composition, due to the combination of the melted churning-rod with the cast iron, the relative amount of carbon in the center of the casting being diminished, and the hardness of the center consequently increased.

Mr. Barr.—The subject of sound castings is one of great interest to manufacturers, and I am sure all will appreciate the efforts of Mr. West in determining the best methods of making such castings. I have endeavored for a long time to arrive at some sort of conclusion with reference to the proper selection of irons for the particular class of work I have had to execute. This particular line of selection has been followed, believing that a proper admixture of pig iron which will produce a neutral casting has more to do with the turning out of sound castings than anything else, assuming the mould to have been properly prepared, of course. I is not easy to tell by fracture merely what irons will make a neutral casting, so I have had a somewhat limited recourse to the chemical analysis of pig iron, thinking that would help somewhat. The results, as might have been anticipated, were quite unsatisfactory. I think it a common experience that mixtures of pig iron to produce castings having certain desired qualities can be had by experiment only. Once the proper kinds and proportions of pig iron are known which will produce a certain quality of work, foundrymen in general will make no changes, and if for any reason a change is made, the following two or three heats are more or less uncertain in quality. Hot short and cold short irons do not always correct each other by melting them together in the same heat. Irons which appear to be neutral in the pig do not always make neutral castings.

Mr. West's remarks in regard to moulding are good. There is no doubt that blow-holes often occur in castings because the moulds were improperly made and vented ; but I still think more trouble

is experienced through an unequal or excessive shrinkage caused by an improper mixture of irons composing the heat than through improper moulding, if ordinary care be exercised.

Mr. West.—In answer to what the gentleman has said, I would say that I shall never forget an incident where a moulder went to work and got the top iron before the bottom iron melted. There is a deal of skill required properly to charge a cupola. To get mixtures, there must be a sufficient quantity of the iron charged in order to get a per cent. out of it for the castings to be made from. Occasionally a man will go to the foundry with a casting weighing probably a couple of hundred, which he desires to have some extra good iron put into. The foundryman will promise it to him—there is no question about that. The iron wanted, as a general thing, is of some superior high-priced quality—a good brand. His intentions may be all right, but in order for him to be sure that he will get the special 200 lbs. of iron, it will depend on the size of the cupola; for instance, if it was a thirty-inch cupola, inside diameter, in order to make it sure that he would get his two hundred out, he would at least have to charge about eight hundred of iron. We do not get foundrymen, the way they are paid for castings nowadays, who will put over eight hundred pounds of expensive good iron in, in order to get two hundred pounds out.

To secure uniformity of shape in cannon-shot, they are poured at a temperature about as dull as they can be well poured. In some places they turn them over so that the shrinkage will not settle as we see on that ball shown. If a person desires a special grade of iron or castings, they must get down to the root of the thing. They should know, if possible, what ore their pig is being made of; and if they find their castings are giving satisfaction, and they know the ore that is being used, they want to stick to that. The Lake Superior ore is known among many foundrymen as the kind to give the most contraction to iron.

Mr. Webber.—We had some little practical experience in trying to get what we call printers' roller-moulds sound, and had some cast by a foundry that had a very large experience in making cannon. I would like to ask Mr. West if he can give us any information in regard to making long cylindrical castings, say from three to five inches in diameter, and being sure to get them sound.

Mr. West.—Were those cored rolls?

Mr. Webber.—Yes, sir.

Mr. West.—What was the trouble?

Mr. Webber.—The blow-holes on the inside. These rolls had to be bored out and polished. They were cast vertically and at angles in the effort to get them right. They were poured in every way.

Mr. West.—I guess that is where the trouble was. In cylinders up to as large as eight feet in diameter, which we make at the Cuyahoga Works, a moulder might be careless and let them scab, and after they were bored out you would not as a general thing see a pin's-head of dirt in them. In pouring anything cylindrical that requires to be turned or bored, the best way to pour the mould, when practicable, is by dropping from the top. Where you drop the iron, there are two advantages gained. The first is, you always have got as "hot iron" filling the top portion of the mould as you have at the bottom; and the second is, that this iron dropping down agitates and cuts up the dirt that will gather in a mould, and keeps it floating upon the top of the rising metal; whereas, if you pour all in through bottom gates, your mould may not scab, and the casting look splendid, but when it comes to be finished up it may prove to be very dirty. In pouring moulds, there is always at the start more or less dirt released. This dirt, combined with the dust and any scabs that may be started off from the surface of the moulds as the metal rises up, is apt, in cylinder moulds cast vertical which are poured altogether from the bottom, to roll more or less in lumps up against the mould sides, and form a mixture of dirt and iron of such a nature as to not be detected until the casting's skin is bored or turned off.

By the above I would not be understood as saying that all vertically cast cylinders should be poured from the top. There are many cases where such a procedure would not be advisable, for the reason that with some the first droppings would cause too much cutting up of the mould's bottom surface. To prevent this, some iron can often be run in through bottom gates, so as to fill up the mould part way before the top gates are started. This combining of top and bottom gates is, when practicable, an excellent plan to adopt, as it prevents the cutting of the mould's bottom, and at the same time causes the uppermost metal to be as fluid as that at the bottom, and also agitates and floats the dirt up to the top, as described above. A thorough discussion of the many points in cylinder casting would of course here be out of place, but nevertheless the above few ideas may be of assistance in many cases.

Mr. Stratton.—Upon the subject of iron I would like to say the chief difficulty experienced in many foundries, so far as my experience has gone, is from two causes. The first is the use of Scotch iron. In foundries here on the coast it is likely that foreigners will be employed, either as moulders or melters. They have an idea that they must have a little Scotch iron, or the mixture will not be good, and Scotch iron invariably tends, when mixed with American iron, to produce blowey castings. I have followed this matter sufficiently to convince me that Scotch iron should not be mixed with American metals. The second cause of trouble is found in the use of too much water in swabbing the moulds. The water that is applied by the moulder to build up a corner or smooth off a fillet remains there, and you may pour the castings in an hour or in five hours after—the melted metal, on coming in contact with the moistened or wetted surface of the mould, immediately produces steam, which later shows its action in a series of blow-holes in the casting.

Mr. Durfee.—It is within my experience that the quality of castings is influenced in no small degree by the behavior of the man who "daubs up" the cupola. It may seem strange that a person charged with that dirty duty should have any influence on the chemical composition of a piece of cast iron. Many years ago I was interested in some very nice No. 1 foundry iron. One of the founders who tried to use that iron said it was "not good for anything, for it 'sand-chilled' an inch thick." This statement was of course exaggerated, but there was no doubt at all that the castings were too hard to be finished. On investigation, I found that they were using a cupola of I do not know how old a construction, in which the holes through the fire-brick and "daubing" intended to act as tuyeres, directed the blast downward at an angle of ten or fifteen or possibly more degrees, and the blast impinging upon the surface of the melted iron, removed a greater or lesser amount of carbon therefrom, the extent of the decarbonization increasing with the time the iron was exposed to the action of the blast. In fact, that cupola was so "daubed up" that its blast acted upon the fluid metal precisely the same as the blast of an old-fashioned refinery fire upon the metal treated therein; the consequence was that castings made from metal melted under the conditions named were always "sand-chilled." I suggested that the tuyeres be turned up so that the blast would go up into the fuel instead of down upon the fluid iron; this they did, and castings made from

the same No. 1 foundry iron, with no change of conditions except the one named of the direction of the blast, were satisfactory in every respect.

Mr. H. F. J. Porter.—I have had some experience with wrought iron, in making a test, which was very much like that suggested in the paper. I believe, however, the general function of cast iron is to resist compression. But the Pennsylvania Railroad, as I understand, requires as a test that a piece of cast iron two inches square and four feet long between supports shall support a weight of 750 pounds. If I am wrong in regard to that, I should like to be corrected. I knew of a turn-table some time ago that required a certain number of track-wheels. It was ordered by the Pennsylvania Railroad, and the founder that made those wheels had great difficulty in getting a cast iron that would stand the strain. They tried very good iron with very poor results. They cast and recast it, and could not get anything to stand the test; and finally they approached it quite nearly by taking the rough cast iron directly from the cupola, and finally they succeeded in getting an iron that would stand the test by making cast iron mixed with quite a quantity of steel chippings, and it seems to me we get in that way a very poor cast iron, even if it will stand the test. It is not a good iron.

Mr. West.—I see no reason why, if a test is made under favorable circumstances, a sample bar should not show what the iron was in the casting.

Mr. Porter.—I see none either. The only thing was that the wheels had to stand a crushing strain, whereas this casting itself stood a transverse strain. This bar is supported at the center and the weight put on it. I wanted to know if that test is a sufficient test to show whether those wheels will stand a good crushing strain.

Mr. West.—That would depend a good deal on what shape the casting was put into—the proportions of the casting and the contraction that would be caused thereby.

CLXI.

A NEW METHOD OF CONSTRUCTING HORIZONTAL TUBULAR BOILERS.

BY FREDERICK A. SCHEFFLER, ERIE, PA.

SINCE the early history of steam boilers, science, in its progress toward a more efficient and, at the same time, a more simply constructed boiler, has been constantly nearing the perfection looked and hoped for, by the many changes for the better which have been made in the rolling of boiler plates, of either iron or steel. It is true that improvements have been made more rapidly in the manufacture of steel plates for boilers than in iron plates, due chiefly to the fact that the steel plates have no grain to be considered in rolling them either lengthways or crossways for their use in boilers, whereas, with iron, the peculiarity of its fibrous structure is an objection to the manufacture of large plates.

Were it not for the superiority (as the writer firmly believes) c steel over iron for use in boilers of all sizes, and the fact that plates from the smallest dimensions to the enormous size of sixteen feet long and one hundred inches wide can be obtained, there could be no possibility of a paper at this time on this subject.

When steel can be obtained, for use in boilers, having a guaranteed tensile strength of 60,000 lbs. per square inch, an elastic limit of 30,000 lbs., an elongation of 20 per cent. in an 8 inch specimen, and a reduction of area from 45 to 50 per cent., and which will also bend down close when cold, without fracture, it is very apparent that the superiority of steel over iron cannot be questioned. Another favorable point is, that this quality of steel is homogeneous and it will not blister.

The fact that these *large* plates of steel can be obtained, caused this question to arise in the mind of the president of the works where the writer is employed: Why not construct horizontal tubular boilers of steel in *two plates* only? The advantages are numerous and of the best kind.

It is well known that attempts have been made, and these attempts have sometimes been successful, to make a boiler of the style in question with a single plate on the bottom, by hammering and swaging into shape. No reliability can be placed on boilers

made by this method, as the iron (when iron is used) loses its toughness and good qualities by being subjected to such a large amount of hammering as must be done to form such a construction. Mr. D. K. Clark refers to some tests which he made on welded

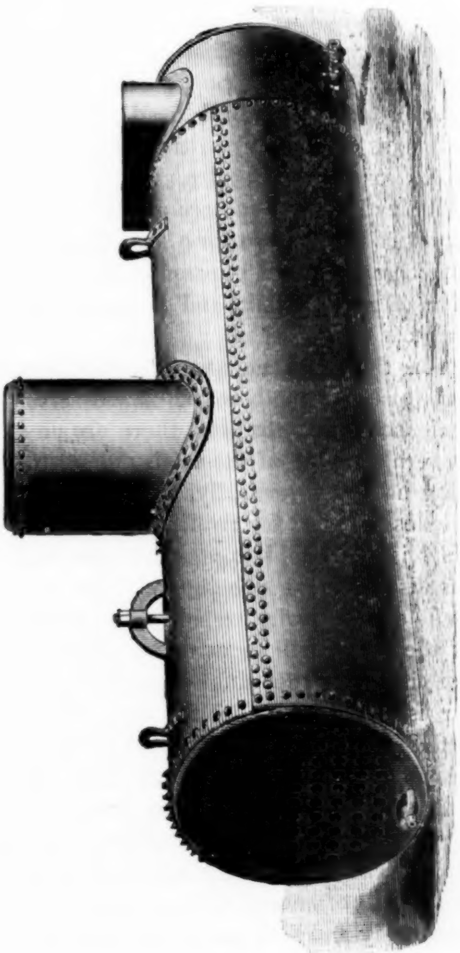


Fig. 33.

iron plates, and although some of the tests were very satisfactory, the danger lest it be not known *positively* whether the weld would be a substantial one or not, causes a reluctance in recommending boilers with welded plates. This method is also very costly,

and with the uncertainty and cost combined taken into consideration, this method has been abandoned.

By making a boiler as shown in Fig. 33 entirely of two plates only (except, of course, the flue sheets), the following advantages are apparent:

I. By carrying the longitudinal seams above the water line (say two inches above the top of the flues) and closing up the brick setting at that height, there will be no seam exposed to the fire other than the lower half of the back head. This is a positive advantage over boilers as commonly constructed, when there are from four to eight seams exposed to the fire.

II. By thus eliminating the exposure of the seams, a greater reduction is apparent of the chances for a leakage around the rivets.

III. The advantage of equal expansion and contraction is so great as to also be of great benefit, and the usual excessive strains are largely reduced.

IV. For cleaning the boiler, no question can be raised as to the benefit of a single plate on the bottom. There are no rivets for lodgment of scale, sediment, etc., and there are no seams to interfere with a thorough scraping of the shell.

V. There being no seams vertically, the boiler must be stronger, as there are no holes punched for rivets.

This latter fact is the one, I apprehend, that will give rise to the most serious discussion of this paper. I am inclined to think some member will question whether one large plate extending the whole length of the boiler will be as strong as the same plate, cut into two or three pieces and then single-riveted together. I believe that the former is the stronger of the two, and if the boiler is properly supported (as all boilers should be) there will not be any trouble with the boiler springing or the plates warping.

A set of hollow rolls 16′ 4″ long, in the clear, and each roll 14″ diameter, and $2\frac{5}{8}$″ thick, was designed some time last spring by the former superintendent. When the writer took charge, it was proposed pushing the matter forward as soon as possible, and patterns were made immediately and then put in the hands of the moulders.

The castings were soon under way, and by the 1st of September the rolls were ready for the first experiment. They were geared to run by belting from the main line running 120 r. p. m., and the speed reduced to 3 r. p. m., on the rolls. Eight-inch single belting was used. The first sheet experimented with was for a 36″ x 10 foot boiler, and the plate was 62″ wide and 10 feet long, by $\frac{7}{32}$″

thick, of steel. The rolls worked somewhat hard at first, being new, and they were allowed to run two days to stretch the belting as much as possible. The stretch was then taken up and the plate entered into the rolls. This plate went through in good shape and so did the top plate—the latter rolling up easier than the bottom plate, because the sheet had been punched for the dome and a hole cut for the man-hole. A larger plate was tried next—a plate 12 feet long, $\frac{9}{32}''$ thick—when it was found that the gearing was insufficient for the work. An alteration was then made, new gears being substituted by which the power was largely increased and 10″ belts were used. The 12-foot plate was then rolled. The amount of power required to roll this immense sheet of steel was entirely beyond expectation. When a 12-foot plate $\frac{5}{16}''$ thick was attempted it was rolled entirely by "coaxing," as the belts would slip, although the pulleys were quite large in diameter and as wide as convenience would permit. The pressure exerted on each roll at its circumference, or at the pitch line of the gears on the rolls (the latter being the same diameter as the gears), is estimated to have been 15,000 lbs., and was supposed to have been sufficient to roll a plate of any size that the rolls would take in.

The idea of belting was at once abandoned and an engine was specially constructed to gear direct.

On the 23d of September, 1884, the first 60-inch boiler, 16 feet long, $\frac{3}{8}''$ thick, of steel, was constructed in two plates. This is the *first* boiler of this description ever made in this country, and as far as the writer has knowledge, in any other country. It is found that a greater degree of accuracy can be maintained in getting the rivet holes in direct line, and thereby the likelihood is greatly reduced that the boiler-makers will have the chance to use the detestable drift-pin, that greatest of all evils in boiler construction.

Some members may have their objections to the use of steel for boilers, and claim that it is so recent in the market that the method of handling it in manufacturing establishments is not yet sufficiently well developed to be relied upon. The writer heard of a gentleman, a short time ago, who objected to the use of steel for boilers for the above reason. The manufacturers who make a specialty of using this material, of course have the advantage over those who use it only on occasional orders; but I do not hesitate to say that, since this material has been in use for upwards of eight years, there has been every opportunity that could be desired for all interested to avail themselves of the proper knowledge of the handling of it.

8

If locomotive boilers constructed of steel should be made with only one course in two plates, from the throat sheet to the smoke-box, there would undoubtedly be a great reduction of the liability to leakage and wear and tear, and probably a longer life of the boiler would be secured, although the fire-box is generally the first place to need repairs.

It is the writer's belief that this new construction of horizontal tubular boilers will be the method of the future for this type, and he hopes that the few points brought forth in this paper will be of service in stimulating discussion and further progress in this direction.

DISCUSSION.

Mr. Stirling.—I notice that this paper speaks of progress in boiler making. We all know that the horizontal tubular boiler has not progressed very much. It is about the same as it was when I was a boy. It will be a matter of surprise to many that an attempt should be made at this late day to improve the construction of this form of boiler, because some of us think that the horizontal tubular boiler is not a good boiler anyway. However, if you ask the engineers in charge of boilers, you will find that they universally prefer horizontal tubular boilers. So far as my inquiries have gone, they prefer them to any other form of boiler in the market, and one of the reasons of their preference is that they have a large area of water surface. Now, one of the things that an engineer in charge of a boiler attends to is his water level, and in most of the water-tube boilers that are built now, this is one great impediment to their usefulness, that the water level changes very rapidly, because it is so limited in area and the engineer is in constant fear of getting into trouble either from shortness or excess of water. The great defect of the horizontal tubular boiler is that it has a shell of such large diameter exposed to the fire. The locomotive engineers are striving for higher pressures of steam, and the shell is the weak point. It is the point that they find difficulty in making strong enough to stand a very much higher pressure than they are carrying to-day. But with all its defects, it seems to me that the horizontal tubular boiler is the best boiler that we have for stationary purposes. I have looked into the matter very carefully, and that is my judgment, and I think that the gentleman who has furnished this paper has done wisely in making improvements in the method of constructing horizontal tubular boilers.

I would like to call attention to a fact, in connection with the boiler business, and that is that the water-tube boilers are not used on steamships, and they are not used largely, so far as my experience goes, for power purposes. I would like to know the reason why. There have been a great many attempts and a great deal of money has been spent in trying to use water-tube boilers on steamships, notably in the case of the steamship Montana. When that vessel first came out, I think that as much as a hundred thousand dollars—I put that as a low estimate—was spent in the attempt to introduce the water-tube boilers on steamships. Now, I should be very glad to know why they are not used on steamships.

Mr. C. E. Emery.—I think we may very easily enlarge the scope of these discussions to such an extent that no other paper can possibly be read. The questions asked by our friend Mr. Stirling are of great interest, but I think they should be brought up in connection with a paper on the subject of shell and sectional boilers, rather than in connection with a paper that shows progress in methods of construction. The possibility of getting large sheets of steel has also made it possible to make the entire lower surface of a boiler of one sheet of metal. All I would add to the paper is in the way of warning in regard to the use of steel. There is a large amount of steel made annually of not the very best quality. There was a large quantity rejected which was furnished for the naval cruisers. The standards there required were very high indeed, as they allowed only 2,000 lbs. difference in tensile strength, required a high elastic limit, and 30 per cent. elongation in eight inches. Unless the material will give 25 per cent. elongation it may be really dangerous. For boiler work it is necessary also to require that the tensile strength shall not exceed 60,000 lbs. I have been very much annoyed in using steel from the fact that the boiler-makers will hammer and torture it to make the seams come together, and thereby set up initial strains which are liable to produce cracks. The real homogeneous steel, which is practically soft iron, will stand it; but if the material be not of the very highest quality, it will be injured by such manipulation. The advances in the manufacture of steel enable us to improve machine and boiler work, but there is need of as much care as ever, for the reason that poor material will be furnished at times which must be excluded by rigorous inspection.

Mr. Stratton.—In response to Mr. Stirling I would like to say that

he is somewhat mistaken in regard to the applications which have been made of water-tube boilers for uses at sea. There are now three vessels in the French navy with water-tube boilers, and one has been at sea for nearly six years, producing better results with higher pressure than have been obtained with the ordinary shell boiler, which is usually adopted in steamship propulsion. The great incentive and first point desired by all marine engineers is to obtain a high-pressure steam generator with the minimum weight of metal. We have no difficulty in using high pressures of steam since the adoption of the compound engine. The difficulty now is to obtain a boiler that will stand high pressure and weigh less than the cumbersome shell boilers in general use. This is only obtainable in the water-tube type, which is the source from which the engineering community expects satisfactory results. The "Voltigeur," of the French navy, has sectional water-tube boilers, and carries 150 pounds of steam. She has been at sea on a three-years' cruise, and it is a matter of record that the results from abuse and neglect on these boilers are less than have ever been seen in those of the ordinary shell type. The liberating surfaces in these generators is less than that generally found in those of the shell type, but that is overcome by mechanical separators. Our own government is now seriously considering the matter of introducing the sectional tubular boiler system, and I have reason to believe that it has sent a distinguished engineer to Europe to look into the matter, with a view of adopting the Belleville or some similar system of boilers in the American navy. On large boilers of the shell type, when made of steel of a tensile strength of 60,000 pounds, it is not possible to carry over 100 pounds of steam, without making them weigh 60 to 65 pounds per square foot of heating surface, while it is possible to carry from 120 to 150 pounds of steam on water-tube boilers upon a weight of 35 to 40 pounds per foot of heating surface. You will find that as regards the steamer "Montana," in which the Guion people adopted water-tube boilers of the Howard pattern, the difficulty experienced there was that they set them in brick furnaces without properly supporting them with casings of iron to spread the strains brought upon the brickwork. The brick walls were very thick, and the boilers lacked the prime necessity of proper circulation.

They were somewhat like the Field tube boiler, but without the circulating tubes common to the Field method. The result was that the tubes soon gave out from lack of water circulation. As

an illustration of what can be done with brick furnaces when properly constructed, I will state that on the Cromwell Line steamship "Louisiana" they had eight boilers of what is known as the Redfield type. After the vessel had run three to four years, the owners found it necessary to remove or renew the legs or water-slabs, and as the job was likely to be exceedingly expensive, upon the advice of their engineer they resolved to line them with brick and shut off all the water connections between the shells and the water-legs. After which, the vessel was sent to sea, and the boilers gave better results with the brick-lined furnaces than they did with their original water-legs connected and acting as direct heating surfaces. After trying the experiment as described, they subsequently concluded to cut the water-legs off and reset the cylindrical shells in brickwork. Mr. Myers Coryell, the engineer-in-chief of the line, deems that the economical and satisfactory results are attributable to the better combustion obtained in the brick furnaces. This would seem to show that this class of furnace is preferable to those composed of slabs or water-legs, both as to durability and economy.

High pressures of steam can be better obtained in water-tube boilers with brick furnaces, and with less weight of material than is possible where shell boilers are used.

Mr. Kent.—As this paper is of considerable historical interest, I regret that it does not contain a statement of when the large horizontal tubular boiler with a single bottom sheet was first made. I know of a battery of ten horizontal tubular boilers, erected two years ago at Beaver Falls, Pennsylvania, and one of Otis steel was exhibited at the Railway exhibition in Chicago. I think Mr. Wellman can give us all the facts on that subject. The paper on page 112 says, "There being no seams vertically, the boiler must be strong." I think that is certainly a mistake. The strain in the direction of the axis of the boiler being only half that in the direction of the diameter, it makes no difference whether the vertical seams are used or not.

The writer of the paper is not quite favorable enough to steel when he says, "I do not hesitate to say that since this material has been in use for upward of eight years, there should be the opportunity desired by all interested," etc. Crucible steel has been used for boilers in this country since 1861, and open-hearth steel since 1870 or earlier. There is now less risk in buying steel than in buying iron for a boiler.

The question of horizontal tubular, and sectional or water-tube boilers I think is out of order in this connection. It is not mentioned in the paper at all, but if any of the gentlemen wish it, I can furnish them privately with an abundance of information which will convince them that water-tube boilers *are* largely used for power purposes.

Prof. Webb.—I would like to supplement Mr. Kent's statement as to the longitudinal strains on the boiler. I presume when there are tubes or flues it will make a difference. Theoretically, in the tubular or flue boiler the strains in the longitudinal direction would be considerably less than half these in the circular direction, on account of the space occupied by the tubes or flues and the consequent reduction of the pressure on the ends of the boiler. There would also be a further reduction on account of the tubes or flues themselves bearing part of the longitudinal strain, but if we take the flues or tubes into account, we must remember that, if they are at a higher temperature than the shell, they may by their expansion considerably increase the longitudinal strain.

Mr. Wellman.—As to the length of time in which a center-sheet has been in use, the Standard Oil Company of Cleveland, to my knowledge, have had them in use for eight years, and I think the first application of this to a boiler was due to the Superintendent, Mr. Andrews. We have had them in use at our works three or four years.

Mr. Partridge.—It may be well to put on record the fact in regard to the boilers of the "Louisiana," that they were not Redfield boilers, though they resembled them in several points of construction. The differences were of a nature to destroy the circulation in the legs, at least.

Mr. Davis.—I think this boiler described in the paper has one very serious practical objection, and that is, it requires brickwork to set it. My idea of a boiler of that class is that it ought to be self-contained. Our company uses a great many plain cylindrical boilers, and they are set in brickwork. The brickwork costs more to build than the boilers, in the first place, and more to keep it in repair afterward. We are looking for a good boiler that would be entirely self-contained to take the place of them. But from the fact that such boilers are almost free from explosion, and when they do explode are so comparatively harmless, we have stuck to them.

Another point in regard to the seams: our seams rarely give

out, although we use very impure and acid water; but it is always the sheet near the seam that gives out from furrowing. It shows most on inner side of outside sheet, just in front of the end of the inside sheet. That is where the boilers give out almost invariably. We have about 2,000 boilers in use, and a great many come into the shop to be cut up, and I have never yet seen an exploded boiler or a used-up boiler that has shown any defects of any consequence in the seams; but I think this long sheet under the bottom of Mr. Scheffler's boiler will avoid a great deal of the wear and tear from furrowing, and add very much to the life of the boiler. I have had occasion lately to examine some new boilers that were exploded by excessive pressure. One exploded last week that was built about six months ago. It was a small mine-locomotive boiler, made out of $\frac{5}{16}$ths best best flange iron all over. That boiler blew up, I am perfectly sure, at over 200 pounds pressure. It was an excellent boiler in every way, so far as a boiler of that kind could be. There was scarcely a rivet broken or pulled out, but in several places the sheets had torn in long ragged lines, like a piece of paper, about an inch or two from the other sheet, perfectly regardless of the fact that the lines of rivets were supposed to be the weakest points. These seams were only single-riveted.

Another point that seems to be against this boiler under discussion is the fact that the severe heating of the bottom sheets, where all the sediment rests, makes this part likely to give out sooner from corrosion than would be the case with an internally fired boiler.

Mr. H. F. J. Porter.—As the boilers of the "Louisiana" have been spoken of, and Mr. Partridge knows something of them, I would like to know if he knows anything about the success of some boilers of a similar type that were put on some of our river tug-boats. I do not think there was any change made in the design of boilers. They were made by Delamater.

Mr. Stratton.—The Delamater Iron Works constructed two or three fruiting vessels with externally fired boilers in them, but I was not aware that they had constructed any tugs with boilers of this kind, and if there are any tugs running in the harbor with externally fired boilers without water-legs, I am not aware of it.

Mr. Partridge.—I can only speak from hearsay in regard to the question asked by Mr. Porter, but I think that all the tug-boat boilers built on the lines of the "Louisiana's" so-called Redfield boilers have met the same fate. The water-legs have fille

up with mud from a lack of circulation, and they have been generally rather expensive and unsatisfactory. I do know that, on the other hand, the Redfield boilers, where they have been, both on the Sound, on the Hudson River, and in this harbor, have given good satisfaction, remaining clean, having a good circulation, and proving very economical. A very slight modification of the original design cuts off the circulation, rendering the water-legs useless, and causing a rapid deposit of mud in them.

Mr. C. T. Porter.—I would like to ask why boilers have not been made so as to avoid the use of longitudinal seams.

Mr. Barnes.—One reason, and a very important one, why seams have not been commonly welded is that a thoroughly good joint can be made by riveting. That may seem to beg the question, but there can be no doubt as to the fact. A riveted seam, if made by reasonably skillful hands and under close inspection, can be known to be a certain thing. There is no question about it whatever. My impresssion is that in the present state of the art a certainty of the soundness of the work can be had more cheaply by riveting than by any method of welding.

Mr. Le Van.—Why not form the rims of our shell boilers in a single rim, in the same manner as steel tires for locomotive drivers are now made, as designed by Mr. John Ramsbottom?

Mr. Davis.—I think the remark Mr. Barnes made is an excellent one in a practical way. I was going to say that where we have forging to do upon which men's lives depend, we resort in a great many cases to riveting pieces of iron together instead of incurring the risks of welding, even under the most favorable circumstances.

Mr. Stirling.—If all remarks in reference to water-tube boilers are not out of order, I would like to ask Mr. Kent to tell us of any steamship of any size or any locomotive that is equipped with water-tube boilers.

Mr. Kent.—The remark I made was that the gentleman was mistaken in the statement that water-tube boilers were not used largely for power purposes. Mr. Stratton mentioned the name of one vessel in the French navy, the "Voltigeur," which has used water-tube boilers for six years, and there are some two other vessels, the names of which I do not know, that have those boilers. It is impossible, I suppose, to use water-tube boilers on locomotives, since on a locomotive every other consideration must be sacrificed to the one of portability. As regards stationary power

on land, there is no question that the water-tube boiler is the boiler of the future, and it is used already, in the world at large, to the extent of somewhere about half a million horse-power.

Mr. Stratton.—I would like to ask if any gentleman present can inform me of the difference in the thickness of these sheets between the center portion of them and the edge.

Mr. Wellman.—With sheets 100 inches wide, we find a difference of nearly one-sixteenth of an inch.

President Sweet.—How large are the rolls that you are now using for that purpose?

Mr. Wellman.—Thirty-one (31) inches in diameter, but they are made of steel. The tests all show that the steel is four times as strong as the best chilled-iron rolls we can get.

CLXII.

THEORY OF THE SLIDING FRICTION OF ROTATION.

BY ROBERT H. THURSTON, HOBOKEN, N. J.

SLIDING FRICTION in systems of mechanism gives rise to losses of energy and to increase of resistance which are usually very considerable, and which are often the only sources of "lost work," and of the reduction of efficiency below its maximum value—unity. In any well-proportioned and properly managed machine or train of mechanism, the wastes of energy are due solely to friction; in badly-proportioned mechanism, subject to overstrain or shock, causing deformation of parts, energy is lost equal in amount to the work expended in producing permanent distortion. But as the engineer generally only considers cases of correct construction in determining the magnitude of such waste, it may be asserted as a general and fundamental principle that in all good engineering the sole cause of waste of mechanical energy is friction. These facts and this principle sufficiently indicate the importance to the engineer of a careful study of the magnitude and of the method of such losses.

The writer has elsewhere * given the results of experimental determinations of the values of the coefficients of sliding friction and has shown that the lost work in mechanism varies very greatly in amount; that it is, under favorable circumstances, vastly less than has been generally supposed; and that it is influenced, where lubrication is employed, by every change of velocity of rubbing, of temperature, and of intensity of pressure, as well as by the character of the lubricant.

In machinery and mill-work, and in mechanism generally, it is usually found that the wastes of energy caused by sliding friction are principally due to friction of rotation, *i. e.*, to the friction of shafting, of journals, and of pivots in their bearings. This form of friction, and this method of waste, are the subjects of the paper here presented. The friction of straight-sliding pieces has been frequently studied and is well understood; that of rotation has been less fully investigated, although considered at some length in a few

* *Friction and Lubrication*, RR. Gazette Pub. Co., N. Y., 1878. *Materials of Engineering*, Part I., 2d Edition, J. Wiley & Sons, N. Y., 1884.

treatises. The importance of the subject is such, however, as will justify the most extended examination.

THE FRICTION OF JOURNALS causes a waste of the energy traversing a machine, which is dependent in amount upon the velocity of rubbing, the magnitude of the load, the method of distribution of the resulting pressure and of variation of its intensity, the size of the journal, the nature of the lubricant and its physical condition, especially as determined by its temperature, and on the value of the coëfficient of friction as determined by all these modifying conditions.

A journal in good order, well-fitted, and smoothed down by prolonged working, should never show evidences of measurable wear. Such journals have sometimes been known, by the writer, to show no appreciable alteration of form or fit, after years of constant work. Two cases may give rise to such permanence of form and fit.

The First Case of permanent fit of journal is that in which the journal is exactly fitted to its bearing when new, and so perfectly adjusted that no sensible wear takes place. In this case, the pressure brought upon the rubbing surface by the load is distributed by a certain method of yielding of the loaded metal, and according to a simple and definite law, the investigation of which is one of the principal objects of this paper. Under this law, an adjustment takes place which, if the bearing surfaces are sufficiently large, of good material, and well lubricated, is permanent.

The Second Case is that in which wear occurs, and until all parts of the journal and bearing in contact wear until a "fit" is thus attained, such that every part is compelled to carry equal pressure, and until wear ceases to be observable.

The Third Case to be considered is that in which the journal is loose and the bearing surface, or surface of actual contact, is assumed to be a straight band of inconsiderable width.

These are the three typical cases for cylindrical journals, and these cases only are to be here studied. Actually, in practice, probably, neither case is often met with; the real condition of the journal is usually intermediate between one or the other pair of these type-examples.

It is evident, from what has been just stated, that the friction of journals, other things being equal, depends greatly for its amount upon the method of distribution of pressure, and is not necessarily, and probably may not be often, measured exactly by the product of the load on the journal by the coëfficient of friction. It may be

sometimes, and in fact is, very much greater. It thus becomes an important problem to determine the manner in which variation of intensity of pressure affects the total resistance due to friction, and the method of that variation as produced by the different modes of fitting journals.

THE WORK DONE AND HEAT DEVELOPED, by the friction of any journal, is measured by the product of the total normal pressure on its rubbing surfaces into the mean coëfficient of friction, and into the space traversed by the surface of the journal, relatively to the surface of the bearing, in the given time.

If p represent the mean intensity of pressure, l the length of journal, θ the angle over which the pressure is distributed, r_1 the radius of the journal, f the mean coëfficient of friction,* and n the number of revolutions made in the given time, the work of friction will evidently be

$$U = p \cdot l \cdot r_1\, \theta \cdot f \cdot 2\, \pi\, r_1 n$$

$$= 2\, \pi\, r_1^2 f\, p\, l\, n\, \theta \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

When $\theta = \pi$, as in a journal receiving no pressure from its cap,

$$U = 2\, \pi^2\, r_1^2 f\, p\, l\, n;$$

and when $\theta = 2\, \pi$, as when the cap is screwed down hard,

$$U = 4\, \pi^2\, r_1^2 f\, p\, l\, n.$$

When the load is fixed, the intensity of pressure varies inversely as the area of the journal, and $p\, l\, r_1\, \theta$ becomes constant. Hence, in such cases, the resistance of friction, $f\, p\, l\, r_1\, \theta$, is constant, and the work of friction and energy wasted becomes proportional to the diameter of the journal; while both quantities are independent of the length, except so far as it affects the coëfficient of friction. Journals are therefore properly proportioned by making their diameter such as is dictated by considerations of strength and safety against "springing," and determining their length by reference to loss of work by friction and the liability to heating.

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* With good lubrication, it may be assumed that, in heavy machinery and under pressures not far from 100 pounds per square inch (7 kgs. per sq. cm.), the value of f may be reduced below one per cent., varying approximately inversely as the square root of the intensity of pressure.

The minimum limit, as to length, is set by this last consideration. As has been fully shown by the writer, the coëfficient of friction increases with increase of bearing area and consequent decrease in intensity of pressure; the conclusion therefore follows that bearings should be as short as is consistent with security against heating. This conclusion is the more important from the fact that a long journal is liable to spring, and thus to concentrate pressure at the end, and to cause heating in that way. With every diameter of shaft, therefore, there is a limit beyond which no increase of length of journal will prevent heating. With a given shaft, heating under a given load, no increase of diameter will reduce liability to heat—if the shaft does not spring—and increasing its length may afford advantage only up to a limit.

THE HEAT DUE TO FRICTION, H, is proportional to the work done on the journal, and is measured in thermal units by the quotient of that work U, by the mechanical equivalent of heat J, *i. e.*,

$$H = \frac{U}{J} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

This heat is carried away by conduction to adjacent masses, and by radiation. If it is carried away as rapidly as it is produced, the journal remains cool; if not thus carried off, the journal heats up until the rate of dispersion is equal to that of production, or until it becomes necessary either to apply cooling agents or to stop the machine.

THE TEMPERATURE ATTAINED by a journal when thus heating is limited by the facilities for conduction and radiation of heat from it. The maximum *safe* temperature is that beyond which liability to rapid and dangerous heating is incurred, and is always below the temperature of decomposition or vaporization of the lubricant. A warm journal will often work better than a cold one; a hot bearing is always a source of danger. A journal so proportioned as to run warm with good lubrication is to be watched with the utmost care. As already seen, since the tendency to heat increases directly as the intensity of pressure and as the amount of work done, and inversely as the area across which the heat can be transmitted, the diameter of the journal, if it be sufficiently strong and stiff, does not affect this phenomenon.

CASE 1.—THE PERFECTLY FITTED JOURNAL is the most interesting of the three cases here to be considered. When a journal

is exactly fitted to its bearing,* as is usually the fact, without pressure, the action of the load will cause a minute change of form, which, although quite imperceptible, will produce a variation of intensity of pressure between the rubbing surfaces, from a maximum on the portion normal to the line of direction of the resultant load, to zero on the surfaces parallel to that line, and according to a simple and easily determined law.

This Method of Distribution of Pressure, which was, as he believes, originally investigated by the writer, is determined in the following manner:

The maximum intensity of pressure under which any journal of good form and correct proportions may be worked, is from 500 pounds to the square inch, with iron journals, to about 1,000 pounds with steel (35 to 70 kgs. per sq. cm.); the elastic limit of the bearing metal is always far above these figures, and that of the metal of the journal usually very much higher still. The compression of the metal, under working pressure, will be proportional to the intensity of the pressure at each point, and this principle will determine the law of distribution of pressure. The intensity of pressure will be everywhere proportional to the elastic displacement of the metal.

Let p represent the intensity of pressure on any element of the surface of the journal having a length l, and a breadth $r_1\,d\theta$, and let N represent the normal pressure on this elementary area $l\,r_1\,d\theta$; then

$$N = p\,l\,r_1\,d\theta \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

The sum of all the vertical components of these elementary normal pressures, $p = N \cos\theta$, where θ is measured from the vertical, is equal to the load W on the journal; *i. e.*,

$$W = \int p\,l\,r_1 \cos\theta\,d\theta \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

But the normal pressure p varies from a maximum, at the bottom of the bearing, to a minimum at the sides, there becoming zero when the bearing is a semi-cylinder. At intermediate points the normal pressure is

$$p = p_1 \cos\theta,$$

*The custom now common of grinding journals to size, and sometimes of scraping the bearing, makes this an increasingly frequent case.

in which quantity p_1 is a constant, the value of which is to be determined. The value of p being obtained, and introduced into the formula,

$$W = p_1 \, l \, r_1 \int \cos^2 \theta \, d\theta \, ; \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

which is to be integrated between the limits

$$\theta = + \frac{\pi}{2}, \qquad \theta = - \frac{\pi}{2},$$

to obtain W in terms of p_1, when the journal bears upon a semi-cylindrical surface, or between the positive and negative values of θ for any other method of bearing. From the relation thus obtained the value of p is deduced. It will be seen that p_1 is the measure of the maximum intensity of pressure between the rubbing surfaces, occurring when $\theta = 0$, *i. e.*, at the bottom line of the bearing.

To Determine the Maximum Pressure p' on the journal, let $A\ C\ B$, Fig. 26, represent the trace of the bearing surface upon the vertical plane, the journal being perfectly fitted and unloaded. When the load comes upon it the journal will sink a minute distance, $O\ O'$, $C\ C'$, $A\ A'$, $B\ B'$, and will rest in equilibrium between the elastic resistance to compression, thus brought into play in the metal, and the weight sustained by the bearing, the surfaces of contact taking the new position $A'\ C'\ B'$, of which every point now lies directly under its original position, and at a distance from it equal, at every point, to $O\ O'$. The compression of the metal in the direction of the normal pressure p, will be at each point, as E, equal to $E\ I$. The product of this vertical displacement, $E\ J$, into $\cos\ \theta$; and the intensity of pressure, the elastic limit of the metal not being exceeded, will be proportional to this compression, varying from 0 at B' to a maximum p' at C'. At any intermediate point, the compression is, for a small total depression of the shaft $\overline{OO'}$, $\cos\ \theta$, and the intensity of pressure, consequently, $p' \cos\ \theta$.

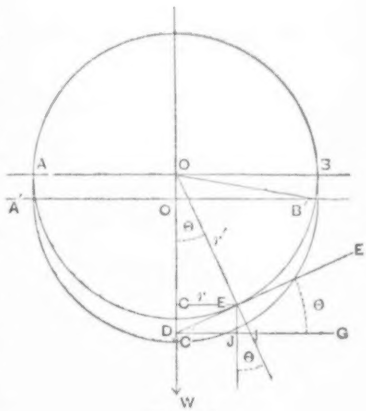

Fig. 26.

Since the load, W, is sustained by the sum of the vertical component of these normal pressures, each of which is of the intensity,

$$w = p \cos \theta = p_1 \cos^2 \theta,$$

$$W = p_1 l r_1 \int_{-\theta'}^{+\theta'} \cos^2 \theta \, d\theta,$$

$$= p_1 l r_1 \left[\tfrac{1}{2} \theta' + \tfrac{1}{4} \sin 2 \theta \right]_{-\theta'}^{+\theta'}$$

as before, and

$$p_1 = \frac{W}{l r_1} \div \left[\tfrac{1}{2} \theta + \tfrac{1}{4} \sin 2 \theta \right]_{-\theta'}^{+\theta'} \quad . \quad . \quad . \quad . \quad . \quad (6)$$

When $\theta' = \frac{\pi}{2}$,

$$p_1 = \frac{W}{1.57 \, l r_1} = \frac{0.64 \, W}{l r_1};$$

and the pressure at any point, E, of the journal, has the intensity,

$$p = p_1 \cos \theta = \frac{0.64 \, W \cos \theta}{l r_1} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

When $\theta = \frac{\pi}{3}$,

$$p_1 = \frac{W}{1.48 \, l r_1}; \; p = \frac{2}{3} \frac{W \cos \theta}{l r_1}, \text{ nearly}; \quad . \quad . \quad (8)$$

and the maximum pressure is nearly equal to two-thirds the load on the journal divided by its projected area on the horizontal plane, and is very nearly the same as in the previous case, although one-third the bearing surface has been removed. This is often done by engineers in charge of heavy machinery, to prevent risk of "gripping" or "binding," should the bearing heat.

It is evident that the same method applies to the case of the spherical journal.

The Extent of Compression is determined by the magnitude of the modulus of elasticity of the metal compressed, thus, for a pressure of 1,000 pounds per square inch (70 kgs. per square cm.) on a bronze bearing, the maximum compression would be but $\frac{1}{12000}$ the thickness of the "brass."

The Force of Friction, at any element, E, is, for the full journal,

$$f\,p = \frac{0.64\ W \cos\theta}{l\,r_1} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

and varies from zero at $\theta = 90°$ to a maximum, at $\theta = 0$, when

$$f\,p_1 = \frac{0.64\ W}{l\,r_1}.$$

The Total Pressure on the bearing is

$$P' = 0.64\ W \int_{\theta = -\frac{\pi}{2}}^{\theta = +\frac{\pi}{2}} \cos\theta\, d\theta$$

$$= 0.64\ W \left(2 \sin \frac{\pi}{2}\right)$$

$$= 1.27\ W \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

The total Force of Friction is

$$f\,P' = 1.27\,f\,W \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

The Moment of Friction is

$$M = f\,P'\,r_1 = 1.27\,f\,W\,r_1 \quad . \quad . \quad . \quad . \quad (12)$$

The Work of Friction is

$$U = M\,a = 1.27\,f\,W\,a\,t\,r_1$$

$$= 2.5\,f\,n\,r_1\,t\,\pi\,W \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (13)$$

where a is the angular velocity of the shaft, t the time taken, and n the revolutions made in the unit of time.

The Power lost in Friction is

$$U \div 550\,t = 2.5\,f\,n\,r_1\,\pi\,W \div 550 \quad . \quad . \quad . \quad . \quad (14)$$

when the units are British and the time is measured in seconds.

As will be seen later, the resistance, the work done, and the power wasted, are 1.3 times as great as in a journal loosely fitted, in which the rubbing surfaces are in contact only along a narrow band parallel to the axis of the journal. The case above considered

9

assumes a fit originally and no subsequent wear. In such case, the journal must be of such size that the maximum pressure, p_1, may be below that at which the unguent is liable to be forced out, or heating to occur.

CASE 2.—UNIFORM PRESSURE ON THE RUBBING SURFACES may be observed in cases of journals so small as to wear slowly under their loads, or, perhaps, when heating, as is often the case, causes the "brass" to grasp the journal by springing the sides inward. Both are familiar cases to every mechanical engineer who has had much experience, especially if accustomed to the management of steam machinery.

In well designed machinery, a bearing is usually composed of a softer metal than the journal which it supports; it therefore takes the wear, and if the extent of rubbing surface is small the journal is merely "smoothed up," while the bearing wears down. If the surface is too small, the bearing may be abraded and "cut," and both it and the journal rapidly injured. If, however, the surface under pressure does not cut, wear takes place slowly, and without excessive waste by friction. Every bearing surface, if not abraded, will, whether fitted or not, wear under heavy pressure, but with decreasing rapidity, until all parts sustain a certain intensity of pressure, when the rate of wear becomes a minimum, under a pressure which is a minimum for that bearing under the existing conditions as to lubrication. In some cases, the whole bearing surface may not be brought into play, the uniform pressure so established by wear over a part of it being sufficient to carry the load without further wear. These two cases are probably the most usual in all cases of heavily loaded, or carelessly fitted, journals, as the preceding case represents the usual case of well-fitted, lightly, or fairly loaded bearings. In every case, there is a certain pressure-limit, above which wear will take place and below which it becomes inappreciable; the bearing will therefore wear down until the pressure due the load is so distributed that this pressure-limit is everywhere reached over a certain limited area, and wear ceases, or until all parts of the bearing are brought to a state of minimum wear under a uniformly distributed pressure.

In the case here studied, the pressure is of uniform intensity, p_1; that on any elementary strip of bearing, of length l and breadth $r_1\,d\theta$, is $p_1\,l\,r_1\,d\theta$; its vertical component is $p_1\,l\,r_1 \cos\theta\,d\theta$, and the total load is, for a semi-cylindrical journal,

$$W = p_1\, l\, r_1 \int_{\theta = -\frac{\pi}{2}}^{\theta = +\frac{\pi}{2}} \cos\theta\, d\theta$$

$$= 2\, p_1\, l\, r_1 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (15)$$

and the intensity of the uniform pressure attained is

$$p_1 = \frac{W}{2\ l\, r_1} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (16)$$

The Total Pressure on the journal surface is

$$P' = p_1\, l\, \pi\, r_1$$

$$= \tfrac{1}{2}\, \pi\ W = 1.57\ W \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (17)$$

or 0.57 greater than the load.

The total Force of Friction is

$$f\,P = 1.57\,f\ W \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (18)$$

The Moment of Friction is

$$M = f\,P'\, r_1 = 1.57\,f\, r_1\ W \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (19)$$

The Work of Friction is

$$U = M\,a = 1.57\ a\,f\,t\,r_1\ W$$

$$= \pi^2 f\,n\,t\,r_1\ W,$$

$$= 10\,f\,n\,t\,r_1\ W, \text{ nearly} \quad . \quad . \quad . \quad . \quad (20)$$

or 1.57 times that on a flat surface, or on a loosely fitting journal.

Were the angle of contact reduced, making the angle $\theta = 30°$, the friction becomes but five per cent. greater than that of an equally loaded flat surface, or of a loose journal. It is thus seen that, where ample area of bearing surface can be secured, it is best given the form of a strip or band lying along the bottom of a long "brass," rather than made to cover the full semi-circumference of a shorter journal.

CASE 3.—A LOOSELY FITTING JOURNAL, *A B C*, before wear has produced a sensible widening of the line along which contact orig-

inally takes place between journal and bearing, when in operation, carries its load at a line parallel to the axis of the journal, and at one side of the line of vertical resultant pressure (Fig. 27). If at the lowest point, B, at starting, it rolls up the side of the

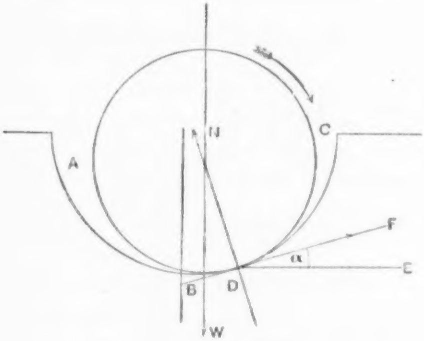

FIG. 27.

bearing until, at some point, D, the inclination becomes equal to the angle of friction $B\ N\ D$, $E\ D\ F$, when it ceases its upward movement and continually rotates, sliding on the line whose trace is D, as long as the coëfficient of friction remains constant, or rising with increasing, and falling with decreasing, friction, continually finding new positions of equilibrium until motion ceases, or the conditions again become constant.

At any one instant there are three forces in equilibrium, the weight W, on the journal, the normal reaction, N, of the bearing, and the resisting force of friction, $F = f\ N$, which may be represented by the sides of the triangle $N\ B\ D$. Then, since N and F are at right angles,

$$W^2 = N^2 + F^2$$

$$= (1 + f^2)\ N^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (21)$$

The Normal Pressure is

$$N = \frac{W}{\sqrt{1 + f^2}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (22)$$

The Force of Friction is, since $\alpha = \varphi$, the angle of friction,

$$F = \frac{f\ W}{\sqrt{1 + f^2}} = \frac{W \tan \varphi,}{\sqrt{1 + \tan^2 \varphi}} = W \sin \varphi. \quad (23)$$

The Moment of Friction is

$$M = F r_1$$

$$= W r_1 \sin \varphi$$

$$= \frac{f W r_1}{\sqrt{1 + f^2}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (24)$$

The Work of Friction, or energy wasted, is

$$U = W a t r_1 \sin \varphi = \frac{f W a t r_1}{\sqrt{1 + f^2}}$$

$$= 2 W \pi n t r_1 \sin \varphi = \frac{2 W f n t r_1^2}{\sqrt{1 + f^2}} \quad . \quad . \quad . \quad . \quad (25)$$

This case is thus seen to be that producing least waste of work and energy, and hence, in that respect most desirable; but it is evidently a purely ideal case which can never be obtained in practice. If approached too closely, rapid wear, heating, and all the consequent cost and damages are increased. Case 3d is that of minimum friction, but of maximum wear and heating, as Case 1st that of minimum wear, and Case 2d that of maximum friction. In practice, the best arrangement is usually, so far as the writer can judge from his own experience, that of freeing the sides of the bearing by cutting them away so as to clear the journal entirely to an angle of at least 30° from the horizontal, and even 60° for very long, stiff journals, moderately loaded. This gives a modification of either Case I. or Case II., and on the whole, minimum friction and wear.

IN CONCLUSION, it may be considered proven that:

(1.) A perfectly fitted cylindrical or spherical bearing of ample area of rubbing surface, will, in the absence of wear, have the pressure distributed in such manner as to make it zero at the sides, and a maximum at the bottom of the bearing, varying at intermediate points as the cosine of the angle included between the given point and the bottom of the journal.

(2.) The friction on such a cylindrical journal, and the lost work, exceed by above one fourth that of the ideal case of bearing on a line, or that of flat surfaces under the same load.

(3.) A bearing so proportioned as to wear constantly, but without "cutting," will be subjected to uniform pressure, throughout the area of rubbing contact.

(4.) The friction on such a bearing is nearly 60 per cent. in excess over that of the ideal case, or on a flat surface similarly loaded; it should always be relieved by freeing the sides from pressure, in the manner often practiced by engineers, as above described.

(5.) Any journal, wearing smoothly and symmetrically, and without shake, will in time wear to a fit, such that the pressure upon its surface will be of uniform intensity throughout the whole area brought into bearing, and the rate of wear reduced to a minimum, while the waste by friction becomes a maximum.

(5.) Maximum efficiency of machinery in which journal friction is the main source of waste of work and energy, is secured by giving the journals such diameter that they will neither twist nor spring under their loads, such length that the load may be carried principally on the lower portion of the bearing, and such form that the "brass" shall not bind or grasp the journal, or in any way subject the journal to serious lateral pressures. All lateral pressure due to grasping or binding action decreases efficiency.

The proper size of journal is, as is evident from what has preceded, determined only in part by the consideration of the amount of power and energy lost by its friction. Since to increase or diminish the diameter of a journal increases the speed of rubbing in precisely the same proportion in which it diminishes the intensity of pressure produced by any given load, it is evident that the work wasted and the heat produced on the unit of area of bearing surface is approximately the same, whatever the diameter of journal, within moderate limits: for, within such limits, the coëfficient of friction may be considered as constant. The *diameter* of journal is therefore determined by the strength and stiffness demanded, and not by the liability to heat, and is calculated by the rules for strength of materials.

The *length* of a journal is determined by reference to the laws of friction. A convenient method is to fix, by reference to experiment, a quantity of work of friction which may be safely allowed for the proposed journal, reckoned per unit of its area. Thus, the writer has found for the crank-pins of marine engines, under intermittent pressure, $60,000 f$, measured in foot-pounds, to be a good allowance; Rankine found $44,800 f$ to be a good figure for locomotive practice: $50,000 f$ is an intermediate figure sometimes taken

for general practice. For unintermitted pressure, still smaller values must be taken.

Then the work of friction is nearly, per unit of area,

$$\frac{f\,P\,V}{l\,d} = 60{,}000\,f \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (26)$$

and

$$\frac{P}{l\,d} = p = \frac{60{,}000}{V} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (27)$$

Thus the intensity of pressure is limited by the velocity of rubbing, and the given load, P, on the proposed journal being divided by this pressure, p, the length must be at least

$$l = \frac{P}{p\,d} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (28)$$

in which expression d is known already by calculations of strength.

(*This paper received joint discussion with Mr. Woodbury's which follows it.*)

CLXIII.

MEASUREMENTS OF FRICTION OF LUBRICATING OILS.

(*Second Paper.*)

BY C. J. H. WOODBURY, BOSTON, MASS.

At the meeting of this society held in November, 1880, the writer presented a paper under this same title,* giving the comparative results of some measurements of friction upon a variety of lubricating oils, submitted to a somewhat narrow range of conditions. On this occasion it is proposed to treat the subject from a different, but perhaps equally practical, point of view, and limit the subject to the examination of a single lubricant under a wide range of investigation.

In the course of some work on this subject for the Factory Mutual Insurance Companies, it became a matter of importance to know the coefficient of friction of a lubricant at a series of temperatures and pressures. These measurements were made upon another machine designed by the writer, similar in principle, but differing from the one used in the previous experiments in its general construction. The earlier machine was made for the specific purpose of testing spindle oils, and fulfilled conditions of high speeds and light pressures in a satisfactory manner, but was unsuited for work with heavy pressures upon the standard bearing where the friction was measured. A standard brand of mineral oil, free from admixture of any animal oil, was selected for these experiments, because previous experience had shown that it was more uniform than any other lubricating oil, and duplicate samples could be obtained when desired. A test of this oil showed:

Flash	342° Fahrenheit.
Fire	410° Fahrenheit.
Evaporation by exposure to 140° Fahr. for twelve hours	.02
Specific gravity	.888

The operation of the machine is based on the principle of measuring the friction between two annular discs, and the whole designed for the purpose of observing this with precision.

* Measurements of Friction of Lubricating Oils. Transactions A. S. M. E., vol. i., p. 73.

The machine, shown in perspective in Fig. 34 and in elevations in Figs. 35 and 36, consists of a cast-iron frame in the form of an arch, with a brace at the rear, and further stiffened with transverse

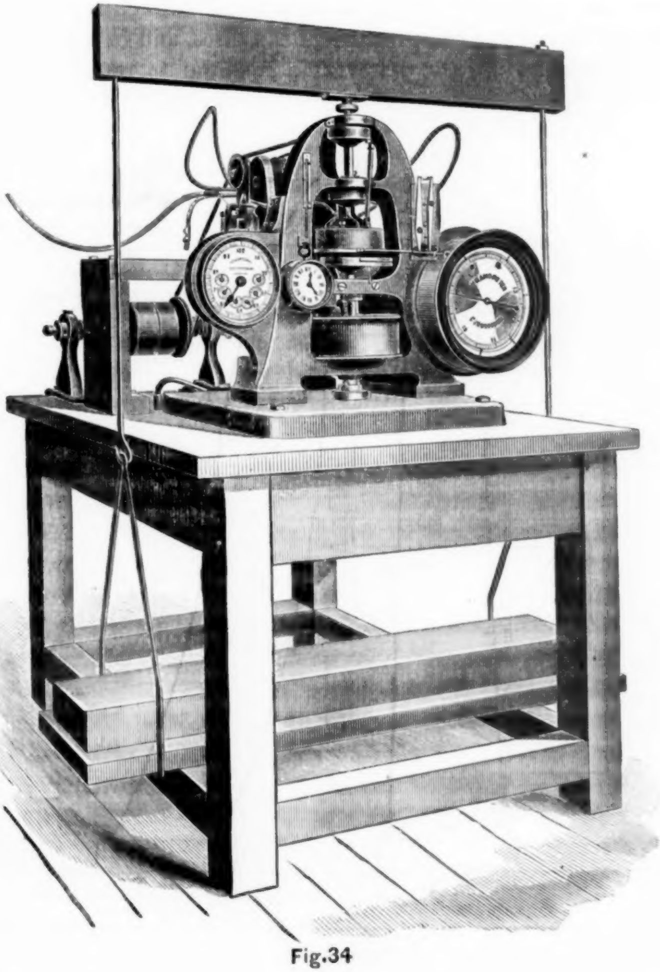

Fig. 34

webs arranged to present the utmost rigidity against the stresses liable to be applied to the machine.

The lower disc is secured upon the top of an upright shaft, its top being an annulus, ground to a true plane surface. Upon this rests the upper disc, which is in the form of a hollow ring based

upon a flat plate, and is made of very hard composition, cast in one piece. The bottom of this disc is scraped to a true plane surface, so that the contact between these two discs is uniform.

A partition divides the interior of the hollow ring forming the upper disc, so that water can be introduced through the connecting tubes to control the temperature of the discs, and in some instances it is desired to use the water as a medium to retain the heat of

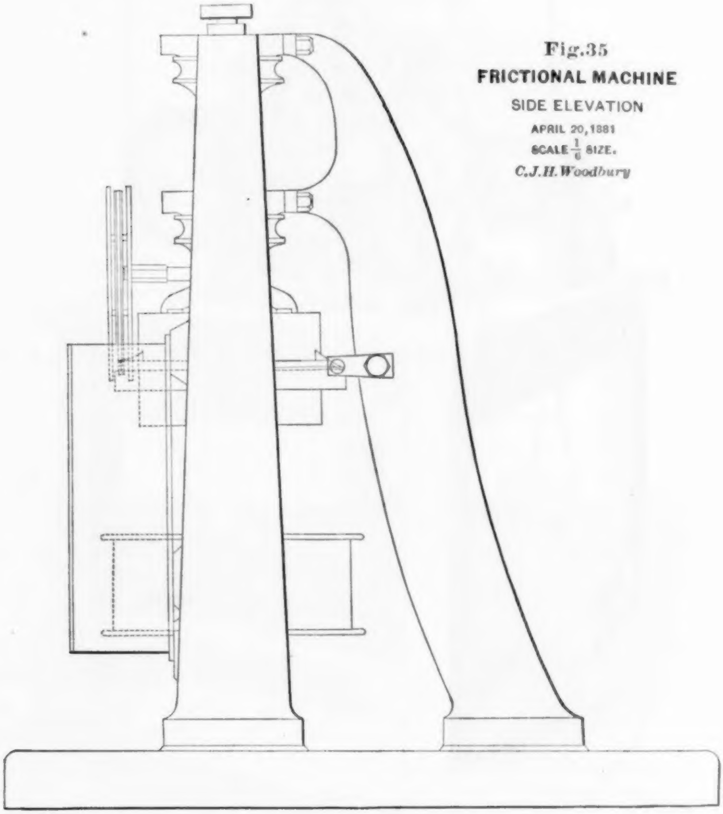

Fig.35
FRICTIONAL MACHINE
SIDE ELEVATION
APRIL 20, 1881
SCALE $\frac{1}{6}$ SIZE.
C.J.H. Woodbury

friction. The sides and top of the upper disc are surrounded by a case made of hard rubber, and the space filled with eider down.

In experimenting, ice-water is generally used to reduce the temperature of the discs to nearly the freezing point of water, and then the friction is noted at each degree of the rise in temperature due to the heat of friction.

A tube of thin copper, closed at the bottom, reaches through to the bottom of the disc, and a thermometer with its bulb placed within this tube indicates the temperature of the frictional surface. A tube leading through the upper disc conducts the lubricant under trial to the recess in the middle of the lower disc. The upper end of this tube, being of glass, indicates the supply and

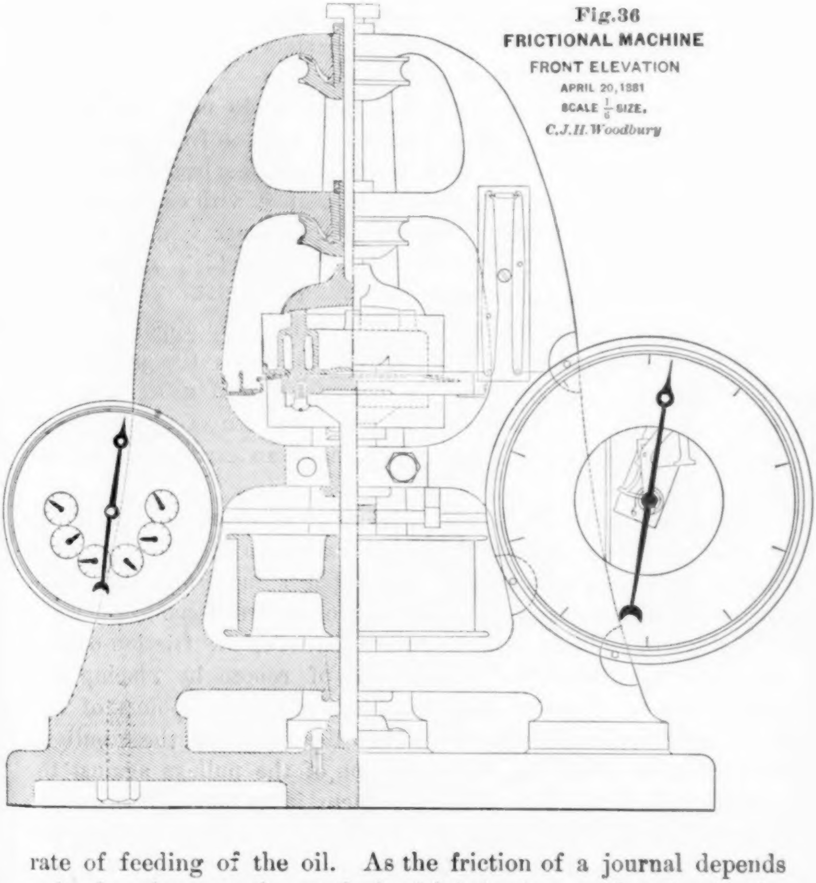

Fig.36
FRICTIONAL MACHINE
FRONT ELEVATION
APRIL 20, 1881
SCALE $\frac{1}{6}$ SIZE.
C.J.H. Woodbury

rate of feeding of the oil. As the friction of a journal depends quite largely upon the method of lubrication, uniformity in the manner of supply is of the utmost importance.

Over the upper disc, a yoke with four arms rests upon four columns which extend through the upper disc to the middle of the frictional surfaces; these columns being cast as a portion of the disc. In the center of this yoke is a hole with hemispherical bot-

tom. The lower end of the upper spindle is round, and fitting into this hole makes a ball and socket joint. This construction transmits the stress due to the weight applied upon the spindle to four points in the middle line of the frictional surfaces, and the strains due to excessive loads will not distort the discs so as to interfere with the uniformity of the thickness of the film of oil between the surfaces; while the ball and socket joint allows the surfaces to meet without any cramp or binding due to imperfection or wear which would prevent the surfaces from revolving in a true plane.

The axes of the upper and lower spindle do not coincide, but are on parallel lines about one-eighth of an inch from each other. This prevents the surfaces from wearing in rings, because the same points are not continuously brought in contact with each other.

A slight counter-sink in the top of the upper spindle receives the center-point in the middle of a beam which sustains the weighted platform beneath the table. The weight pressing the discs together is thus exerted along the axis of the upper spindle.

It has been found that it is essential to obtain the pressure by the direct application of weight, for any plan of using weighted levers or springs upon a disc requires the use of an additional point of support whose friction introduces an error which cannot be measured.

The upper disc with its load must be free to turn slightly in the easiest possible manner with slight changes in the friction. The use of knife edges to support the upper spindle was out of the question, because it must be sustained in all directions.

Instead of holding it in ordinary journals, the friction of motion was substituted for the friction of repose by placing two pulleys whose arbors were long sleeves, at the two points of support, and running the spindle through the middle of these pulleys. The torsional effect due to the friction of the pulleys against the spindle was nullified by revolving them in reverse directions, so that the friction of motion due to one thousand revolutions per minute was substituted for the greater friction of repose. The friction of the two pulleys is so nearly in equilibrium that the spindle could be sustained on a smooth surface, without the friction of one pulley exceeding the other enough to turn the upper spindle; that is, the frictional couples at the supports neutralized each other.

At the left of the machine, a counter records the number of

revolutions made during any given time. A lever at the top controls a small friction clutch in order to stop or start the counter at any time during an experiment.

Under certain conditions, the friction varies so rapidly that the dynamometer measuring it must be instantaneous and automatic in its action.

The dynamometer shown on the right hand of the machine consists of a mechanism of segments and pinions for multiplying the deflection of a steel bar, and indicating the stress necessary to produce such deflection, by the position of the hand on the dial. An arm which ends in the arc of a circle projects from the lower surface of the upper disc, and is connected to the dynamometer with a flexible brass tape. When the machine is in operation, the lower disc is revolved, and tends to carry the upper disc around with it, by a force equal to the friction due to the lubricant between the discs.

The frictional resistance is obtained from the dynamometer by the principle of couples of equal moments. The reading on the dynamometer indicates the force of a couple whose arm is the length of the lever projecting from the upper disc, and this couple is opposed by a couple of equal moment, of which the dimensions of the frictional surface form the data for computing the arm, and the frictional resistance of the lubricant is the unknown quantity.

When the friction is too great for the dynamometer a pair of compound levers reduces the stress upon the steel bar in the dynamometer to one-fifth that of the whole pull of the frictional component, so that the capacity of the dynamometer is five times the amount marked upon the dial. The resistances at higher pressures are so much less than was anticipated, that it has not been necessary thus far to use these reducing levers.

The coefficient of friction is deduced from the data of observation in the following manner.

Let

P = Weight on discs, lbs.
R = Outer radius of frictional contact, feet.
r = Inner " " "
ρ = Radius of any infinitesimal ring or band of the frictional surface, feet.
N = Number of revolutions per minute.
W = Reading on dynamometer, lbs.
l = Length of arm on upper disc, feet.

φ = Coefficient of friction.

Suppose that the annular surfaces of the disc be divided into an infinite number of elementary areas by equidistant circles and radial lines, then will

Width of band $= d\rho$ . . . . (1)

Angle between two successive radial lines $= d\theta$ . . . . (2)

Length of arc between two radii $= \rho\, d\theta$ . . . . . . (3)

Elementary area $= \rho\, d\rho\, d\theta$ . . . . (4)

Area of annulus $= \pi\,(R^2 - r^2)$ . . . . (5)

Pressure per unit of area $= \dfrac{P}{\pi(R^2 - r^2)}$ . . . . (6)

Pressure on elementary area $= \dfrac{P\rho\, d\rho\, d\theta}{\pi(R^2 - r^2)}$ . . . (7)

Friction on elementary area $= \dfrac{\varphi P\rho\, d\rho\, d\theta}{\pi\,(R^2 - r^2)}$ . . . (8)

Moment of friction on elementary area $= \dfrac{\varphi\, P\rho^2\, d\rho\, d\theta}{\pi\,(R^2 - r^2)}$ . . (9)

Moment of friction on entire disc $= \dfrac{\varphi P}{\pi(R^2 - r^2)} \displaystyle\int_r^R \int_0^{2\pi} \rho^2\, d\rho\, d\theta$ (10)

Integrating $= \dfrac{2\pi\,\varphi\, P}{\pi(R^2 - r^2)} \left\{ \dfrac{\rho^3}{3} \right\}_r^R$ (11)

Substituting the limits $= \dfrac{2\varphi\, P\,(R^3 - r^3)}{3\,(R^2 - r^2)}$ . . (12)

Work of friction per minute $= \dfrac{4\varphi\,\pi\, P\, N\,(R^3 - r^3)}{3\,(R^2 - r^2)}$. (13)

Resistance of the dynamometer $= 2\,\pi\, l\; W N$ . . . (14)

The friction equals the resistance, hence

$$\frac{4\varphi\,\pi\, P\, N\,(R^3 - r^3)}{2\,(R - r^2)} = 2\,\pi\, l\; W N \quad . \ . \ . \ . \quad (15)$$

$$\varphi = \frac{3\, W l\, (R^2 - r^2)}{2\pi\, (R^3 - r^3)} \quad . \; . \; . \; . \; . \; . \; . \; . \quad (16)$$

This is not in a form convenient for continual use, and is susceptible of much simplification, if the proper dimensions are used for the various parts in connection with the frictional surfaces, and the dynamometer arm. It is also important for the sake of simplicity that the length of the line of mean area of the disc be one foot, so that the number of revolutions per minute is equivalent to the frictional velocity in feet per minute. For convenience, it was desirable that the area of the discs be ten square inches.

If c = radius of circle whose circumference is 12 inches, then

$$2\,\pi\, c = 12 \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (17)$$

$$c = \frac{12}{2\,\pi} = 1.909 \text{ inches.} \quad . \; . \; . \quad (18)$$

Area within this circumference,

$$\pi\, c^2 = 11.46 \text{ square inches.} \quad . \; . \; . \quad (19)$$

If this circumference divide the annulus of 10 square inches area into two equal parts, then the outer rim of the annulus will circumscribe an area of 11.46 + 5 = 16.46 square inches. The radii corresponding to these circles are

$$R = \sqrt{\frac{A}{\pi}} = 2.289 \text{ inches} = .1907 \text{ feet.} \quad . \; . \; . \; . \quad (20)$$

$$r = \sqrt{\frac{a}{\pi}} = 1.434 \text{ inches} = .1195 \text{ feet.} \quad . \; . \; . \; . \quad (21)$$

$$R^2 - r^2 = .0221 \text{ feet}\,; \; R^3 - r^3 = .00523 \text{ feet.} \quad . \; . \quad (22)$$

Substituting these values in equation (16)

$$\varphi = \frac{6.338\, W\, l}{P} \quad . \; . \; . \; . \; . \; . \; . \; . \quad (23)$$

This equation can be made still more simple if the length of the arm l, is of such length that

$$\varphi = \frac{2\, W}{P} \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (24)$$

Substituting this value of φ in equation (23), we have

$$l = .3156 \text{ feet} = .3787 \text{ inches.}$$

Generally the weight on the discs is referred to in pounds to the square inch, then

$$\varphi = \frac{W}{5\,p} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (25)$$

If the reducing levers which have been referred to are used, the reading on the dynamometer is $\frac{1}{5}$ of the pull on the arm, and when the machine is used with this attachment

$$\varphi = \frac{W}{p} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (26)$$

The blank used in taking notes of the observations made upon the work with the machine is shown in Table III.

After the temperature of the discs has been reduced by a current of ice-water, the circulation of the water is stopped, the machine started, and the reading of the dynamometer noted at each degree of temperature.

As the machine is generally used without the compound levers, the column of coefficient of friction is obtained by dividing the dynamometer reading by five times the pressure in pounds per square inch.

Table I. contains the record of the dynamometer readings, and shows the resistance of friction of a paraffine oil tested in the machine, at a series of pressures of one to forty pounds per square inch, and temperatures from forty to one hundred degrees Fahrenheit. Readings were noted at each degree, but a tabulation of the friction at every fifth degree answers all required purposes.

The results are clearer expressed by the diagram, which shows in a graphical manner the relations of these measurements to each other within the limits of the data. (Fig. 39).

TABLE I.

RESISTANCE OF FRICTION OF A PARAFFINE OIL AT A VELOCITY OF 300 FEET PER MINUTE.

PRESSURE IN LBS. PER SQ. IN.	TEMPERATURES.												
	40°	45°	50°	55°	60°	65°	70°	75°	80°	85°	90°	95°	100°
	RESISTANCE OF FRICTION.												
1	2.69	2.39	2.13	1.91	1.70	1.51	1.34	1.19	1.06	.95	.85	.75	.69
2	2.99	2.61	2.32	2.08	1.86	1.66	1.50	1.34	1.21	1.09	.99	.89	.80
3	3.16	2.78	2.49	2.23	2.00	1.80	1.62	1.47	1.32	1.20	1.10	.99	.90
4	3.34	2.93	2.62	2.35	2.12	1.92	1.74	1.59	1.45	1.33	1.21	1.10	.99
5	3.50	3.08	2.76	2.49	2.25	2.04	1.85	1.69	1.55	1.43	1.30	1.19	1.09
6	3.65	3.20	2.88	2.61	2.36	2.15	1.96	1.79	1.65	1.51	1.39	1.28	1.17
7	3.79	3.32	3.00	2.71	2.47	2.25	2.06	1.89	1.74	1.61	1.48	1.36	1.26
8	3.91	3.43	3.10	2.82	2.57	2.34	2.16	1.99	1.83	1.69	1.56	1.45	1.34
9	4.05	3.56	3.22	2.93	2.67	2.45	2.25	2.07	1.92	1.78	1.65	1.53	1.42
10	4.18	3.66	3.33	3.03	2.77	2.54	2.34	2.17	2.01	1.86	1.74	1.62	1.51
11	4.30	3.79	3.43	3.14	2.88	2.65	2.45	2.26	2.11	1.96	1.83	1.70	1.59
12	4.41	3.89	3.55	3.25	2.99	2.75	2.54	2.36	2.19	2.04	1.91	1.78	1.66
13	4.52	4.00	3.65	3.35	3.08	2.84	2.63	2.44	2.27	2.13	1.99	1.85	1.73
14	4.64	4.10	3.75	3.44	3.16	2.93	2.72	2.53	2.36	2.22	2.07	1.94	1.81
15	4.75	4.21	3.85	3.55	3.26	3.02	2.80	2.61	2.44	2.29	2.15	2.01	1.88
16	4.85	4.32	3.95	3.61	3.36	3.12	2.90	2.70	2.53	2.36	2.23	2.09	1.95
17	4.95	4.42	4.06	3.75	3.46	3.21	3.00	2.79	2.62	2.46	2.31	2.17	2.04
18	5.08	4.54	4.16	3.84	3.56	3.30	3.08	2.89	2.71	2.54	2.40	2.25	2.12
19	5.18	4.63	4.26	3.93	3.65	3.40	3.18	2.98	2.80	2.64	2.49	2.33	2.21
20	5.28	4.73	4.35	4.63	3.75	3.49	3.27	3.07	2.89	2.73	2.57	2.41	2.27
21	5.36	4.84	4.46	4.13	3.83	3.59	3.36	3.16	2.98	2.82	2.65	2.49	2.35
22	5.46	4.94	4.56	4.23	3.94	3.68	3.45	3.25	3.07	2.90	2.73	2.57	2.42
23	5.55	5.05	4.65	4.32	4.03	3.76	3.54	3.33	3.15	2.97	2.81	2.64	2.49
24	5.65	5.13	4.74	4.41	4.10	3.84	3.62	3.42	3.23	3.05	2.89	2.72	2.56
25	5.75	5.22	4.83	4.50	4.19	3.93	3.70	3.49	3.31	3.13	2.95	2.79	2.63
26	5.83	5.31	4.92	4.58	4.26	4.01	3.78	3.57	3.38	3.21	3.03	2.86	2.71
27	5.93	5.41	5.01	4.67	4.35	4.09	3.86	3.65	3.46	3.29	3.11	2.94	2.78
28	6.03	5.50	5.10	4.75	4.45	4.18	3.95	3.73	3.54	3.36	3.18	3.02	2.85
29	6.10	5.59	5.19	4.84	4.54	4.26	4.02	3.81	3.62	3.44	3.26	3.09	2.92
30	6.19	5.67	5.28	4.92	4.61	4.33	4.10	3.88	3.69	3.51	3.34	3.15	2.99
31	6.26	5.75	5.35	5.01	4.69	4.41	4.16	3.95	3.76	3.58	3.40	3.23	3.05
32	6.35	5.83	5.43	5.09	4.77	4.49	4.24	4.04	3.84	3.65	3.46	3.29	3.12
33	6.43	5.91	5.52	5.16	4.85	4.57	4.33	4.11	3.91	3.72	3.54	3.35	3.18
34	6.50	6.00	5.60	5.25	4.93	4.65	4.41	4.19	3.99	3.80	3.61	3.43	3.24
35	6.58	6.08	5.69	5.32	5.01	4.73	4.48	4.26	4.05	3.86	3.68	3.50	3.32
36	6.65	6.15	5.75	5.40	5.09	4.81	4.57	4.34	4.13	3.93	3.75	3.56	3.38
37	6.73	6.22	5.83	5.49	5.17	4.88	4.64	4.41	4.21	4.01	3.82	3.63	3.45
38	6.80	6.31	5.92	5.57	5.25	4.97	4.72	4.49	4.29	4.09	3.89	3.70	3.52
39	6.88	6.39	6.00	5.65	5.34	5.04	4.79	4.56	4.35	4.15	3.95	3.76	3.57
40	6.97	6.46	6.06	5.73	5.41	5.12	4.86	4.63	4.42	4.21	4.02	3.82	3.62

As the temperature rises, the increasing fluidity of the oil diminishes the friction within the limits of free lubrication.

It is also seen that the resistance does not increase proportion-

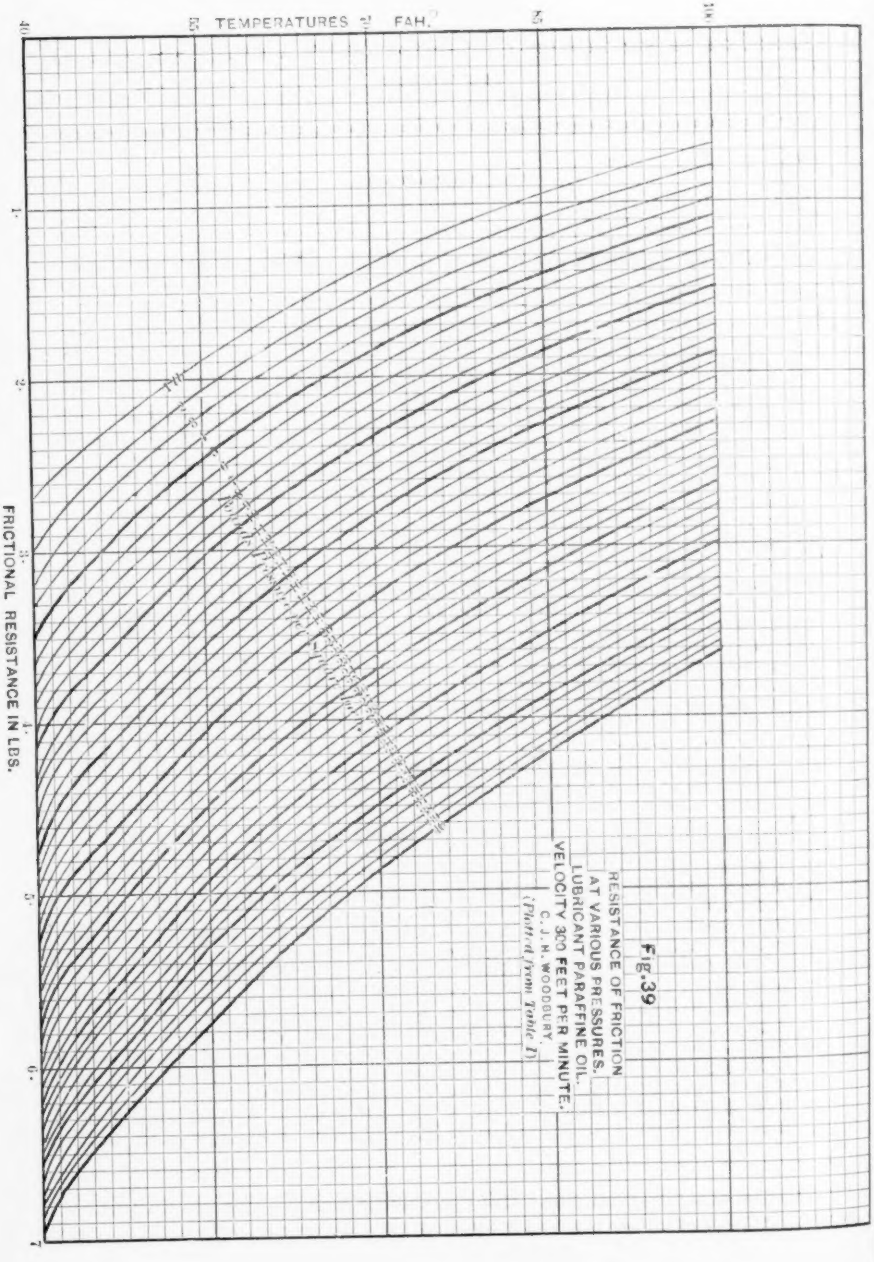

Fig. 39
RESISTANCE OF FRICTION
AT VARIOUS PRESSURES.
LUBRICANT PARAFFINE OIL.
VELOCITY 300 FEET PER MINUTE.
C. J. H. WOODBURY.
(*Plotted from Table I*)

ately with the pressure, nor at a uniform rate. The lubricant, while separating the surfaces of a journal, and protecting them from injury, also introduces the resistance of its own cohesion; and at small pressures the film of oil is thicker and the resistance due to viscosity of the oil exceeds that at high pressures, when a smaller amount of oil lies between the surfaces.

A film of the lubricant adheres to each of the frictional surfaces, and that portion which lies between these two films is pulled in one direction upon one side, and in the other direction upon the other side, and as a resultant, the movement of this centre layer is a rolling motion, whose rate of progression varies with the difference between the adhesion of oil between the two frictional surfaces.

Nearly five years ago I stated,* as a result of some early work on this subject, that "friction exists at the surface of the two disks between the film of oil acting as a washer and the particles of oil partially imbedded within the pores of the metal," and the result of all subsequent investigation has tended to confirm this view of the subject.

Table II. shows the co-efficient of friction as computed from the first table of resistances by the formula previously given.

$$\varphi = \frac{W}{5\,p}$$

φ = coefficient of friction.

W = resistance of friction as shown by dynamometer in lbs.

p = pressure upon frictional surfaces in pounds per square inch.

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* Transactions New England Cotton Manufacturers' Association, Fifteenth Annual Meeting, p. 61.

TABLE II.

CO-EFFICIENT OF FRICTION OF A PARAFFINE OIL AT A VELOCITY OF 300 FEET PER MINUTE.

PRESSURE IN LBS. PER SQ. IN.	TEMPERATURES.												
	40°	45°	50°	55°	60°	65°	70°	75°	80°	85°	90°	95°	100°
	COEFFICIENT OF FRICTION.												
1	.5380	.4760	.4260	.3820	.3400	.3020	.2680	.2380	.2120	.1900	.1700	.1500	.1380
2	.2990	.2610	.2320	.2080	.1860	.1660	.1500	.1340	.1210	.1090	.0990	.0890	.0800
3	.2167	.1853	.1660	.1487	.1333	.1200	.1080	.0980	.0880	.0800	.0733	.0675	.0600
4	.1670	.1465	.1310	.1175	.1060	.0960	.0870	.0795	.0725	.0665	.0605	.0550	.0495
5	.1400	.1232	.1104	.0966	.0900	.0816	.0740	.0676	.0620	.0592	.0520	.0476	.0436
6	.1217	.1067	.0960	.0870	.0787	.0717	.0653	.0597	.0550	.0503	.0463	.0427	.0390
7	.1089	.0949	.0847	.0774	.0706	.0643	.0588	.0540	.0497	.0460	.0423	.0388	.0360
8	.0978	.0858	.0775	.0705	.0642	.0585	.0540	.0498	.0458	.0423	.0390	.0359	.0335
9	.0900	.0791	.0715	.0651	.0593	.0544	.0500	.0460	.0427	.0395	.0367	.0340	.0316
10	.0836	.0732	.0666	.0606	.0554	.0508	.0468	.0434	.0402	.0372	.0348	.0324	.0302
11	.0782	.0687	.0624	.0571	.0524	.0482	.0445	.0411	.0384	.0356	.0330	.0311	.0289
12	.0735	.0648	.0592	.0542	.0498	.0458	.0423	.0390	.0365	.0340	.0315	.0297	.0277
13	.0695	.0615	.0561	.0515	.0474	.0437	.0405	.0375	.0349	.0328	.0306	.0285	.0266
14	.0663	.0586	.0533	.0491	.0451	.0419	.0389	.0361	.0337	.0317	.0296	.0263	.0259
15	.0633	.0561	.0513	.0475	.0435	.0403	.0375	.0349	.0325	.0305	.0280	.0268	.0255
16	.0608	.0540	.0494	.0455	.0420	.0390	.0363	.0338	.0316	.0295	.0278	.0261	.0240
17	.0582	.0520	.0477	.0441	.0407	.0378	.0353	.0328	.0308	.0289	.0272	.0255	.0244
18	.0564	.0504	.0462	.0426	.0396	.0364	.0342	.0321	.0301	.0282	.0264	.0250	.0237
19	.0545	.0487	.0448	.0414	.0384	0358	.0335	.0314	0295	.0278	.0262	.0245	.0233
20	.0528	.0473	.0435	.0403	.0375	.0349	.0327	.0307	.0289	.0273	.0257	.0241	.0227
21	.0510	.0460	.0424	.0394	.0364	0342	.0320	.0302	.0284	.0268	.0252	.0238	.0224
22	.0496	.0450	.0414	.0384	.0358	.0334	.0314	.0296	.0280	.0264	.0248	.0234	.0220
23	.0483	.0439	.0404	.0374	.0350	.0227	.0308	.0290	.0274	.0258	.0244	.0230	.0216
24	.0471	.0436	.0396	.0368	.0342	.0320	.0302	.0285	.0269	.0254	.0241	.0226	.0213
25	.0460	.0418	.0386	.0360	.0336	.0314	.0296	.0279	.0265	.0250	.0236	.0223	.0210
26	.0448	.0408	.0378	.0352	.0328	.0308	.0290	.0274	.0260	.0246	.0233	.0221	.0208
27	.0439	.0400	.0370	.0346	.0322	.0302	.0286	.0270	.0256	.0243	.0230	.0218	.0206
28	.0430	.0392	.0364	.0340	.0318	.0298	.0282	.0266	.0252	.0240	.0228	.0216	.0204
29	.0421	.0386	.0358	.0334	.0313	.0294	.0277	.0263	.0250	.0237	.0225	.0213	.0201
30	.0413	.0378	.0352	.0328	.0307	.0289	.0273	.0259	.0246	.0234	.0222	.0210	.0199
31	.0404	.0371	.0347	.0323	.0304	.0284	.0268	.0255	.0243	.0231	.0219	.0208	.0197
32	.0397	.0364	.0339	.0318	.0298	.0281	.0265	.0252	.0240	.0228	.0216	.0205	.0195
33	.0390	.0358	.0335	.0313	.0294	.0277	.0262	.0249	.0237	.0226	.0214	.0203	.0193
34	.0382	.0353	.0330	.0309	.0290	.0274	.0260	.0246	.0235	.0224	.0213	.0202	.0191
35	.0376	.0347	.0325	.0304	.0286	.0270	.0256	.0243	.0231	.0220	.0210	.0200	.0190
36	.0370	.0342	.0320	.0300	.0283	.0267	.0254	.0244	.0230	.0219	.0208	.0198	.0188
37	.0364	.0336	.0315	.0297	.0279	.0264	.0251	.0239	.0228	.0217	.0206	.0196	.0186
38	.0358	.0332	0312	.0293	.0276	.0262	.0248	.0235	.0226	.0215	.0205	.0195	.0185
39	.0353	.0328	.0308	.0290	.0274	.0258	.0246	.0234	.0223	.0213	.0203	.0193	.0183
40	.0349	.0323	.0303	.0289	.0271	.0256	.0243	.0232	.0221	.0211	.0201	.0191	.0181

It does not seem feasible to deduce a formula which will meet the limitations of this table; and if such an equation were given, it could serve no practical use either in practice or theory, because it would be bound to these specific results, and unsuited for appli-

cation elsewhere; but from these results one can observe certain generalities capable of wide application.

It will be observed that, in a general way, the coefficient of friction diminishes inversely with the pressure, and directly with the fluidity of the oil, as indicated by the temperature; and that the rate of these differences diminishes with the increase of pressure. The reason for this is that the resistance due to the viscosity of oil is greater at low than at high temperature, and that with heavier pressures the film of oil is actually thinner, besides being relatively smaller in proportion to the pressure.

On this account, the frictional difference between lubricants is much less at high than at low pressure during continuous lubrication, although the differences in regard to endurance are more widely marked at high pressure.

It is almost universally asserted to be a general principle that the coefficient of friction is independent of the pressure, regardless alike of the actual facts in the matter, and of the limitations of Morin's Experiments,* which form the common source of authority on the subject.

The coefficient of friction between any two solids is accepted to be a constant ratio; but when a lubricating medium is interposed, then the frictional relation between these three substances becomes variable, according to the effect of temperature, pressure, and velocity upon the lubricant, and the problem bears certain analogies to those of hydrodynamics relative to the efflux of a fluid through a narrow orifice. When the pressures are great, these variables form such a small ratio to the whole frictional resistance that they escape observation unless the measurements of friction are taken in an accurate manner.

If the lubricant is not used, the variables disappear altogether, and then the coefficient of friction becomes reduced to a constant ratio. This latter class of friction is rarely considered, except for the friction of repose, in matters pertaining to the stability of structures; while the problems of mediate friction enter into the operation of the moving parts of every machine.

This is not the place to enter into a criticism of the work of Morin, but it should be observed that his investigations were de-

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* Nouvelles Expériences sur le Frottement, Faites à Metz en 1831. Par Arthur Morin, Capitaine d'Artillerie. 128 pp., 4°. Plates.

Second Mémoire. 1832. 103 pp., 4°. Plates.

Troisième Mémoire. 1833. 142 pp., 4°. Plates.

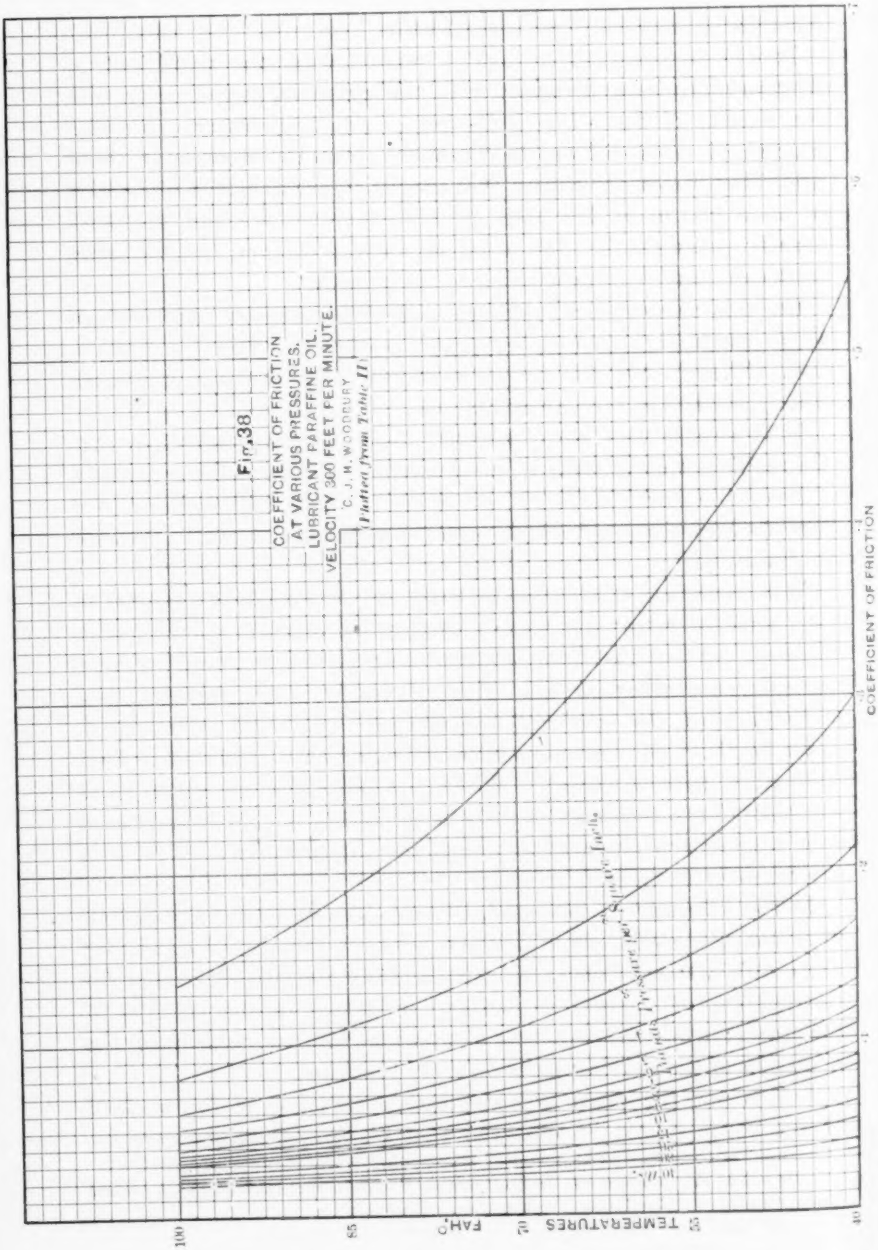
Fig. 38
COEFFICIENT OF FRICTION
AT VARIOUS PRESSURES.
LUBRICANT PARAFFINE OIL.
VELOCITY 300 FEET PER MINUTE.
C. J. H. WOODBURY
(Plotted from Table II)
Pounds Pressure per Square Inch.
COEFFICIENT OF FRICTION
100
85
FAH.°
70
TEMPERATURES
55
40

voted to measurements of a sled upon tracks in the interests of the ordnance corps; and although he made some experiments upon friction of oiled bearings, they were not subjected to the continuous friction of lubricated journals under conditions analogous to those in machines.

In a letter * written March 15, 1879, Gen. A. Morin said [Translation], "The results furnished by my experiments as to the relations between pressure, surface, and speed, on the one hand, and sliding friction on the other, have always been regarded by myself, not as mathematical laws, but as close approximations to the truth within the limits of the data of the experiments themselves."

Considerations of safety have fixed the minimum limit of the flashing point of a lubricating oil at 300° Fahrenheit, with a proportion of volatile matter not exceeding five per cent. thrown off by exposure to 140° Fahrenheit for twelve consecutive hours. With the saving clause of proper limits of pressure, a fluid oil offers less frictional resistance than a viscous one.

Although the data in Table II. show that the coefficient of friction diminishes with the increase of fluidity, it does not warrant any extreme position in respect to the use of thin oils, except for light pressures, because, under all circumstances, the film of oil must be thick enough to keep the surfaces of a journal from actual metallic contact. In the severe work of heavy pressure a viscous oil must be used in order to retain its place upon the bearing surfaces in sufficient thickness to prevent the inequalities upon the journal from colliding. In some places, it has been found that the use of an extremely thin oil resulted in a diminution of the friction of the machines at the expense of more rapid wear of the journals. Such results are not apt to occur upon journals of light pressure, such as spindles, where a thin oil is used with good judgment.

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* Transactions Institution Mechanical Engineers of Gt. Britain, 1883, p. 666.

TABLE III.

BOSTON MANUFACTURERS' MUTUAL FIRE INSURANCE CO.

March 12, 1883.

Frictional Tests of No. 58 Unknown Mineral Oil.

Temp. Disc. Ending.........95 " " Beginning......35	Time { Ending ....10.31 Time { Beginning........10.10	Counter { Ending ......910,382 Counter { Beginning ...904,162
Temp. Room { Ending......60 Temp. Room { Beginning...62	Duration of Experiment.....21	Total Revolutions.........6,220 Rev. per Minute } ....296 Feet " " }

Pressure on frictional surfaces, 33 pounds per square inch.

Temp.	Dynamometer.	Coeff. of Friction.	Temp.	Dynamometer.	Coeff. of Friction.	Temp.	Dynamometer.	Coeff. of Friction.
35	8.00	.0485	64	4.80		93	5.45	
36	7.00		65	4.75	.0288	94	5.80	
37	6.65		66	4.70		95	6.10	.0370
38	6.45		67	4.66				
39	6.30		68	4.60				
40	6.12	.0371	69	4.57				
41	6.02		70	4.54	.0275			
42	5.90		71	4.50				
43	5.82		72	4.45				
44	5.75		73	4.42				
45	5.68	.0344	74	4.39				
46	5.61		75	4.37	.0265			
47	5.55		76	4.36				
48	5.49		77	4.35				
49	5.42		78	4.32				
50	5.36	.0325	79	4.30				
51	5.32		80	4.29	.0260			
52	5.28		81	4.27				
53	5.22		82	4.25				
54	5.17		83	4.25				
55	5.12	.0310	84	4.28				
56	5.09		85	4.31	.0261			
57	5.06		86	4.41				
58	5.03		87	4.51				
59	4.99		88	4.62				
60	4.95	.0300	89	4.75				
61	4.91		90	4.90	.0297			
62	4.88		91	5.05				
63	4.84		92	5.22				

Table III. shows the record of a test of a very limpid mineral oil which reached, in the frictional machine, its limit of lubrication at 82° Fahrenheit under a pressure of 33 pounds to the square inch; beyond that point, the oil became so fluid that the pressure reduced the thickness of the sheet of oil, until portions of the surfaces met in actual contact.

At lower temperatures, the greater resistance shows a large coefficient of friction on account of the viscosity of the oil, while the

rise in friction at higher temperatures, as shown in the upper part of the curve, indicates a resistance produced by the collision of portions of the discs, and the diagram illustrates a graphical representation of the beginning of a hot bearing.

These results have been submitted in the hope of presenting facts which will add somewhat to the means for a fuller treatment

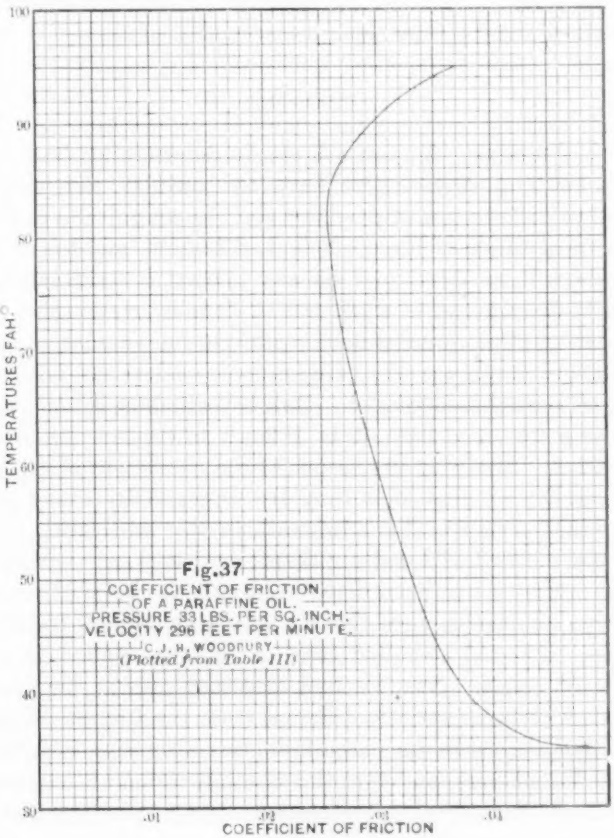


Fig.37
COEFFICIENT OF FRICTION
OF A PARAFFINE OIL.
PRESSURE 33 LBS. PER SQ. INCH.
VELOCITY 296 FEET PER MINUTE.
C. J. H. WOODBURY
(Plotted from Table III)

of the problem of lubrication. The several economies pertaining to lubrication operate at the expense of each other. An economy of oil may represent an extravagance in motive power; a liberal allowance of limpid oil may save motive power at the expense of the repair account if it fails to keep the surfaces free from contact with each other, and above all the final result must show the greatest amount of lubrication for a dollar. Lubricants are

wasted, not worn out by attrition, and it is of more importance to know how to use oil, than what oil to use. Safety having been first assured, the problem of lubrication seeks to know what combination of oil, coal, and repairs will represent the fewest dollars; and in its broad sense, it cannot be solved on any experimental basis, nor settled by a final dictum from any one source, but it will reach its solution through the practical experience of intelligent observation, aided by the resources of technical science.

DISCUSSION.

Prof. Thurston.—As you will see, in determining how we shall handle rubbing surfaces, we have to consider, not simply the method of distribution of pressure and the magnitude of journals, but the magnitude of the coefficient of friction, and the amount of heat that will be developed on any journal is evidently proportioned to the amount of work that is done in overcoming friction. The heating of the journal is increased as the amount of work increases, and the power of cooling is increased as the area and the length of journal is increased. But, nevertheless, if our coefficient of friction becomes high, it is difficult to control the heating of the journal. Studying the values of the coefficient of friction experimentally, we find that they are changed by every physical change that occurs in the lubricants, in the lubricating surfaces, or in the method of handling them; and it is only within a very few years that it has been discovered that the laws of friction, as formerly enunciated, were entirely wrong as applied to lubricated surfaces. The old so-called laws of friction applied to the dry friction of solids. Now we find that, not only is the friction of lubricated surfaces entirely different from the friction of unlubricated surfaces, as to its magnitude, but also that it follows entirely different laws, and that the absolute value of the coefficient is modified by every physical change that is produced, either in the substances used for lubricants, or the substances used for bearings on rubbing surfaces. What has been most needed of late years, by the engineer, has been a determination of the values of this coefficient of friction for the conditions under which we all work. Now the range of conditions is enormously great. In cotton-mill and woolen-mill practice, where we are handling spindles, the pressures come down to a few pounds to the square inch. In heavy machines, they go up to hundreds of pounds to

the square inch. On the crank-pins of our heavy engines, they run up to as high as 1,200 pounds, and under the pivots of draw-bridges they run up to as high as 9,000 pounds, and, under all these conditions, engineers find ways of making their work successful. But, nevertheless, the determination of methods of proportioning has been a tentative process entirely. We do not yet know precisely what are the laws governing friction under these various conditions. But this we have found out: that the proportional resistance of friction is reduced in a very rapidly increasing ratio by increase of pressure. We have also found that, with any given lubricant running under good conditions, the resistance decreases as that lubricant becomes warmed up, and the law again changes. Thus we find that every change of pressure, diameter, and speed, produces a change in the value of the coefficient of friction. So many conditions come in to determine what shall be its exact value, that no one yet has been able with any satisfactory degree of accuracy to express these laws mathematically, or to express in their analysis all of the many conditions that determine what shall be the magnitude of the coefficient of friction, or what is the magnitude of the journals and rubbing surfaces that we should adopt. The way in which this investigation is best made is well illustrated in Mr. Woodbury's paper, and in the tables in that paper are found the coefficients. Making experiments with the oil reported on, and with sperm oil and various other oils, we may finally establish a set of values which can be used in designing: and this is the first step in so doing. But Mr. Woodbury has been working with very low pressures. In the record which he presents to-night, he goes to 40 pounds to the square inch, which in mill machinery is high, but in heavy machinery is very low. Fortunately, to complement this work of Mr. Woodbury, we have experiments made for the British Institute of Mechanical Engineers some time ago by Mr. Tower, where the pressures were carried up to the higher figures which the engineer reaches in actual work. Taking the two together, we have to-day a very valuable set of tables for the engineer's purposes; and by reference to this work we can learn what are to be expected to be the coefficients of friction for work of whatever kind we may be engaged in, and for pretty nearly every range of conditions ordinarily met with. The range of pressure met with by engineers is something very great, and the behavior of the different metals is very different under any given set of conditions, but,

fortunately, we find that these heavy mineral oils, which are to-day almost entirely superseding the animal and vegetable oils, have qualities which fit them to take any desired place in the series. That is to say, under very light loads, we can put in the lighter oils. Under the heavier loads, we can put in the heavy mineral oils.

There is still another thing that is found to determine largely the value of the coefficient of friction in many cases, and is still more effective in producing changes, and that is the method in which the lubrication is carried on. I suppose that no one in the profession five years ago imagined to what extent the method of lubrication affects the value of these coefficients of friction. But it is found now, especially at high speeds, that we can adopt the oil-bath system of lubrication, *i. e.*, running the journal in a flood of oil, and the coefficient of friction can thus be brought down, in many cases, to small fractions of what are common values with the ordinary systems of lubrication. I received a letter lately from a gentleman, asking me why my figures differed from what he obtained from some oils. The difference was one of 100 per cent., and was simply due to the fact that he had the oil-bath system of lubrication, while I had the ordinary system of lubrication by the oil-cup and wick. He fed in his oil very freely and on the sides. He got for a coefficient of friction of a light oil two-thirds of one per cent., while, under the same pressures, I obtained, by the other system, coefficients of about one per cent. But that is not at all extraordinary, in comparison with the differences we met with in other cases. For example, Tower obtained coefficients of one-tenth of one per cent. In my experiments the coefficient would have stood about one per cent., that is to say, a difference of ten to one is made by the methods of applying the oil.

I will not stop to say anything about the machine that Mr. Woodbury has devised. It strikes me as exceedingly well adapted to do the work. I want to suggest to members the advisability of obtaining the paper of Mr. Woodbury, and also a copy of the paper of Mr. Tower. Taking the two together, we have a set of values of the coefficient of friction which will be a very useful guide; but it will be necessary to remember that Tower's results are obtained by the method of flooding the journal by the oil-bath system.

The rate of motion of the two surfaces, one upon the other, is a

condition that affects the value of the coefficient of friction. For example, with oil-bath lubrication, I suppose that the journal acts like a pump, forcing in the oil between itself and the bearing, and so holding itself upon a fluid cushion, and in that way bringing down the coefficient of friction to the marvelously low figure that Mr. Tower obtained. It is a very curious, and very interesting, and very instructive thing, and I will take time to indicate what occurs. Suppose that the upper surface of a journal, which is loosely fitted, be represented by a sketch on the blackboard, and that the load is thrown on the bearing as shown. If this journal is driven at very high speed in a bath of oil, it acts like a pump, and carries up the oil, forcing it up into this space between journal and bearing, and it presently cannot go further, and a returning stream is produced, and an eddy. Now the effort to drive that oil in there at high speeds of rotation is so great that the pressure on the crown of the journal becomes very high. That fact was discovered by Mr. Tower in this curious way. There was an oil-cup, originally, on the top, and the oil-hole had been plugged for the purpose of using the journal in this way: and after a while it was observed that the plug came out. It was then driven in pretty hard, and it was forced out again. After some experience of that kind, Mr. Tower put on the oil-hole a steam-pressure gauge, and the gauge-hand went up to 200 pounds to the square inch. But it is perfectly evident that this forcing action drives in this volume of oil, and holds it in there, with a pressure that is greater as the velocity of turning becomes greater; so that, with oil bath lubrication, where the maximum pressure did not exceed 200 pounds to the square inch, it was forced directly over the crown of the journal, and the journal was simply running on a cushion of fluid, and that brought down the coefficient of friction to this marvelously low figure. That is a condition very frequently met with in engineering pracice, and, while using the oil-hole and oil-cup, we often cut a groove and run it down laterally. But we are then losing a certain part of the efficiency of this pumping system, and the correct method, for high speeds and heavy loads, is to adopt lubrication at one side and make a flooded journal. If we make this action effective, and the cap of the journal comes down within a very minute distance of the journal, but does not touch, we have a journal running on a fluid cushion.

The laws of friction here illustrated are the laws of fluid fric-

tion. The laws of solid and fluid friction are entirely different. With solid friction, the resistance is, within certain limits, entirely independent of the rate of rotation. With fluid friction, it increases with the square of the velocity of rubbing. It was found by Mr. Tower that, under certain conditions, he had such a rate of change in his values of the coefficient of friction that it was easily to be seen that they might, after a while, at still higher speeds, assume a very objectionable magnitude. But, in ordinary journals, we have a friction partly made up of friction of rubbing of the parts in contact, and partly made up of fluid friction. In these experiments, before attaining 200 pounds pressure, he did not reach the conditions of fluid friction, the resistance of friction varied, not as the square of the speed, but as the square root of the speed; so that, in the very best case of lubrication, the method of lubrication being however perfect, the friction is neither the friction of solids nor the friction of fluids, but is intermediate. In all these cases, and under these various conditions affecting the method of application, it is impossible to say what is the law. There are many laws involved; but, as Mr. Woodbury says in his paper, no algebraic expression is yet obtained, or is likely to be obtained, that will be of any great value to the engineer. [Applause.]

Mr. Towne.—In anticipation of the debate we will undoubtedly have on this very interesting subject, I would like to supplement very briefly what Professor Thurston has said in regard to the experiments of Mr. Tower. In the first place, they are to be found in the report of the proceedings of the Institution of Mechanical Engineers of Great Britain of January or February of this year.

Prof. Thurston.—Also in the London *Engineering* of about that time, and I do not know but that they may be published in this country.

Mr. Towne.—It is also a matter of practical interest at least to mention other deductions that Mr. Tower makes from his work. As Prof. Thurston has said, the best possible condition of lubrication for a journal is that of the oil-bath. But of course that is often not available, and Mr. Tower addressed himself to determining the best conditions where that is not available. The next best form, if I remember rightly, is what he called pad lubrication, the journal running in a half box and being lubricated by a pad of wool, or some similar substance, containing the oil and

constantly in contact with and rubbing the journal, and depositing the lubricant in that way. Next to that he found, where the lubrication must be by oil-cup from above, that where the pressures became at all high, lubrication at the apex or top of the journal was at precisely the worst possible point. It was exactly the wrong place for the introduction of the lubricant. It was a very curious and striking fact which he discovered, that with the ordinary groove in the top of the box, and receiving its supply of oil through a vertical tube above, the oil, instead of coming down through that tube and being distributed through the groove, the groove acted as a scraper and took the oil off the journal and forced it up into the tube. And finally, for journals of that class, he determined that points about 120 degrees apart properly supplied or fed with oil from above were the most efficient for introducing oil where it had to be supplied in that way.

Mr. Schuhmann.—A few months ago the same subject came up for discussion before the mechanical engineers of Germany. One of the gentlemen stated that he had the same trouble which Prof. Thurston just spoke about, because of the journal-box closing in on the sides, scraping the oil off and causing the journal to heat. It was used on an old style rolling-mill engine, and, owing to the action of the crank, one would expect the journal to wear at *b* (Figure 85), but it continued to press hard at *b* and began to open at *a*, so that liners could be inserted. He then put bolts through the body of the pedestal, to hold the sides back,

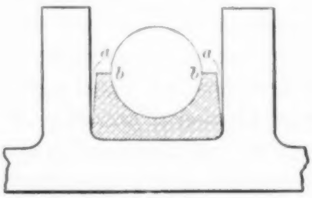


Fig. 85.

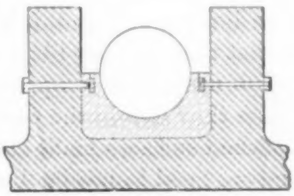


Fig. 86.

and prevent them closing in on the shaft (Figure 86). Subsequent experiments proved that the friction was reduced considerably, and the journal did not heat again. If those of the gentlemen who understand German will read *Stahl und Eisen* of July last they will find the discussion about this very interesting.

Mr. Holloway.—In building large vertical engines we have found that same difficulty, the tendency of the brass to turn in sideways

and bind—and I have used the means referred to above, putting bolts at the side for pulling out the brasses, for several years. I am glad to know it is considered a good plan. It met with good success with us.

Mr. Kent.—In some rolling-mill machinery it is customary to have four brasses, two above and two at the sides, with a large space between adjacent brasses at the corners. The pressure in that case is partly vertical and partly horizontal. Each side brass has its own separate adjustment. Binding is thus prevented, and ease of lubrication insured.

Mr. C. E. Emery.—I think the whole Society should congratulate itself that we have those among us who are competent to undertake this class of work, and to make such investigations as are a credit to themselves, to the Society, and to the country, as well of benefit to the world at large.

*Dr. F. W. Arvine** (by invitation).—I must thank you for your courtesy in giving me an opportunity to speak to you. But I really do not feel competent to add anything to the question which I know by practical experience involves so many intricate problems. It occurs to me, however, that in listening to these gentlemen, and having read something of their writings before, that what they tell us is of exceedingly great interest to a designing engineer, and of great interest scientifically considered. But to a practical man who wants to know what he needs to put upon his engine, the value of the "coefficient of friction" is extremely low. We know perfectly well that it depends upon so many conditions that are difficult to maintain and to understand. Even the wisest admit that there is very little yet found out which throws a practical light upon that subject. For instance, we know perfectly well when they decide the coefficient of friction of paraffine oil, on two polished surfaces made to fit perfectly, we gain no light at all on what we need to use on bearings that do not fit equally well, or have the same finish. If we are to decide how to construct a bearing, what its proportions would be, how large a segment it should cover, all of these researches would be directly in point. But if we have an engine that runs hot, or a certain construction of railway bearings that makes it difficult to find a suitable lubricant, these investigations afford us no light whatever, and I think that any engineer will agree with me that crediting all possible value to researches regarding the coefficient of friction, what we

*Chemist of the Standard Oil Co.

want most to know is vastly beyond that. For instance, an engineer comes to you and says, "I was using such a paraffine and my engine ran cold, I am now using such a paraffine and it runs hot." We find on examination that there is no practical difference between the two, but when we come to apply them to a bearing we find that for some strange reason the two oils did not work alike. When we apply them to spindles we find that two paraffine oils often bear no relation to each other in their practical behavior. We find that one will run at a much higher temperature for some obscure reason. We might think it due to its viscosity, but we find by our instruments that it does not depend on that. In some instances, too, the results which we obtain are exceedingly contradictory. The Rochester and Pittsburgh Railroad Company run their cars with a bearing, the surface of which is composed apparently of lead. There is a very thin film of lead --it appears to be lead—spread over the inner surface of the composition box, which looks as though it might be gun metal. They say they will run an oil on this soft bearing that I know positively fails under similar circumstances, and on the same class of cars on the Erie, New York Central and Delaware and Lackawanna—just why, I cannot say for the life of me. I know that that shaft will sink down on that bearing, and yet it runs cold and runs on a very cheap oil. One of the Standard Oil Companies manufactures an oil which is composed largely of a mineral base which is united with a fatty acid. All things considered, that oil has been the most successful for running cold of any oil used on railways. And yet that fails to equal some other cheaper oils on the friction testing machines. We have had occasion lately to make some comparative tests on fast running spindles of the Sawyer pattern. We have half a dozen oils from Europe and several made in this country: Taking a spindle frame, which I have by the kindness of Mr. Woodbury, and adapting a dynamometer of my own, I expected to find that oils that had the best body would be the best oils for those spindles. I found I did not know anything about it. After having read many papers and listened to a great deal of talk from gentlemen who know a great deal about it, I found that the spindles did not bear out anything that I expected. Now a spindle that was nicely fitted behaved in one way; but a spindle that was badly fitted behaved very differently. The effect of this was to establish a friction of its own in addition to the friction which we generally speak of as due to the oil itself. If we

put on oil that has good body the heat increased very rapidly. Now we should say at first that that was an objection to that oil. But we found after running them for a short time that the oils that gave us the best results in the economy of power were the ones that produced the most rapid wear. We found that after running constantly at some 6,500 revolutions a minute that it had lost as much as $\frac{1}{10}$ of one gramme in actual weight. When we took another paraffine oil the wear was not one-half of that, and there seems to be no apparent reason for it. One reason why these spindles of the Sawyer pattern run so well with these oils is that they run on a "flooded bearing." I have come to think lately that the question of wear on bearings as ordinarily found is one of vastly more importance than the cost of oil. One large mill owner came to me recently and said that he wanted an oil for his spindles. He said he did not care what it cost. He said, "after running some eleven mills for many years I have discovered that we sustain such great loss by wear due to poor lubrication, as to make the mere price of the oil unimportant."

Referring to the diagram and what was said about the compression of journals, I noticed once a peculiar fact. When the engines of the Brooklyn Bridge were started the faulty construction of the bearings and journals caused rapid heating. I think it was generally believed by the engineers who examined them that the construction was faulty. The main bearings were like those shown in Figs. 85 and 86, and the pinching on the sides appears to have been such as described. While watching those bearings quite intently one day I happened to put my finger on the shaft (which I think measures eleven inches in diameter) at a place where it showed extra pressure on the box, and I found it quite hot, while just an inch and a half from that section the shaft felt cold, showing how great and intense mu t be the heat generated when the conductivity of such a mass of iron failed to remove it. It seems to me that the conditions and behavior of those journals corresponded with what Prof. Thurston has just described, and the binding in all such cases being simultaneous with an increase of temperature, I am led to wonder if it be not due to expansion by heat rather than to yield in the journal.

We generally find that where we can use oils of higher viscosity ordinary machinery bearings run colder. But in some instances of high speed and heavy pressure only thin oils answer at all, and the bearing must be flooded. I believe the engines of the Edison

Electric Light Company, which run the dynamos in Pearl Street, are rated 75-horse power and run at the rate of 375 revolutions per minute. I am credibly informed that the oil they are using on these engines is the only sort yet found to run cold. This is an oil of about 32 degrees Beaumé gravity, which is almost as light and fluid as common kerosene and requires to be supplied constantly. I have come to think that this arranging of a steady current of thin oil is a very desirable plan wherever it can be practically carried out, as in case of the Sawyer & Rabbeth spindles, and nicely finished and fitted bearings of larger dimensions like those in the machines these gentlemen have devised for testing oils. With bearings nicely finished and journals carefully scraped and ground to fit, an abundant and constant supply of thin oil will give the best possible result in keeping the bearing cold and saving power. With such machines you may learn much of the proper proportions of bearings, and of the conditions of friction in relation to pressure and speed, and of oils, too, under the exact conditions of these machines, but nothing of the requirements of machinery in common use. Why, these gentlemen think of a journal and bearing as two cylindrical surfaces in perfect contact. It would be more scientific and practical to regard the fit of ordinary bearings like that of a pea in a bushel basket. In many instances one point of contact would be almost too much to expect, and the "body" or viscosity of the oil must make the fit. I know by my own observation that the results obtained in testing heavy lubricating oils on these machines do not correspond with the practical working of these oils on railway axles and other machinery, and any engineer to whom you might supply oils selected from such data, would soon come back to you, as the saying is, "with tears in his fist and his eyes doubled up." We have been there—both sides.

The fact is, the most important conditions under which these tests are made, are the very ones that can only be maintained under special and unusual circumstances, and are never found in the great mass of machinery that must be lubricated. This makes the frictional tests by special machines worthless as a means of selecting lubricants, and a delusion to those whose opinions of oils are thus founded.

I believe that the great value of these friction tests, and the machines by which they are made, lies in another direction: for we know that scientific research lies at the root of all industrial progress. But I think we must try an engine oil on an engine, a

spindle oil on a spindle, and a wagon grease on a wagon, all as nearly as possible under the ordinary conditions, to learn anything of the real value of the lubricant.

While speaking of spindle oils, I should have added that I found their bearing parts did not wear bright. I considered their frosted look as indicative of a friction I saw no way of estimating, although it was attended with rapid wear. With oils of higher viscosity and heavier gravity the bearing surfaces polish like a mirror, even though the friction, judging by the power consumed, is much greater with the more viscous oils. I infer that we cannot judge of the actual friction occasioned by the moving surfaces in contact, while we are unable to estimate separately that friction attributed to the oil itself. For using a thin oil we decrease that one friction so greatly that we cannot note any lesser increase in the other which occasions wear. If it be true that the question of wear is frequently of paramount importance in choosing special oils, then a low coefficient of friction is no measure of value, and our frictional tests are worthless as a means of judging.

It is frequently said that we have petroleum oils which alone answer all requirements of lubrication. It is not so. I wish it was. I believe practical experience fully proves that in many instances compounded oils are best. Sometimes a thin petroleum oil with a small portion of animal oil gives better results. You may find instances where the best petroleum oils do not answer on an engine, when lard oil or a mixture principally lard oil keeps the engine cold and is every way satisfactory. This is sometimes the case whether the petroleum be of heavier or lighter gravity than the lard oil. But I am glad to say these are rare instances, where petroleum oils do not answer every requirement.

I do not feel that I have much information as yet that will seem to you as profound or new. I can, at most, only hope in throwing out these rough suggestions, to give a clue to facts which may enable others here to learn much more of that we need to know than I shall be able to discover.

Mr. Woodbury.—In the course of work with transmission dynamometers I have had cause to note the variations in the frictional resistance of the dynamometer caused by differences in the rate of lubrication, and the method of freely oiling the bearings was adopted in order to render the friction constant as far as practicable. In some instances, the oil has been heated over a water bath, in order to render its fluidity constant, but that seemed to be a futile attempt at refinement. When I designed my friction

machine, this experience with the dynamometer caused me to consider the method of obtaining a uniformity in the method of lubrication, by preserving a constant head of oil in the glass feeding tube.

The friction of a lubricated bearing follows some law of fluid friction unless there is contact of the journal and its bearing; in which instance certain features of solid friction are introduced. In time, a journal will wear to a fit. All journals in good order float upon a film of oil; and that matter was brought to my attention by the ease with which one could push a certain line of heavy shafting weighing with its pulleys 56,000 pounds. When in motion, a pressure of about five pounds applied to the end of the shaft would force the revolving mass as far as the collars on the shaft would permit. In designing my friction machine I used the method of sustaining the upper spindle of the friction machine between collars revolving in reverse directions as described in the paper.

The conclusions of the experiments of Mr. Tower in 1883 are in accord with those of mine, made in 1879, whenever the same issues were considered, although the apparatus was dissimilar, and he confined his attentions to pressures of from 100 to 625 pounds per square inch.

His apparatus was a refinement of the method used by Hirn in 1856, both using the friction of a weighted cap upon a cylindrical bearing. Hirn secured a weighted scale-beam to this cap and kept it in equilibrium by placing sufficient excess of weights in the scale pan which was pulled upward by the revolution of the journal. Tower hung weights in a cradle suspended on knife edges below the cap, and obtained the frictional moment by the torsion of the cap produced by the rotation of the journal. Such small measurements introduced many difficulties in matters of observation, and later he modified his apparatus by placing a sufficient number of weights in a scale pan at the end of an arm projecting from the cap of the journal to keep it in a position of equilibrium.

In my first paper on friction reference was made to experiments showing that the lubricating property of oil was not coincident with its specific gravity or fluidity, and it is a matter of gratification to learn that this is so fully confirmed by the broad practical experience of Prof. Arvine.

The following table contains the results of some experiments upon this subject.

Manufacturer or Dealer.	Kind of Oil.	Coefficient of Friction at 5 lbs. per sq. inch, 560 feet per minute. 100° Fah.	Flash. Degrees Fah.	Loss by evaporation in 12 hours at 140° Fah.	Specific gravity at 60° Fah.	Weight per gallon at 60° Fah.		Fluidity at 100° Fah.
				per cent.		lbs.	ounces.	
Downer Oil Co.	32° Extra Machinery	.0756	284	5.50	.8680	7	3.72	76
" " "	Light Spindle	.1132	314	2.70	.8913	7	6.83	129
" " "	Heavy "	.1187	338	1.30	.8955	7	7.39	224
" " "	" "	.1208	326	0.95	.8951	7	7.34	245
" " "	Champion	.1732	350	1.22	.9055	7	8.72	186
Leonard & Ellis	Valvoline White Spindle	.1493	324	3.90	.8637	7	1.46	153
" "	" " Loom	.1401	318	3.30	.8616	7	3.27	168
" "	" Machine	.2243	286	7.20	.8763	7	4.83	471
Aiken & Swift	Bleached Winter Sperm	.0956	374	+0.30	.8836	7	5.79	155
" "	" " "	.1141	440	0.47	.8833	7	5.76	162
Olney Bros	German Spindle	.1190	322	1.90	.8970	7	7.59	149
" "	" A " Spindle	.1103	282	5.00	.8870	7	6.25	129
John P. Squire	Lard	.2181	...	+0.40	.9193	7	10.56	345
N. K. Fairbanks & Co.	No. 1 Lard Oil	.1850	...	0.32	.9180	7	10.39	362
" " "	Leaf " "	.2046	...	0.75	.9200	7	10.65	417
" " "	Dead Hog Lard Oil	.1865	...	0.55	.9185	7	10.45	365
" " "	No. 2 " "	.1885	...	1.25	.9180	7	10.39	366
" " "	Neatsfoot	.1937	418	0.37	.9170	7	10.25	397
" " "	Pigsfoot	.1950	488	+0.22	.9185	7	10.45	398
Alex. Boyd & Sons	Neatsfoot	.2427	440	0.80	.9228	7	11.03	434
Unknown	Seal	.1608	486	1.40	.9263	7	11.49	259
	Mixed paraffine and lard	.1166	290	2.38	.8878	7	6.36	157

Although only one series of temperatures is given, the work extended from 60 to 150 degrees, and the details are given in the former paper on friction.

This work was undertaken on behalf of the Factory Mutual Insurance Companies on account of the great number of fires ascribed to lubricating oil, either by spontaneous combustion or hot journals, and I have submitted those portions presumed to be of general mechanical interest. Within the last five years the mineral oils have been improved in their characteristics; and the use of pure animal oils unmixed with mineral oils has become infrequent. In 1878, out of 257 samples of lubricating oils, 53, or about one-fifth, contained from 10 to 30 per cent. of matter volatile at the ordinary temperature of a bearing. At the present time the proportion of such volatile matter in the mineral oils used in the textile mills, within my experience, is less than two and one-half per cent. Of course, I cannot say what it may be in the general market. With other improvements, the flash test of oils has been increased, thus adding to the element of safety.

It is difficult to state the measure of the improvements in the lubricating properties of oils, but they are undoubtedly very material.

It would be greatly to the advantage of the underwriters and of all consumers of lubricating oils, if pure mineral oils could be used for all purposes of lubrication, and oiling wool; but whenever a lubricant is to be subjected to severe usage, either by reason of pressure, excessive velocity, or poorly fitted journals, then an admixture of animal oil is very desirable; while for oiling wool, no substitute, capable of giving general satisfaction, has been found for lard or olive oil.

The purposes of the underwriters in interest in this matter of friction have been reached, and the work is at an end for the present, but I trust that some one will investigate the friction of oils solely from a technical stand point, and obtain results capable of permitting a solution of the problems of mediate friction in matters pertaining to proportions of journals, use of lubricants, and also the physical characteristics defining the lubricating quality of oil.

In breaking down the custom of applying the laws of solid friction to problems of lubricated surfaces, there is no rule left to fill the place.

CLXIV.

NON-CONDUCTING COVERINGS FOR STEAM PIPES.

[*Conclusion.**]

BY PROF. JOHN M. ORDWAY. PRESENTED BY C. J. H. WOODBURY, BOSTON.

INTRODUCTION.

This paper forms the conclusion of the investigations made for the Factory Mutual Insurance Companies upon non-conductors for steam pipes, and contains experiments upon the relative value of methods suggested in the course of the discussion upon this subject at the last annual meeting of this Society. This supplementary work was undertaken in order to decide by actual experiment whether the value of a non-conductor can be determined in a more accurate manner by an actual calorimetric measurement of the heat radiated from a protected pipe containing steam in active circulation, as is generally the case where such protection is used; or whether the desired facts could be obtained by the usual method, estimating the loss by radiation on the assumption that it was represented by the thermal equivalent of the water entrained in a pipe of quiescent steam. The comparative results have in every particular given the preference to the method originally used in this work, of measuring the actual loss by radiation by means of calorimeters fitting around the pipe covering.

In carrying out this second series of experiments, measurements have also been made of the radiation from envelopes of numerous materials which have been suggested since the former paper was read. In this connection it must be emphasized that the use of combustible organic material is not recommended for steam pipe coverings unless protected against fire by water-glass, or some equivalent material which is both non-combustible and adhesive.

C. J. H. W.

The experiments, of which an account is given in the former report, were limited mostly to samples of pipe coverings sent in by different manufacturers. It had been hoped that many other trials

* See Vol. V. Transactions A. S. M. E., pp. 73 and 212.

might be made, but steam could be had only a few days after the middle of May, and the building was altered during the following half year, so that the work could not be resumed till winter. At length the machinery was removed, the room was extended, and

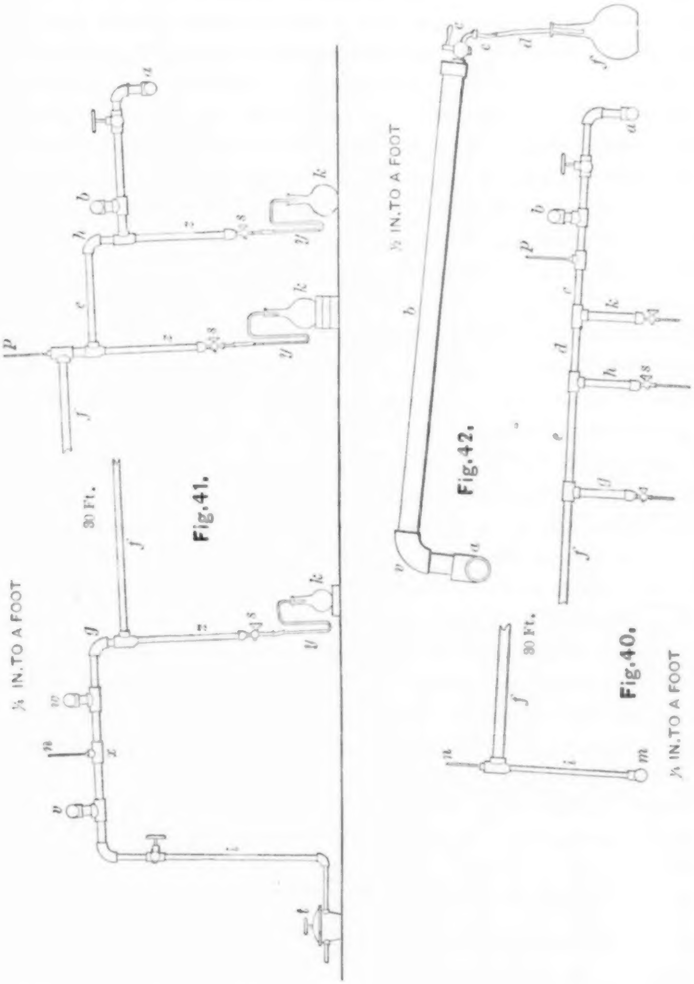


Fig. 41. Fig. 42. Fig. 40.

special apparatus was fitted up for continuing the investigation. The steam was now taken from a large pipe connected with three boilers about one hundred feet away.

To make experiments on condensation, a two-inch pipe was set

at a convenient height, with an upward slope of about four degrees, as shown in Fig. 40. Three branches, *g*, *h*, *k*, directed downwards, served respectively to receive the returning water from the 1, 2 and 30 foot lengths *d*, *e*, *f*. To the elbow *b* was connected a blind pipe 30 inches long, similar to the one shown in Fig. 42. The steam, after traversing the 2-inch pipe, passed downwards through the pipe *i* and entered the general circulation of the building at *m*. A valve interposed between *i* and *m* is not shown in the drawing. Thermometers *n* and *p* showed the temperature of the steam, which was generally about 155° C. = 311° F.

The heat within the pipe being so high, of course the condensed water was overheated and could not be drawn off, from time to time, without some loss by boiling and vaporization during its escape. This is a trouble which necessarily occurs in all experiments by condensation, and I was unable to devise any way in which the error could be wholly avoided. It was lessened as much as possible by drawing the water very slowly through a double siphon of glass tube, of ⅛ inch bore, into a glass flask. This arrangement is shown in Fig. 41 at *y*, *k*. A small brass tube would answer as well, for one can tell when the water is all out by a sudden change in the rushing sound.

It is also difficult to get stop-cocks which are tight enough and will continue so for any length of time. It was expected that whatever water came forward with the steam would be intercepted by the pocket *k*, and that therefore what collected in *h* would be due to the condensation by *e*, and what was drawn off from *g* would show the amount condensed by *f*. But these reasonable anticipations were not realized. In fact, on trial of the naked pipe, the condensation by the two-foot piece was almost as great, apparently, as that by the thirty-foot length.

The whole was then wrapped with cotton batting, and still the anomaly continued. As the mean of two trials the

30 ft. length	gave	19	grams	of	water,	per	foot,	per	hour.
2 ft.	"	" 177	"	"	"	"	"	"	"
Blind 2½ ft.	"	31	"	"	"	"	"	"	"

Now the blind pipe must have given the most nearly correct result, for whatever entered it had no chance to go beyond or to go back. Hence it is evident that much water was brought in with the steam, and it was not all retained by the first pocket, but some was pushed on into the second; and much that should have run

back into the third pocket, after being condensed in *f*, was thrust forward into the pipe *i*. It seemed quite possible that the cooling in *f* produced mostly floating mist instead of running water, and this mist was swept onward by the current of steam.

As some changes in the boiler connections rendered it necessary to suspend the trials for a time, it was thought best to make a new arrangement of the pipe meanwhile, with a view to obviate the difficulty already experienced. Accordingly the whole structure was altered to the form shown in Fig. 41. The steam was admitted as before through the pipe *a*, and the final outlet was through a trap *t* into a pipe returning to the hot water tank. To the elbow *b* was attached a 2½ ft. blind pipe, to *w* a 10 ft. blind pipe, and to *v* another 5 ft. long. This latter is shown, in side view, in Fig. 42. But all the blind pipes were furnished with the glass siphon tubes like *y*, Fig. 41. The pockets *z* were made of 1¼ inch pipe 2 ft. long. All the siphon pipes *y* were held steady by upright wooden supports not shown in the drawing. The running pipes *e* and *f* were 2 and 30 ft. long respectively.

This arrangement was an improvement on the former one, as it was now possible to compare blind pipes of different lengths; but with the running pipes the anomaly still continued. Thus, after covering only the pockets with cotton batting, an average of 5 trials showed for the condensation per foot per hour:

2½ ft. Blind	178 grams.
5 " "	189 "
10 " "	181 "
2 " Running	328 "
30 " "	140 "

After covering the whole with cotton batting, so that the external diameter was about 4 inches, the apparent condensation in three trials per foot per hour was:

2½ ft. Blind	41	45	46
5 " "	40	40	39
10 " "	38	38	38
2 " Running	143	78	72
30 " "	14	23	28

So while the blind pipes were pretty uniform in their yield, both the running pipes were very variable. The 2 ft. length gave nearly twice as much as it should have done, and the 30 ft. piece gave little more than half of the true quantity. In many other experi-

ments, the results with the running pipes were equally irregular and at variance with the probable truth.

The time required for other experiments did not allow any other transposition of pipes, and therefore I was unable to determine for a certainty why the long running pipe yielded so little condensed water.

It seemed likely that mist or "priming" in the steam might account for some of the irregularities, and so condensation calorimeters were fitted up for testing the quality of the steam. One of these is shown in section in Fig. 43. Fig. 44 shows the same as seen from above, *a* the calorimeter body, made of No. 25 sheet brass, *b* the cover, provided at the top with a short tube *p*, so that the cotton packing may be kept from interfering with the stem *l* of the stirrer *m*. This stem is kept in position by the pin *w* passing through a hole in the brass cross *k*, whose ends are soldered to the bottom of the brass vessel. The stem is steadied by passing through a hole in the wooden crossbar *o*, which is attached to the box *s*. The stem is turned by means of the crank *n*. The stirrer *m* is made of a piece of inch board perforated in the center to receive the stem, and having the opposite long edges shaved down to form screw blades. The brass pipe *e* is for drawing off the water. It is closed at the lower end with the cork *x*. The pipe *c* serves for the introduction of water by means of the removable funnel *r*, and its upper end is closed with a cork when the funnel is taken out. The neck *y* of the calorimeter is extended out at *d*, so that the small pipe *i* may not be disturbed when the cover *b* is taken off, and this side extension is closed with a cork through which the pipe *i* passes. The brass tube *i*, having an internal diameter of one-twelfth of an inch, or 2.1 mm., is soldered at one end to the cap *z*, which screws on the stop-cock *h* connected with the tee *g* of the steam pipe *v*. The tube terminates in the coil *j*, which is open at the end. A thermometer *t* is inserted through the side tube *f* and held fast by a perforated cork.

The brass calorimeter is inclosed in the wooden box *s*, which is large enough to admit a good packing of cotton wool.

A short brass tube is soldered to the inner end of the cock *h*, and extends to the central part of the pipe *v*, so as to receive the best of the steam.

This calorimeter was connected with the tee *x* of Fig. 41. For two others, of simpler construction, the cocks were screwed directly into the elbows *h* and *w*, Fig. 41.

In the first place the water equivalent of the calorimeter must be determined once for all. For this purpose the vessel is filled nearly full of hot water. After stirring well, the temperature is observed. The hot water is run off and cold water of known temperature is quickly let in. The stirrer is turned for a few moments,

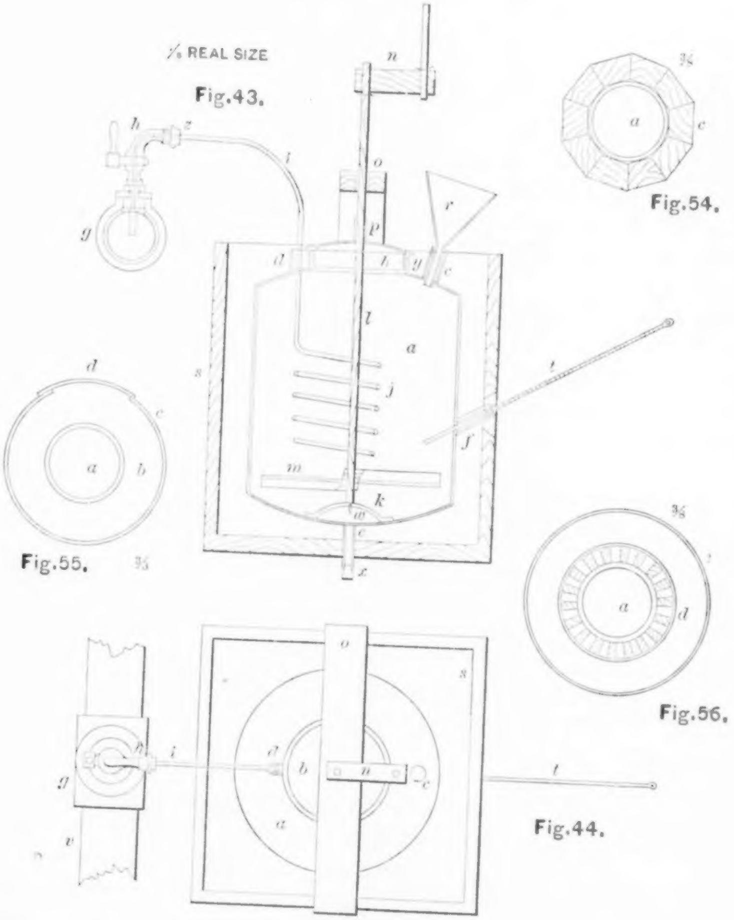


Fig.43.

Fig.54.

Fig.55.

Fig.56.

Fig.44.

and the height of the thermometer is noted. The warmed water is drawn off and weighed.

Now let $t' =$ temperature of hot calorimeter.

$t =$ temperature of cold water.

$T =$ temperature of the warmed water.

a = weight of water finally drawn off.
c = water equivalent of calorimeter itself.

Then $$c = \frac{a\,(T - t)}{t' - T}.$$

In trying the quality of steam, a weighed quantity of cold water is put into the vessel a (Figs. 43, 44)—about 8 litres, or enough to cover the coil j. After stirring awhile, the height of the thermometer t is noted. Steam from the pipe v is now admitted into the water through the pipe i and the coil j for about three minutes. After a minute or two the temperature is observed, and the warmed water is drawn off and weighed. The temperature of the steam is observed at the beginning and at the end of the experiment by means of the thermometer p, Fig. 41, inserted in a thimble in the steam pipe.

Then let a = weight of cold water.
d = " " hot "
$b = d - a$ = gain in weight.
x = weight of live steam.
$b - x$ = weight of mist.
c = water equivalent of calorimeter, previously found.
t = temperature of cold water.
t' = " " steam.
T = " " hot water.
h = total heat in steam at t', as found in Regnault's tables.
h' = total heat in water at t'.
H = total heat in water at T.
n = latent heat of steam at t'.

It is obvious that
$$\frac{(a + c)\,t + hx + h'\,(b - x)}{a + b + c} = H,$$
or $$x = \frac{(a + c)\,(H - t) - b\,(h' - H)}{n}.$$

Thus, in one trial, the temperature of the steam was found to be; at

4.17 P.M.	=	155.8°C.
4.22 "	=	155.4°
average		155.6°

$$
\begin{aligned}
a &= 8080g. & d &= 9000g. \\
b &= 920g. & c &= 210g. \\
t &= 10.4^\circ & h' &= 156.9. \\
t' &= 155.6^\circ & H &= 61.85. \\
T &= 61.7^\circ & n &= 496.8.
\end{aligned}
$$

$$
x = \frac{(8080 + 210)(61.85 - 10.4) - 920\,(156.9 - 61.85)}{496.8} = 674.
$$

Here, then, were 674*g.* live steam to 246*g.* mist.

In fifty-two such trials, twelve showed a quantity of mist ranging from 7 to 57 per cent. of the whole increase. So what was mentioned in the former report as being a possible source of error in determining the loss of heat from steam pipes by the apparent condensation, proves to be no imaginary trouble. The steam is liable to be, at times, very far from dry, and the damp state may come on and cease undetected in the interval between two trials of quality, unless these trials are kept up in almost unbroken succession. The frequent detection of a faulty condition of things has by no means increased my confidence in the condensation method. Still, when the fireman is skillful and careful and the steam is used the day through for uniform work, the results attained by condensation will often be approximately correct.

It may be of some use, then, to compare the condensation in a covered blind pipe exposed to the air, with the heat radiated into a water calorimeter applied to the covering.

The 30-inch blind pipe, at *b*, Fig. 41, was covered with straw board so as to inclose a half-inch air space all around, and over this straw board was applied an inch of hair felt, with a wrapper of cotton drilling. When the covering was exposed to the air, the average condensation appeared to be 40 grams per foot per hour.

A calorimeter being applied for 28 inches of the length, the average condensation was 39 grams.

A calorimeter being applied to only 14 inches of the length, while the rest of the covering was exposed to the air, the condensation was apparently 38 grams per foot per hour.

So the transmission into water differed very little in amount from the radiation into the air. And there is little room for the objection to the water calorimeters that they place the coverings under different conditions as to radiation from those which practically occur in ordinary exposure.

The 28-inch calorimeter above mentioned received 20,042 kilocent. heat units per foot per hour, which would correspond to a

condensation of 40.3*g.* of dry steam at 156° C. to water at 156°. And considering the uncertainty as to the amount of vaporization during the drawing, and as to the actual temperature of the water before drawing, we may say that the 39 grams actually obtained come quite as near the theoretical yield as could be expected.

Experiments made with a 14-inch calorimeter on the five-foot blind pipe at *v*, Fig. 41, covered with slag wool inclosed in straw board, showed heat given out answering to 33.7*g.* of water condensed, while the actual yield was 31.0*g.*

And trials with a 14-inch calorimeter on the blind pipe at *w*, Fig. 41, covered with silicated cotton-seed hulls, indicated 54.4*g.* steam condensed, there being really only 50 obtained. But the coverings of slag wool and cotton-seed hulls were not perfectly uniform in thickness throughout, and the calorimeters were put on where the thickness was not exactly the average.

As the amount of condensed water increases, and there is consequently a necessity for much more frequent drawing, the loss of water becomes greater. Thus the 30-inch blind pipe was evenly covered with asbestos paper wound round to a total diameter of 4 inches. The water drawn off now amounted to 54*g.* per foot per hour, and at the same time the calorimeter indicated a loss of heat equivalent to the liquefaction of 66*g.* of dry steam.

The 28-inch calorimeter was made partly for trying whether calorimeters of different lengths would show any difference of results, other things being equal. As there was a chance to draw it on over the end of a blind pipe, it was made of a simpler form than those previously described. Fig. 45 gives a side view, Fig. 46 a longitudinal section through *AB* of Fig. 47, which is an end view. Fig. 48 shows a transverse section through *CD*, seen from behind. *a* is the outer shell, *c* the inner. For greater strength the top is swelled out at *b*. The water is poured in through the tube *d* with a loose funnel, *f*; *e* is a tube for drawing off the water, closed with a cork *g*. The thermometer *t* is inserted through the tube *h*, a perforated cork making it tight. Through the neck *l* passes a stirrer which consists of a flat pine paddle, *p*, fastened at the thicker end into the bent brass tube *n*. In the other end of this tube is fastened the wooden handle *m*. The tube turns or oscillates on the pin *r* which is held by the ears *w*. The shell is made of No. 25 brass. It slips endwise over a covering of moderate diameter and needs no clamps, and as there is no joining of halves the heat is better confined.

In Table III. *h* and *i* show the results of trials of the same covering with the ordinary clamped 14-inch calorimeter, and with this new 28-inch one; the numbers in *i* were found with the new and those in *h* with the older. The difference proves to be too inconsiderable to make it worth the while to use a calorimeter more than

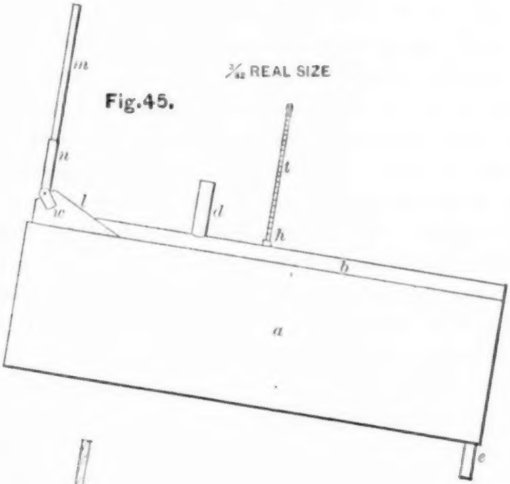


Fig.45.

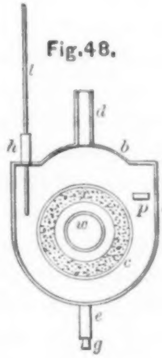


Fig.48.

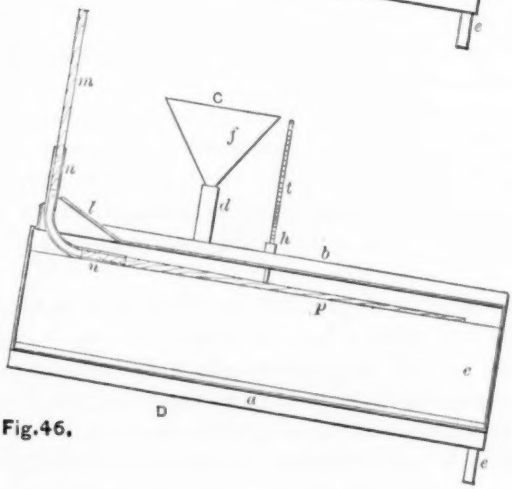


Fig.46.

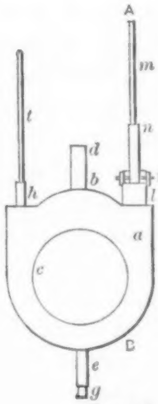


Fig.47.

14 inches long. A calorimeter of this general form presents some advantages over those made in halves. It has but one inlet, one outlet, one stirrer, one thermometer, and no clamping bolts are required. There is less work in its construction, and somewhat less material. The inner shell can easily be made truly cylindrical,

while it is difficult to get half cylinders exactly right. When one has a blind pipe suitably arranged, the whole calorimeter is more readily applied and more easily supported in place. On the other hand, a blind steam pipe requires a frequent drawing off of the condensed steam, and this gives no little trouble. A second disadvantage is that when a whole cylinder is to be slipped over the covering, from the end, the fit must be rather loose. But this last matter is really of little importance; for experiments have been made to determine whether a loose fit is likely to induce error, and the results are given in Table III., *d*, *e*. The covering, in these cases, was of ground cork and water-glass, moulded on the hot pipe in a wrapper of cotton drilling. The calorimeter used in *d* had an inner diameter of 5⅜ inches, and was so loose that it had to be held in place by wooden wedges. The other, used in *e*, had an inner diameter of 5 inches, and made a very close fit, after some inequalities of the cork had been shaved off. The difference between the 59.2 and 60.1 heat units is no greater than the variations of one and the same calorimeter on successive days.

We may fairly say, therefore, that very close contact of the calorimeter with the pipe covering is by no means essential to accuracy. A little looseness does no harm, if the non-conducting coat of the calorimeter itself is made so close at the ends that no warm air can escape out of the unfilled space.

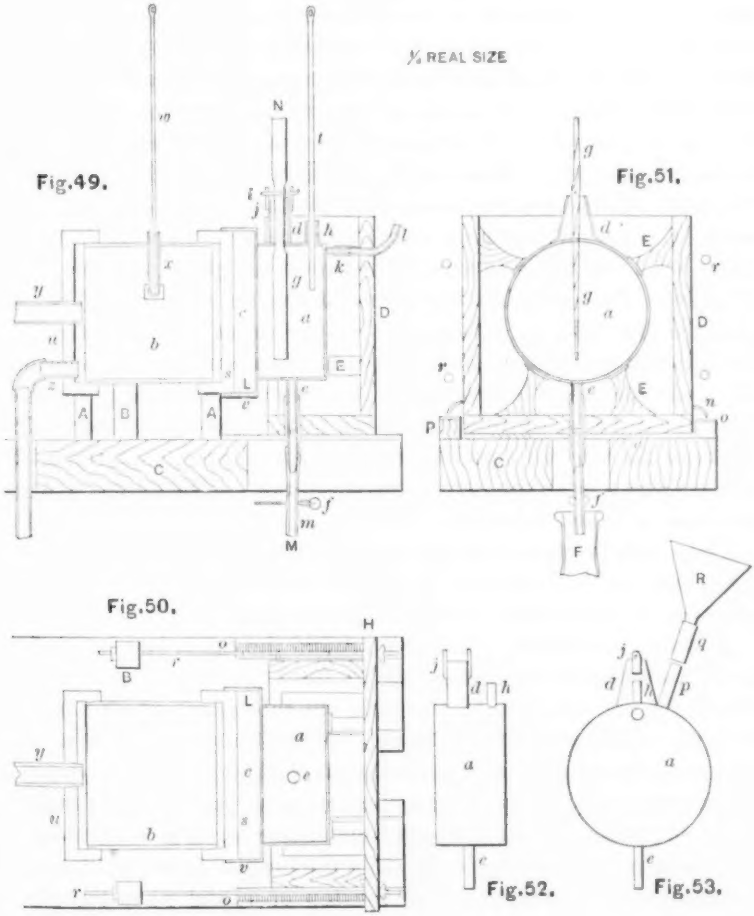
But when a calorimeter that is somewhat too small is drawn close around an elastic pipe covering, there is a possibility of error, not on account of compression and consequent increase of density, but because the diameter of the covering itself is lessened, and a decrease in the thickness of the non-conductor allows more heat to be transmitted.

This effect of lessened diameter cannot be measured to a nicety with any of the apparatus which I have described hitherto, because it is almost impossible to apply any covering to a cylindrical pipe so as to have it of perfectly uniform and known thickness. So I was led to devise a plan by which coverings might be varied in thickness, and the thickness could be made equable and be exactly measured. The apparatus represented in Figs. 49, 50, 51, 52, 53, dissimilar as it is, was suggested by one contrived by Andrée and described in the *Teksnisk Tidskrift*, xiii., 131. The drawings are made on a scale of one-eighth of the actual size.

Fig. 49 shows a longitudinal vertical section. Fig. 50 gives a longitudinal horizonal section. Fig. 51 represents a vertical cross

section through *MN* of Fig. 49. Fig. 52 is a side view, and Fig. 53 a front view of the canteen-like calorimeter.

The six-inch steam pipe *b* was screwed into the caps *s* and *m*. The outer flat part of *s* had been turned to a true surface. At the other end, *u* was tapped to receive the pipe *y*, bringing steam, and



the exhaust pipe *z*. The thimble *x*, capped at the lower end, was screwed up through the top of the large pipe, to receive the thermometer *w*. The calorimeter *a*, made of thin brass, is provided with the pipe *p*, through which water may be poured by means of the funnel *R*. Whenever occasion requires, this funnel is connected

with *p* by an India-rubber tube. Then the funnel is removed, and the end of *q* is corked. The brass outlet pipe *e* is lengthened by an India-rubber tube *m*, which may be opened or closed with the pinch-cock *f*. The tube *h* serves for the insertion of the thermometer *t*, a perforated cork serving to hold the thermometer in place. The $\frac{1}{12}$-inch overflow pipe *k* is lengthened out with an India rubber tube *l*, which can be turned up after the calorimeter is filled with water. This overflow pipe might be dispensed with, for, in fact, finding out just how much water would fill the brass box, I have generally put in a measured quantity. The neck *d* serves to receive the pine-wood paddle *g*, which turns on the pin *i*, that rests in holes of the ears *j*. These thick brass ears are soldered on, because the neck itself is too thin to support the pin. The calorimeter is held in place in the pine box *D* by the pine supports *E*, bits of thick wool felt being interposed between the props and the brass. The front of the pine box has the projecting ends *H*, through which pass the four bolts *r*. These bolts at the other end pass through holes in the uprights *B*, so that the pine box carrying the calorimeter and sliding on the strips *V* may be adjusted at any distance from the cap *s*, by turning the thumb nuts. The space intervening between the faces of *a* and *s* can be measured exactly on the millimeter scales *o*, with the help of the pointers *n*. The guides *P* keep the faces of *a* and *s* parallel.

The whole is supported at a convenient height on the shelf *C*, the steam box *b* being held up by the soapstone supports *A*. The pine box is well packed within with cotton wool.

A strip of cardboard *v* is drawn around the cap *s*, and fastened to it with water-glass cement, so as to form a hood of any desired depth, for the reception of the different substances whose non-conducting qualities are to be tested. When the substance is a powder, a cardboard ring *L* is secured to the edges of the hood *v* and the calorimeter *a*, by pasting around strips of paper so as to make a tight box, of which the naked calorimeter face forms most of one side, and the face of *s* the other. To allow the powder to be put in, a strip about an inch long is cut out of the top of *v*, and when the whole space has been filled this strip is laid back again, and the whole is made tight by pasting over it a strip of paper somewhat wider and longer. The hood is surrounded with cotton batting during the trial.

When the experiment is completed, a small strip may be cut out of *v*, low down on the cylindrical side, and the powder can be

swept out through the hole. Then the bit of cardboard may be fastened on again by pasting over it a larger piece of paper, and the hood is ready to receive some other substance.

In making a trial, the steam is let into the box *b*, and when the thermometer *w* indicates that the box is fully heated, the calorimeter is filled with water several degrees colder than the air of the room. The time, the height of *w* and *t*, and the temperature of the room at the height of the apparatus are noted down. The observations are often repeated till the water is as much warmer than the average temperature of the air as it was colder at first. Of course the paddle *g* must be well oscillated before every observation. Finally, the water is drawn into a flask and weighed.

The necessary calculations are not very complicated. Thus in one experiment with fossil meal:

Water at first was at	5.8° C.
" " last "	31.5°
Gain therefore =	25.7°
Average temperature of room	18.4°
" " " steam	154.1°
Time =	299 minutes.
Weight of water =	1,326 grams.
Water equivalent of calorimeter =	50 "
Surface of calorimeter face =	0.0182 sq. m. = 0.196 sq. ft.

$\frac{60}{299} \times \frac{1326 + 50}{1000} \times \frac{25.7}{0.0182} = 389.9$ kilo-cent. heat units per sq. meter per hour; or,

$\frac{60}{299} \times \frac{1376}{1000} \times \frac{25.7}{0.196} \times \frac{9}{5} \times 2.205 = 143.7$ Pound-Fahrenheit heat units per sq. foot per hour.

Generally, at least three trials have been made and the results have been averaged. But in the case of animal and vegetable fibers it is hardly worth the while to repeat experiments with the same lot, because these organic matters are considerably scorched by a few hours' exposure to a heat of 155° C., and of course they are no longer the same substances as before. For such things it would be better to have, instead of steam, a current of hot water, of constant temperature, running through the box.

The pointers being arranged, in the first place, to indicate 0 when the faces of the calorimeter and the steam-box are in contact, of course the thickness of the stratum to be tried can be measured

very exactly. Moreover, an elastic, coherent substance, like wool, can be compressed to any desired degree by turning the four thumb-nuts. But when a powder is to be compressed a little, it may be crowded into the tight hood at the top aperture by ramming. Such powders as can be made to cohere by strong pressure, like magnesia and zinc white, may be compacted by forcing them into a mould with a powerful press. The cake being made a little large, can be shaved to the required dimensions, the edges being trimmed so that the disc will just slip into the open hood.

This apparatus is very convenient, and requires but a moderate amount of material for a trial. It would be still better to have both caps turned to true faces, so that two calorimeters could be used at a time, for it is almost as easy to attend to two trials as to one. In this case, of course, the steam pipes would be let into the cylindrical sides of the steam box.

Tables V. and VI. give the results of a series of experiments extending through four months.

The first column gives the names of the substances tried. The second shows their thickness in millimeters. The numbers in the third column are obtained by calculating the weight of 1,000 cubic centimeters of the substance from the known bulk and the weight as determined when the trial was just finished, while the stuff was in its driest state. The specific gravities given in the fourth column were determined by weighing under a liquid which expelled all the air. The animal and vegetable fibers, lampblack, magnesia, cork, charcoal, plaster of Paris, plumbago and chloride of sodium were weighed in toluol, the rest were tried with water. There may be some doubt about the feathers in 13 and 24, as it is exceedingly hard to get all the air out of them. The weight per litre divided by the specific gravity and by ten, shows what percentage of the whole bulk is occupied by solid matter, the rest being the air in the interstitial spaces. Thus in number 2 there is only one cubic centimeter of cellulose to 99 c. c. of air, and the great efficiency of this soft pad is almost wholly due to the stagnant air. The sixth column shows how many kilograms of water would be raised 1° C. by the heat which the substance in question transmits in an hour, through each square meter of the specified thickness, the surfaces being flat. In the last column these numbers have been multiplied by $2.205 \times 1.8 \times 0.0929 = 0.3687$, to find how many pounds of water would be heated 1° F. in an hour for every square foot of the transmitting surface.

In Table V. the substances are arranged and numbered in the order of efficiency. In Table VI. the numberings correspond to those of Table V., but the arrangement is by groups of the same substance in different states and thicknesses. After organic fibers come the different varieties of carbon; then Pattinson's calcined magnesia, and magnesia alba; then the different forms of silica. From "Omahalite" on, there is little chance for grouping. What I have called Omahalite, for convenience, in Nos. 42, 43, is a very light mineral, in coarse powder, which was sent from Omaha, Neb., for trial. It had been separated into the coarse and the fine by sifting. The fine contained about 70 per cent. of silica, 7 of alkalis, 15 of alumina, and 8 of water. It is in small scales, appearing under the microscope like fragments of skeleton crystals, and owes its lightness to its scaly character. Whatever other economic value it may have, it certainly has very little for covering steam pipes.

In Table VI., 1, 3, 4, 26, 36, 39, the same lot of wadding was tried first 50 mm. thick, then it was screwed up to 40, then to 30, and so on. In 2 and 17 the same lot of carded cotton was used, and in 23 the outer half of it was tried. But in 10, 20, and 21 a new quantity of the very clean soft batting was used. In 13 and 24, the feathers were put into a bag of very thin muslin, new feathers being taken each time.

The sand which was to be used in 52, 53, was first well washed, so as to float off the finest part, and the residue, after drying, was sifted with a sieve of 20 meshes to the linear inch. What passed through was separated into coarse and fine with a sieve of 40 meshes to the inch. It was nearly pure quartz. The Plymouth sand was treated with muriatic acid, and well washed to take out the oxide of iron. After drying, it was passed through a silk bolting cloth of 14,400 meshes to the square inch.

The fossil meal, before being used for 34 and 37, was separated by elutriation from about 20 per cent. of sand.

Table VII., including only such trials as were made with the uniform thickness of 25 millimeters of material, is arranged according to the bulk of air contained in 100 measures of the substances as they were used. This table may serve to facilitate the comparison of different articles occupying nearly the same absolute space, such as the groups *d, e, f, g—i, j—r, s—G, H—R, S.*

The table shows what astonishing air traps are many of the non-conductors that we have to deal with. We need not wonder that the calcined magnesia of the druggist acts so much like a fluid, for

every particle surrounds itself with a thick air cushion, and about 98 per cent. of the space that it fills is taken up by the elastic fluid.

It was very desirable to find the conducting power of discs of solid chalk, heavy spar, quartz, plumbago, rock salt, and anthracite coal, since these additional data are needed for a full discussion of the results already obtained. But time was lacking.

With other than compact solid substances, the results are of course complex, as they depend partly on the closeness of contact of the particles of solid matter, partly on their specific conducting power and partly on the friction which the particles exert on the included air. The fewer the points of contact of the solid grains, the less chance will there be for the transmission of heat by conduction; and the more rigidly the air is held in the interstices, the less transmission will there be by convection. Air alone, as is shown by 50 in the tables, transfers much heat when it can move about in a closed space—unless, indeed, the source of heat is placed at the top. The usefulness of mere air spaces has been much overestimated, for they can rarely be placed so as to render much service.

The air no doubt slides much more freely over smooth particles, like those of plumbago, and this must account, in part, for the great difference between the efficiency of the same absolute bulk of chalk and black lead, as shown in *I*, *J*, Table VII. And it is not unlikely that the great difference between wool and asbestos, as shown in *u* and *v*, is largely due to the smoothness of the soapy mineral fiber. It is hard to conceive of anything better fitted to counteract the mobility of air than the irregular twists of flattened cotton fiber, the crinkles and scaliness of wool, the fringed edges of down filaments, or the feathered angles of snow crystals. The wonderful structure of the minute diatoms which make up fossil meal also accounts for the efficiency of this light silica as a non-conductor.

When more and more organic fiber is crowded into a given space, the thickness of the stratum remaining the same, the transmissive power appears to be diminished till a certain limit is reached, beyond which there comes an increase. Probably then the mobility of the air has been brought to a minimum, and the proper conducting power of the fiber begins to act more decidedly.

The statement of Peclèt requires some qualification when he says: * "Il est important de remarquer, que la conductibilité des matières textiles étant sensiblement indépendante de leur densité;

* Traité de la Chaleur, 3me Ed., i. 407.

il s'ensuit necessairement que leur conductibilité est la même que celle de l'air stagnant."

The tables show that the compression of non-fibrous matters, like lampblack, fossil meal, magnesia, and zinc white, increases the conducting power in a marked degree. And this is a strong argument against using such things in the form of a paste to be plastered on.

But it is worth the while also to notice, as a matter of much practical and economic importance, that the quantity of the substance remaining the same, compression which lessens the thickness of a covering thereby decreases its heat-retaining power in no small degree. This may be seen from the trials 1, 3, 4, 26, 36, and 39, in Table VI., in which the same quantity of wadding was successively reduced to 40, 30, 20, 15, and 10 mm. in thickness, and the transmission was at last almost quadrupled. Also, in 2 and 17, the same carded cotton was condensed from 50 to 25 mm. thick, and the efficiency was lessened 44 per cent.

The obvious moral is: Use any non-conductor light and thick, rather than dense and thin.

Many new experiments on actual pipe coverings have been made, to supplement those of which an account is given in the former report, and the results are shown in Table III., in the order of efficiency. Table IV. includes the items of Table III., and those of all but the more complex coverings of Table I., arranged in groups so as to show the effect of various substances used in different ways. Thus, those specimens in which hair-felt forms the prominent constituent are put first, and those are brought together in which an air space is a characteristic feature.

For slag wool, ashes, dry fossil meal, and mere rice hulls, cases of straw board were made around the pipe, as formerly described, except that the straw board was shaped by binding it, while damp, around a cylinder of the right size and letting it dry before taking off. Fig. 30 shows a transverse section of such a case, *a* being the pipe, *b* the hollow space, and *c* the case with its cover *d*.

Air spaces were made with smaller straw board cases, held off from the pipe by flexible rings. These rings may be made of narrow strips of cardboard, which are painted over on the inner side with water-glass; then cheap vial corks are stuck on endwise, and the whole is bent around the pipe as shown in Fig. 56, *d*, and held in place by pasting down the overlapping end of the cardboard. Or bits of thick asbestos cord may be wet with water-glass and drawn around the hot pipe. Or, again, long strips of thick paper coated with the adhesive

silicate may be wound round and round till they have formed the desired thickness. But the air spaces in 47 and 48 were formed by the calorimeters themselves, paper props being used at the ends. In 19, the space was $\frac{1}{4}$ inch thick, in the rest about $\frac{1}{2}$ inch. The multiplex air spaces of *u* were made by winding around the pipe spirally some asbestos wicking, then putting on silicated straw-board, then winding around a spiral of hemp cord, and so on till there were four spiral cords and four cases.

The result of the multiplex arrangement was not good enough to pay for the trouble and cost; but, by comparing *u* with 48, we see that a much divided air space is better than a simple one.

Comparison of *h* and 20 with 1 and 2, of 19 with 16, and of *o* with *l*, do not turn out to the advantage of air spaces. And in 34 and 36 we see that the slight gain will not pay for the extra cost of material. It would be better in all cases to fill up the air space with fossil meal, which would be far more efficient in preventing the scorching of the organic matter.

A mixture of fossil meal with one-seventh of its weight of cork sawdust proved somewhat more efficacious than fossil meal alone; but as both were used as they were procured in the market, possibly the fossil meal was not just alike in the two cases. Indeed, the article used in *c*, on examination proved to be not the best, for by washing it yielded 20 per cent. of sand. Another sample was obtained, but not used, for it contained much more sand, and, as it had been burned, there were mny hard baked lumps in it. For the use in question, diatomaceous earth ought to be freed from sand and left unburned. From its peculiar structure it is more tractable than other light powders like ashes or magnesia, and it can easily be applied in the dry state, in which it seems to have the maximum efficiency. As this substance is abundant in various parts of the world, and can be afforded at a low price, it is likely to come into pretty general use for coverings. It works quite as well as slag wool, and is not liable to the same objections. Its dust is not irritating; it is not decomposed by heat, moisture, and carbonic acid; it does not undergo continual shrinkage by jarring; it does not cause the corrosion of pipes.

I was able at length to get some slag wool of the best quality, very light, tolerably elastic, and almost wholly free from shot-like particles of slag. This was applied to the whole length of the blind 5-foot pipe (*v*, Fig. 41). It gave a very good result, as shown in *b*, Table IV.

After making the trial, a portion of the cover of the straw-board case was removed, and water was poured in from time to time and allowed to evaporate. This was done for about four weeks. The inner part thus became a more compacted mass, crushing easily between the fingers to a somewhat spicular powder. Water digested with this became very faintly alkaline, and showed the presence of a sulphate. Of course, a part of the sulphide of calcium in the original substance had been oxidized to sulphate. The steam-pipe was not particularly rusty. In a published account of some cases of corrosion said to be caused by slag wool,* it is suggested that sulphuric acid had been set free and had acted on the iron, but the author does not explain how the acid could be set free from a strong base when there is an excess of the base present. Chemists will hardly admit that sulphate of calcium can be dissociated by anything short of an intense red heat. Sulphate of calcium promotes the oxidation of iron, as every one knows who has left wet plaster of Paris in contact with the metal; but it is not because sulphuric acid is set free first.

The slag wool of $35\frac{1}{2}$ was not of as good quality as that used in *b*, and it was not thick enough to do well.

One of the best non-conductors is cork. It is strong, elastic, waterproof, and not very changeable. The "Société Anonyme des Liéges Appliquées à l'Industrie," of Paris, manufactures envelopes for steam-pipes, boilers, etc., of long strips of cork, so beveled at the edges as to fit exactly and form a polygonal prism, which is bound together with tinned wire. Such a covering, on account of its firmness and elasticity, is particularly suitable for locomotives, or any other apparatus exposed to jarring and shocks; but it is necessarily expensive, when the strips are made thick enough. By the kindness of the director of the company, I have received a covering long enough for a trial. It is made in 10 pieces, as shown in cross section in Fig. 44. Being hardly $\frac{5}{8}$ in. thick, this covering does not give a very favorable result. A greater thickness of this excellent material may be secured at very moderate cost, by cementing finely cut waste fragments of cork with water-glass. This conglomerate has not the strength of solid cork, but it makes a pretty firm and elastic coat, and when protected with a cloth wrapper it forms one of the best coverings yet tried. For the experiments *d*, *e*, parings of bottle-corks were cut up with a sausage-meat cutter, and moistened with one and a half times their weight of

* Transactions A. S. M. E., Vol. III., p. 230.

water-glass at 30° Baumé, and this mixture was applied to the hot pipe with the help of a wire cage. The water-glass does not soak in, but coats every bit of cork superficially, and so its cementing power is exerted to the best advantage. Thus by drying, the mass becomes strongly coherent. This covering can also be moulded in halves, without the cloth, and after drying it will bear handling. Prepared in this way, it can be applied to the pipe with great ease, and may be held on by putting around paper or cloth. As the inner surface becomes somewhat changed by long heating, it would be better to make larger half-shells of cork, and line them with $\frac{3}{8}$ or $\frac{1}{2}$ in. of fossil-meal paste. A covering made in this latter way I have not yet had a chance to try, but I believe it is, in all respects, one of the very best that could be devised.

For other coverings with a water-glass cement, I have tried rice chaff, cotton-seed hulls, and pine charcoal. The rice chaff requires its own weight of water-glass, and this ought to have the strength of 35° B. I used it at 30° B., and though for a while after drying the chaff was coherent, in the course of a few weeks the adhesion was very much impaired.

The cotton-seed hulls were those of rough seed cotton, covered with a short furze, which makes them lie light and loose. The hulls were mixed with twice their weight of water-glass at 30° B. It took some time for this mass to become dry, but it was pretty firm and elastic, and remained so. It is difficult to get an even coating with the rough woolly mixture, and this material proved less effective than some other things.

The charcoal was made of white pine wood by distilling in a pyroligneous acid retort holding a cord. It was ground up in a corn-cracking mill, and moistened with $1\frac{1}{2}$ times its weight of water-glass at 30° B. When dry the mass was coherent, and continued so, though a water-glass a little stronger would have been better.

I am thus particular to give the proper weight of the water-glass liquid, because a novice would be apt to put in a great deal more than is needed; and when the mixture is used too wet, the excess of liquid drains to the under side, and makes an unpleasant dripping. It is much better to take just enough, and stir patiently till a slight, uniform moistening is effected. Then the damp mixture may be inclosed in cloth, with the aid of the wire cage, and the water-glass does not come through the cloth. The mixture may be done easily with a hoe in a mortar-box.

Pine charcoal proves better than hard wood coal, and probably charred cork or tan-bark would be still better.

The stair or carpet pad of *p* and *v* is made of rolls of cotton roving laid side by side, knit together with cotton yarn by machinery. It is too expensive and combustible to be used for steam pipes, even if it made a more advantageous showing than it does.

The paper used in *w* did much worse than was expected. This covering was made by cementing the edge of a sheet of the best blotting paper along the pipe with water-glass and winding the paper around as tightly as possible. The other edge was pasted down with water-glass. Over this another sheet was applied in the same way, and so on, till the thickness of three-quarters of an inch was reached.

The asbestos paper of *x* was put on in the same way. Strangely enough the mineral and the vegetable paper show nearly the same conducting power. Paper that is to be used for non-conductors should evidently be made soft and spongy, like that of 16.

The ashes of *q* were taken from the 3-in. tubes of a boiler, and those of *r* and *y* were sent in for trial.

The carbon which made the plastered mass of 34 superior to that of 44 was what is left when the black liquor from the soda boil of wood paper pulp is dried down and the ignited residue is leached with water to recover the soda. It is something between ordinary charcoal and lamp-black.

As the outcome of what has been done hitherto we may say:

1. Trials of steam-pipe coverings by the method of condensation are liable to errors on account of the not infrequent priming of steam which may occur at any time of day. It is also difficult to draw off, without loss, the water that is condensed.

2. Some method depending on a constant temperature in the pipe, which can be looked to at any and all times, is preferable to one dependent on the dryness of steam, which is a matter that cannot be constantly watched.

3. The transmission of heat into the water of a calorimeter does not differ materially from that into free air.

4. The calorimetric method is preferable as being applicable to running pipes, and not requiring any special arrangement of side branches.

5. It is useless to make the testing apparatus of cumbrous dimensions, for as in chemical analysis we use a gram or less of the sam-

ple, instead of kilograms, so in physical experiments increase of size does not necessarily enhance the accuracy of the results.

6. Air chambers in pipe coverings are not advantageous, but it is better to fill hollow places with some light powder.

7. Compression lessens the actual efficiency of loose powders or fibres, by diminishing the thickness of the covering.

8. Of all the substances tried, the most advantageous are hair felt, cork, fossil meal, magnesia, charcoal, and rice chaff.

Slag wool would also be good if it could be made of a silicious slag free from sulphide of calcium.

Lamp-black is very efficient, and might do if it were not combustible and unpleasant to handle.

At first it might seem as though magnesia is too costly to be taken into account, but with the great abundance of useless magnesium salts in the Stassfurt deposits and the present exceedingly low price of soda ash, there is nothing but lack of demand to hinder a very economical production of magnesia alba, or even of calcined magnesia, the lightest and nicest of all incombustible non-conductors.

The following tables give the results obtained in this investigation since the preparation of the former paper on this subject, and which contains tables I. and II.*

* See Transactions A. S. M. E., Vol. V., p. 95, *et seq*

TABLE III.

		Diameter in Millim.	Weight per meter in grams.	Kilo-Cent. heat units 1 m. 1h.	Diameter in inches.	Weight per foot in oz av.	Pound Fahr. heat unit 1 foot, 1 hour.
a	Fossil meal and cork (12 p. c.), straw board	130	2,297	53.6	5⅛	24.7	64.8
b	Best slag wool, straw board	121	1,228	55.1	4¾	13.2	66.6
c	Dry fossil meal, straw board	130	1,934	56.6	5⅛	20.8	68.4
d	Silicated cork chips, drilling (calorimeter loose)	133	1,376	59.2	5¼	14.8	71.4
e	Silicated cork chips, drilling (calorimeter close)	133	1,367	60.1	5¼	14.7	72.7
f	Silicated pine charcoal, drilling	133	3,208	64.7	5¼	34.5	78.3
g	Air space, straw board, cork chips, straw board	130	1,116	65.3	5⅛	12.0	79.0
h	Air space, straw board, hair felt, drilling (short cal.)	130		65.3	5⅛		79.0
i	Air space, straw board, hair felt, drilling (long cal.)	130		65.8	5⅛		79.5
j	Cotton batting, cotton cloth	102	558	65.9	4	6.0	79.7
k	Silicated rice chaff, drilling	133	1,627	70.6	5¼	17.5	85.4
l	Dry rice chaff, straw board	130	1,553	71.9	5⅛	16.7	87.0
m	Air space, straw board, pine turnings, straw board	130	1,302	74.1	5⅛	14.0	89.6
n	Air space, straw board, paper pulp, straw board	130	1,274	75.8	5⅛	13.7	91.7
o	Air space, straw board, rice chaff, straw board	130	1,590	78.1	5⅛	17.1	94.5
p	Air space, straw board, cotton roving	121	1,357	79.3	4¾	14.6	96.0
q	Anthracite flue dust, straw board	127	3,497	80.1	5	37.6	96.8
r	Bituminous coal ashes, straw board	127	3,832	81.3	5	41.2	98.4
s	Cork chips	92	623	87.1	3⅝	6.7	105.3
t	Silicated cotton seed hulls	130	4,743	88.9	5⅛	51.0	107.6
u	Multiplex air spaces with straw board and cords	111	2,613	95.4	4⅜	28.1	115.3
v	Air space, straw board, thin cotton roving	108	772	102.6	4¼	8.3	124.0
w	Blotting paper	102	2,688	105.6	4	28.9	127.8
x	Asbestos paper	102		108.5	4		131.2
y	Anthracite coal ashes	130	5,329	108.5	5⅛	57.3	131.2

TABLE IV.

		Diameter of covering in Millim.	Weight per linear meter in grams.	Kilo-Cent. heat units 1 meter 1 h.	Diameter in inches.	Weight per foot in oz. Av.	Pound Fahr. heat units 1 foot, 1 hour.
1	**Hair felt, burlap**	137	1,990	42.1	5.37	21.4	51.0
2	" " "	114	1,228	42.6	4.5	13.2	51.6
5	Asbestos paper, hair felt, duck	114	1,609	49.5	4.5	17.3	59.7
7	" " " " paper, duck	114	1,851	51.7	4.5	19.9	62.5
8	" " " " " "	127	1,711	52.7	5	18.4	63.8
11	" " " " " "	102	1,600	58.4	4	17.2	70.6
15	" " " " " "	114	1,498	64.4	4.5	16.1	77.9
a	**Fossil meal and cork (12 p. c.); straw board**	130	2,297	53.6	5.12	24.7	64.8
c	**Fossil meal, straw board**	130	1,934	56.6	5.12	20.8	68.4
21	**Paste of fossil meal and hair**	121	5,647	69.4	4.75	60.7	83.9
26	" " " " "	108	2,967	75.3	4.25	31.9	91.0
27	" " " " "	105	2,502	77.0	4.12	26.9	93.1
36	" " " " asbestos	114	3,199	97.1	4.5	34.4	117.5
b	**Best slag wool, straw board**	121	1,228	55.1	4.75	13.2	66.6
24	**Poor " " " "**	108	2,241	74.8	4.25	24.1	90.5
35½	**Asbestos paper, slag wool, asbestos paper**	102	2,437	96.8	4	26.2	117.0
9	**Air space, tin plate, hair felt, duck**	120	1,934	56.2	4.75	20.8	67.9
g	" " straw board, cork, straw board	130	1,116	65.3	5.12	12.0	79.0
h	" " " " hair felt, drilling	130		65.3	5.12		79.0
i	" " " " " " "	130		65.8	5.12		79.5
19	" " asbestos paper, paper cylinder	121	2,725	67.1	4.75	29.3	81.1
20	" " straw board, hair felt	121	1,116	67.9	4.75	12.0	82.1
22	" " " " sphagnum, cotton cloth	121	986	71.0	4.75	10.6	85.8
m	" " " " pine turnings, straw board	130	1,302	74.1	5.12	14.0	89.6
n	" " " " paper pulp, " "	130	1,274	75.8	5.12	13.7	91.7
o	" " " " rice chaff, " "	130	1,590	78.1	5.12	17.1	94.6
p	" " " " cotton roving stair pad	121	1,357	79.3	4.75	14.6	96.0
34	" " wire netting, paste of fossil meal and asbestos	127	3,813	93.8	5	41.0	113.4
u	" " multiplex	111	2,613	95.4	4.37	28.1	115.3

v	Air space, straw board, cotton roving	108	772	102.6	4.25	8.3	124.0
47	" " simple	75		161.6	2.94		195.4
48	" " simple	121		165.4	4.75		200.0
d	Silicated cork chips, drilling (loose calorimeter)	133	1,376	59.2	5.25	14.8	71.4
e	" " " " (close calorimeter)	133	1,367	60.1	5.25	14.7	72.7
g	Air space, straw board, cork chips, straw board	130	1,116	65.3	5.12	12.0	79.0
s	Cork in strips	92	623	87.1	3.62	6.7	105.3
f	Silicated pine charcoal	133	3,208	64.7	5.25	34.5	78.3
29	" hard wood charcoal	127	3,896	80.9	5	41.9	97.8
18	" rice chaff, cotton cloth	121	2,111	66.3	4.75	22.7	89.2
k	" " " drilling	133	1,627	70.6	5.25	17.5	85.4
l	Rice chaff, straw board	130	1,554	71.9	5.12	16.7	87.0
o	Air space, straw board, rice chaff, straw board	130	1,590	78.1	5.12	17.1	94.5
32	Rice chaff, straw board	95	781	90.6	3.75	8.4	109.5
16	Paper cylinder—Reed's covering	111	2,837	64.5	4.37	30.5	78.1
j	Cotton batting	102	558	65.9	4	26.0	79.7
19	Air space, asbestos paper, paper cylinder—Reed's covering	121	2,725	67.1	4.75	29.3	81.1
28	Straw rope, quadruple cotton cloth	114	1,879	80.1	4.50	20.2	96.9
t	Silicated cotton-seed hulls, drilling	130	4,743	88.9	5.12	51.0	107.6
w	Blotting paper	102	2,688	105.6	4	28.9	127.8
x	Asbestos paper	102		108.5	4		131.2
50	" "	73		184.9	2.87		223.7
q	Anthracite coal flue ashes, straw board	127	3,497	80.1	5	37.6	96.8
v	Bituminous coal ashes, " "	127	3,832	81.3	5	41.2	98.4
y	Anthracite coal ashes, " "	130	5,329	108.5	5.12	57.3	131.2
21	Paste of fossil meal and hair, plastered on	121	5,647	69.4	4.75	60.7	83.9
26	" " " " " " " "	108	2,967	75.3	4.25	31.9	91.0
27	" " " " " " " "	105	2,502	77.0	4.12	26.9	93.1
34	Air space, paste of fossil meal and asbestos	127	3,813	93.8	5	41.0	113.4
36	Paste of fossil meal and asbestos	114	3,199	97.1	4.50	34.4	117.5
30	Carbon, plaster Paris, flour and hair, in paste, plastered on	114	3,069	88.3	4.50	33.0	106.8
42	Paste of clay and vegetable fibre plastered on	133	8,751	120.7	5.25	94.1	146.0
44	Paste of anthracite ashes, plaster of Paris, flour, and hair	121	7,366	128.5	4.75	79.2	155.4
49	Paste of clay and vegetable fibre	108	7,633	169.7	4.25	65.2	205.3

TABLE V.

		Thickness in Millim.	Weight in 1,000 c. c.	Net Specific Gravity.	Per cent. solid matter.	Kilo-Cent. heat units 1 sq. m. 1 h.	Lb. Fahr. heat units 1 sq. ft. 1 h.
1	Glazed cotton wadding	50	15.8 *g.*	1.55	1.0	129.1	47.6
2	Carded cotton	50	15.8	1.58	1.0	157.7	58.1
3	Glazed cotton wadding	40	19.7	1.55	1.3	193.4	71.3
4	Glazed cotton wadding	30	26.4	"	1.7	205.5	75.8
5	Wool wadding	25	73.1	1.31	5.6	219.8	81.0
6	Wool wadding	25	90.4	"	6.9	223.6	82.4
7	Wool wadding	25	126.8	"	9.7	237.4	87.5
8	Wool wadding	25	103.9	"	7.9	238.5	87.9
9	Wool wadding	25	118.0	"	9.0	246.2	90.8
10	French cotton wadding	25	65.7	1.58	4.1	247.7	91.3
11	Wool wadding	25	56.8	1.31	4.3	253.1	93.3
12	Cotton roving knit together	28	128.5	1.58	8.1	257.1	95.0
13	Live geese feathers	25	50.5	1.01	5.0	261.6	96.4
14	Lampblack, loose	25	95.0	1.70	5.6	266.5	98.3
15	Hair felt	25	242.7	1.31	18.5	277.4	102.3
16	Wool wadding	25	40.9	1.31	3.1	279.1	102.9
17	Carded cotton	25	31.5	1.58	2.0	281.4	103.7
18	Lampblack, compressed	25	415.0	1.70	24.4	286.5	105.6
19	Hair felt	25	121.3	1.31	9.2	292.8	107.9
20	French cotton batting	25	13.8	1.58	0.9	298.7	110.1
21	French cotton batting	25	30.5	1.58	1.9	299.5	110.4
22	Wool wadding	25	27.3	1.31	2.1	300.6	110.8
23	Carded cotton	25	16.1	1.58	1.0	310.4	114.4
24	Live geese feathers	25	20.0	1.01	2.0	320.9	118.4
25	Cork charcoal, ground fine	25	78.7	1.43	5.3	323.6	119.3
26	Glazed cotton wadding	20	39.5	1.55	2.5	326.4	120.3
27	Calcined magnesia, loose	25	76.0	3.26	2.3	335.2	123.6
28	Calcined magnesia, crowded	25	160.0	3.26	4.9	310.1	125.4
29	Cork charcoal, coarsely ground	25	45.0	1.43	3.1	343.4	126.5

30	Glazed cotton wadding	20	18.3	1.55	1.2	370.9	136.6
1	Carbonate of magnesia = "magnesia alba."	25	132.	2.21	6.0	370.9	136.6
32	Pine charcoal	"	163.	1.37	11.9	376.4	138.8
33	Carbonate of magnesia, crowded	"	207.	2.21	9.4	386.7	142.6
34	Washed fossil meal, loose	"	147.	2.44	6.0	393.4	145.1
35	Carbonate of magnesia, compressed	"	333.	2.21	15.0	416.5	153.6
36	Glazed cotton wadding	15	52.7	1.55	3.4	424.2	156.4
37	Washed fossil meal, crowded	25	273.	2.44	11.2	425.8	157.0
38	French zinc white, loose	25	483.	5.48	8.8	466.0	171.8
39	Glazed cotton wadding	10	79.1	1.55	5.1	502.4	185.2
40	Paris white = carbonate calcium	25	781.	3.09	25.3	559.6	206.3
41	Barium sulphate, fine flour	"	179.2	4.70	38.1	728.6	268.6
42	Coarse Omahalite	"	491.	2.62	18.7	777.5	286.6
43	Fine Omahalite	"	628.	2.54	24.7	823.1	303.5
44	Plaster Paris = anhydrous calcium sulphate	"	964.	2.62	36.8	839.2	309.4
45	Pumice stone, finely ground	"	873.	2.55	34.2	844.6	311.4
46	Extremely fine sand from Plymouth, Mass	"	991.	2.78	35.6	861.0	317.5
47	Anthracite coal, ground	"	827.	1.63	50.6	968.2	357.0
48	Calcined magnesia, compressed	"	931.	3.26	28.5	1,155.9	426.0
49	Zinc white, compressed	"	177.2	5.48	32.3	1,163.8	429.1
50	Air space	"	1.2	0.0	0.0	1,391.7	479.9
51	Fibrous asbestos	"	247.	3.05	8.1	1,328.6	489.9
52	Coarse sand	"	143.9	2.72	52.9	1,683.6	620.7
53	Fine sand	"	140.8	2.74	51.4	1,689.7	623.0
54	Plumbago	"	628.	2.40	26.1	1,922.5	708.8
55	Fine table salt = sodium chloride	"	103.2	2.15	48.0	1,982.7	731.0

TABLE VI.

		Thickness in Millimeters.	Weight in 1,000 c. c.	Net specific gravity.	Per cent. solid matter.	Kilo-Cent. heat units 1 sq. in. 1 h.	Pound-Fah. heat units 1 sq. ft. 1 h.
1	Glazed cotton wadding	50	15.8	1.55	1.0	129.1	47.6
3	" " "	40	19.7	1.55	1.3	193.4	71.3
4	" " "	30	26.4	1.55	1.7	205.5	75.8
26	" " "	20	39.5	1.55	2.5	326.4	120.3
30	" " "	20	18.3	1.55	1.2	370.9	136.6
36	" " "	15	52.7	1.55	3.4	424.2	156.4
39	" " "	10	79.1	1.55	5.1	502.4	185.2
2	Carded cotton	50	15.8	1.58	1.0	157.7	58.1
17	" "	25	31.5	1.58	2.0	281.4	103.7
23	" "	25	16.1	1.58	1.0	310.4	114.4
10	French cotton batting	25	65.7	1.58	4.1	247.7	91.3
20	" " "	25	13.8	1.58	0.9	298.7	110.1
21	" " "	25	30.5	1.58	1.9	299.5	110.4
12	Cotton roving, stair pad	28	128.5	1.58	8.1	257.1	95.0
5	Wool wadding	25	73.1	1.31	5.6	219.8	81.0
6	" "	25	90.4	1.31	6.9	223.6	82.4
7	" "	25	126.8	1.31	9.7	237.4	87.5
8	" "	25	103.9	1.31	7.9	238.5	87.9
9	" "	25	118.0	1.31	9.0	246.2	90.8
11	" "	25	56.8	1.31	4.3	253.1	93.3
16	" "	25	40.9	1.31	3.1	279.1	102.9
22	" "	25	27.3	1.31	2.1	300.6	110.8
13	Feathers	25	50.5	1.01	5.0	261.6	96.4
24	"	25	20.0	1.01	2.0	520.9	118.4
15	Hair felt	25	242.7	1.31	18.5	277.4	102.3
19	" "	25	121.3	1.31	9.2	292.8	107.9
50	Air alone	25	.....	...	0.0	1301.7	479.9
14	Lamp black, "Golden Star," loose	25	95.0	1.70	5.6	266.5	98.3
18	" " compressed	25	415.0	1.70	24.4	286.5	105.6
25	Cork charcoal, fine	25	78.7	1.43	5.3	323.6	119.3
29	" " coarse	25	45.0	1.43	3.1	343.4	126.5

32	Pine charcoal	25	163.0	1.37	11.9	376.4	138.8
47	Anthracite coal	25	827.0	1.63	50.6	968.2	357.0
54	Plumbago	25	628.0	2.40	26.1	1922.5	708.8
27	Calcined magnesia, loose	25	628.0	2.40	26.1	1922.5	708.8
28	" " crowded	25	76.0	3.26	2.3	335.2	123.6
48	" " compressed	25	160.0	3.26	4.9	340.1	125.4
31	Carbonate magnesia, loose	25	931.0	3.26	28.5	1155.9	426.0
33	" " crowded	25	132.0	2.21	6.0	370.9	136.6
35	" " compressed	25	207.0	2.21	9.4	386.7	142.6
34	Fossil meal, loose	25	333.0	2.21	15.0	416.5	153.6
37	" " crowded	25	147.0	2.44	6.0	393.4	145.1
46	Plymouth sand	25	273.0	2.44	11.2	425.8	157.0
52	Coarse sand	25	991.0	2.78	35.6	861.0	317.5
53	Fine "	25	1439.0	2.72	52.9	1683.6	620.7
42	Coarse Omahalite	25	1408.0	2.74	51.4	1689.7	623.0
43	Fine "	25	491.0	2.62	18.7	777.5	286.6
45	Pumice stone, finely ground	25	628.0	2.54	24.7	823.1	303.5
51	Asbestos	25	873.0	2.55	34.2	844.6	311.4
38	Oxide of zinc, loose	25	247.0	3.05	8.1	1328.6	489.9
49	" " compressed	25	483.0	5.48	8.8	466.0	171.8
40	Paris white—ground chalk	25	1772.0	5.48	32.3	1163.8	429.1
44	Plaster of Paris	25	781.0	3.09	25.3	559.6	206.3
41	Barium sulphate, flour	25	964	2.62	36.8	839.2	309.4
55	Common salt	25	1792.0	4.70	38.1	728.6	268.6
		25	1032	2.15	48.0	1982.7	731.0

TABLE VII.

		Per cent. Solid Matter.	Kilo-Cent. Heat Units.	
50	Air space	0.0	1302	*a*
20	French cotton	0.9	299	*b*
23	Carded cotton	1.0	310	*c*
21	French cotton	1.9	299	*d*
17	Carded cotton	2.0	281	*e*
24	Feathers	2.0	321	*f*
22	Wool	2.1	301	*g*
27	Calcined magnesia	2·3	335	*h*
16	Wool	3.1	279	*i*
29	Cork charcoal, coarse	3.1	343	*j*
10	French cotton	4.1	248	*k*
11	Wool	4.3	253	*l*
28	Calcined magnesia	4.9	340	*m*
13	Feathers	5.0	262	*n*
25	Cork charcoal, fine	5.3	324	*o*
5	Wool	5.6	220	*p*
14	Lampblack	5.6	266	*q*
31	Carbonate magnesia	6.0	371	*r*
34	Fossil meal	6.0	393	*s*
6	Wool	6.9	224	*t*
8	Wool	7.9	238	*u*
51	Asbestos	8.1	1329	*v*
38	Zinc white	8.8	466	*w*
9	Wool	9.0	246	*x*
19	Hair felt	9.2	293	*y*
33	Carbonate magnesia	9.4	387	*z*
7	Wool	9.7	237	*A*
37	Fossil meal	11.2	426	*B*
32	Pine charcoal	11.9	376	*C*
35	Carbonate magnesia	15.0	416	*D*
15	Hair felt	18.5	277	*E*
42	Omahalite, coarse	18.7	777	*F*
18	Lampblack	24.4	286	*G*
43	Omahalite, fine	24.7	823	*H*
40	Chalk	25.3	560	*I*
54	Plumbago	26.1	1922	*J*
48	Calcined magnesia	28.5	1156	*K*
49	Zinc white	32.3	1164	*L*
45	Pumice stone	34.2	845	*M*
46	Plymouth sand	35.6	861	*N*
44	Plaster Paris	36.8	839	*O*
41	Barium sulphate	38.1	729	*P*
55	Common salt	48.0	1983	*Q*
47	Anthracite coal	50.6	968	*R*
53	Fine sand	51.4	1690	*S*
52	Coarse sand	52.9	1684	*T*

CLXV.

STEAM BOILERS AS MAGAZINES OF EXPLOSIVE ENERGY.

BY ROBERT H. THURSTON, HOBOKEN, N. J.

SECTION I.—*Computation of Stored Energy.*

IN the following paper it is proposed to present the results of a series of calculations relating to the magnitude of the store of energy contained in masses of steam and of water, when heated to temperatures customarily met with in the various applications of the expansive power of steam, in the arts, and especially in steam boilers. This energy may be measured by the amount of work which may be obtained by the gradual reduction of the temperature of the mass to that due atmospheric pressure, by continuous expansion.

The subject is one which has often attracted the attention of both the man of science and the engineer. Its importance, both from the standpoint of pure science and from that of science applied in engineering and the minor arts, is such as would justify the expenditure of vastly more time and attention than has ever yet been given it. The first attempt to calculate the amount of energy latent in steam boilers, and capable of greater or less utilization in expansion by explosion, was made by Mr. George Biddle Airy,* the Astronomer Royal of Great Britain, in the year 1863, and by the late Professor Rankine† at about the same time. Mr. Airy and Professor Rankine published papers on this subject in the same number of the Philosophical Magazine (Nov., 1863), the one dated the 3d of September and the other the 5th October of that year. The former had already presented an abstract of his work at the meeting of the British Association of that year.

In the first of these papers, it is remarked that "very little of the destructive effect of an explosion is due to the steam which is confined in the steam-chamber at the moment of the explosion. The rupture of the boiler is due to the expansive power common at the moment to the steam and

* "Numerical Expression of the Destructive Energy in the Explosions of Steam Boilers."

† "On the Expansive Energy of Heated Water."

the water, both at a temperature higher than the boiling point; but as soon as the steam escapes, and thereby diminishes the compressive force upon the water, a new issue of steam takes place from the water, reducing its temperature; when this escapes, and further diminishes the compressive force, another issue of steam of lower elastic force from the water takes place, again reducing its temperature; and so on, till at length the temperature of the water is reduced to the atmospheric boiling point, and the pressure of the steam (or rather the excess of steam-pressure over atmospheric pressure) is reduced to 0." Thus it is shown that it is the enormous quantity of steam so produced from the water, during this continuous but exceedingly rapid operation, that produces the destructive effect of steam-boiler explosions. The action of the steam which may happen to be present in the steam-space at the instant of rupture is considered unimportant.

Mr. Airy had, as early as 1849, endeavored to determine the magnitude of the effect thus capable of being produced, but had been unable to do so in consequence of deficiency of data. His determinations, as published finally, were made at his request by Professor W. H. Miller. The data used are the results of the experiments of Regnault and of Fairbairn and Tate, on the relations of pressure, volume and temperature of steam, and of an experiment by Mr. George Biddle, by which it was found that a locomotive boiler, at four atmospheres pressure, discharged one-eighth of its liquid contents by the process of continuous vaporization above outlined, when, the fire being removed, the pressure was reduced to that of the atmosphere. The process of calculation assumes the steam so formed to be applied to do work expanding down to the boiling point, in the operation. The work so done is compared with that of exploding gunpowder, and the conclusion finally reached is that "the destructive energy of one cubic foot of water, at a temperature which produces the pressure of 60 lbs. to the square inch, is equal to that of one pound of gunpowder."

The work of Rankine is more exact and more complete, as well as of greater practical utility. The method adopted is that to be described presently, and involves the application of the formulas for the transformation of heat into work which had been ten years earlier derived by Rankine and by Clausius, independently. This paper would seem to have been brought out by the suggestion made by Airy at the meeting of the British Association. Rankine shows that the energy developed during this, which is an adiabatic method

of expansion, depends solely upon the specific heat and the temperatures at the beginning and the end of the expansion, and has no dependence, in any manner, upon any other physical properties of the liquid. He then shows how the quantity of energy latent in heated water may be calculated, and gives, in illustration, the amount so determined for eight temperatures exceeding the boiling point. Approximate empirical expressions are given for the calculation of the energy and of the ultimate volumes assumed during expansion, as follows, in British and in Metric measures:

$$U = \frac{772\,(T - 212)^2}{T + 1134.4}; \qquad U_m = \frac{423.55\,(T - 100)^2}{T + 648};$$

$$V = \frac{36.76\,(T - 212)}{T + 1134.4}; \qquad V_m = \frac{2.29\,(T - 100)}{T + 648}.$$

These formulas give the energy in foot-pounds and kilogrammeters, and the volumes in cubic feet and cubic meters. They may be used for temperatures not found in the tables to be given, but, in view of the completeness of the latter, it will probably be seldom necessary for the engineer to resort to them.

This subject attracted the attention of the writer at a very early date. Familiarity, from early boyhood, with the destructive effects of steam boiler explosions, the singular mystery that has been supposed to surround their causes, the frequent calls made upon him, in the course of his professional practice and of his studies, to examine the subject and to give advice in matters relating to the use of steam, and many other hardly less controlling circumstances, invested this matter with an extraordinary interest. Probably no subject, within the whole range of the practice of the engineer has demanded or has received more attention than this; and probably no such subject is to-day less satisfactorily developed in theory and less thoroughly investigated experimentally than this. It is one which the writer has endeavored, at several different periods in the course of his work, to take up and reduce if possible to a consistent theoretical and practically applicable form. On each occasion, however, his labors were interrupted before they were fairly begun.

In the year 1872 the writer received from the Secretary of the Treasury of the United States a communication in which he was requested to prepare, for the use of the Treasury Department, a report on the causes and the conditions leading to the explosions of steam boilers, and he began the preparation of such a report, in which he proposed to incorporate the facts to be here presented.

In the year 1875, the writer, then a member of a commission formed by the government to investigate the subject, was asked by the Cabinet officer having direction of the matter to accept the chairmanship of the commission and to give his time to the subject under investigation. For sufficient reasons he was unwilling to undertake the work, and an older and wiser head was appointed, at his request. A little later, ill health compelled him to resign from the commission; but his brief connection with the board led them to the further study of the subject of this paper; the investigation was, however, again interrupted, and has not since been taken up in the systematic manner then proposed.

In this paper, it is proposed to limit the subject to the investigation of the quantity of energy stored in some of the familiar and commonly used forms of steam boilers which are now everywhere seen endangering, to a greater or less extent, the lives and property of all who may be either permanently or temporarily within range of them.

A steam boiler is a vessel in which is confined a mass of water, and of steam, at a high temperature, and at a pressure greatly in excess of that of the surrounding atmosphere. The sudden expansion of this mass from its initial pressure down to that of the external air, occurring against the resistance of its "shell" or other masses of matter, may develop a very great amount of work by the transformation of its heat into mechanical energy, and may cause, as daily occurring accidents remind us, an enormous destruction of life and property. The inclosed fluid consists, in most cases, of a small weight of steam and a great weight of water. In a boiler of a once common and still not uncommon marine type, the writer found the weight of steam to be less than 250 pounds, while the weight of water was nearly 40,000 pounds. As will be seen later, under such conditions the quantity of energy stored in the water is vastly in excess of that contained in the steam, notwithstanding the fact that the amount of energy per unit of weight of fluid is enormously the greater in the steam. A pound of steam, at a pressure of six atmospheres (88.2 pounds per square inch), above zero of pressure, and at its normal temperature, 177 C. (319° F.), has stored in it about 75 British Thermal Units, or nearly 600,000 foot-pounds of mechanical energy per unit of weight, in excess of that which it contains after expansion to atmospheric pressure. A pound of water accompanying that steam, and at the same pressure, has stored within it but about one-tenth as much

available energy. Nevertheless, the disproportion of weight of two fluids is so much greater as to make the quantity of energy stored in the steam contained in the boiler quite insignificant in comparison with that contained in the water. These facts will be fully illustrated by the figures to be hereafter presented.

The quantity of work and of energy which may be liberated by the explosion, or utilized by the expansion, of a mass of mingled steam and water has been shown by Rankine and by Clausius, who determined this quantity almost simultaneously, to be easily expressed in terms of the two temperatures between which the expansion takes place.

When a mass of steam, originally dry, but saturated, so expands from an initial absolute temperature, T_1, to a final absolute temperature, T_2, if J is the mechanical equivalent of the unit of heat, and H is the measure, in the same units, of the latent heat per unit of weight of steam, the total quantity of energy exerted against the piston of a non-condensing engine, by unity of weight of the expanding mass is, as a maximum,

$$U = JT_2 \left(\frac{T_1}{T_2} - 1 - \text{hyp. log.} \frac{T_1}{T_2} \right) + \frac{T_1 - T_2}{T_1} H \ldots (A).$$

This equation was published by Rankine a generation ago.*

When a mingled mass of steam and water similarly expands, if M represents the weight of the total mass and m is the weight of steam alone, the work done by such expansion will be measured by the expression,

$$U = M J T_2 \left(\frac{T_1}{T_2} - 1 - \text{hyp. log.} \frac{T_1}{T_2} \right) + m \frac{T_1 - T_2}{T_2} H \ldots (B)$$

This equation was published by Clausius in substantially this form.†

It is evident that the latent heat of the quantity m, which is represented by mH, becomes zero when the mass consists solely of water, and that the first term of the second member of the equation measures the amount of energy of heated water which may be set free, or converted into mechanical energy by explosion. The available energy of heated water, when explosion occurs, is thus easily measurable.

As has already been stated, this method was first applied by Rankine to the determination of the available energy of heated

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* Steam Engine and Prime Movers, p. 387.

† Mechanical Theory of Heat, Browne's Translation, p. 283.

water for several selected temperatures and pressures. It has long been the intention of the writer to ascertain the magnitude of the quantities of energy residing, in available form, in both steam and water, for the whole usual range of temperatures and pressures familiar to the engineer, and also to carry out the calculations for temperatures and pressures not yet attained, except experimentally, but which are likely to be reached in the course of time, as the constantly progressing increase now observable goes on. The maximum attainable, in the effort to increase the efficiency of the steam engine and in the application of steam to new purposes, cannot be to-day predicted, or even, so far as the writer can see, imagined. High pressures like those adopted by Perkins and by Alban may yet be found useful. It was therefore proposed to carry out the tables to be constructed far beyond the limit of present necessities.

It was further proposed to ascertain the weights of steam and of water contained in each of the more common forms of steam boilers, and to determine the total and relative amounts of energy confined in each under the usual conditions of working in every-day practice, and thus to ascertain their relative destructive power in case of explosion. This part of the work is reserved for description in a succeeding section of this paper. The present section is devoted to the first part of the subject.

At the commencement of this work, the writer employed the late Mr. W. G. Cartwright, M. E., as computer, and, with his aid, prepared tables extending from 50 pounds per square inch to 100, at intervals of ten pounds, up to 250 with intervals of 25 pounds, then 300, and up to 1000 pounds per square inch by 100 pounds, and with larger intervals up to 10,000 and 20,000 pounds. The available energy of the heated water was computed, the energy obtainable from the so-called "latent heat," and their sum, *i.e.*, the available energy of steam per unit of weight. In the course of this work, each figure was calculated independently by two computers, and thus checked. As a further check, the figures so obtained were plotted, and the curve representing the law of their variation was drawn. This was a smooth curve of moderate curvature and an incorrect determination was plainly revealed, and easily detected, by falling outside the curve. Three curves were thus constructed which will be given later: (1) the Curve of Available Energy of Heated Water; (2) the Curve of Available Energy of Latent Heat; (3) the Curve of Available Energy of Steam. The second of these curves presents an interesting peculiarity which

will be pointed out when studying the forms of the several curves and the tables of results.

The work was interrupted by more pressing duties, and was finally resumed in the spring of 1884 and completed in the form now presented. The computers of the more complete tables here given were Messrs. Ernest H. Foster, M.E., and Kenneth Torrance, M.E., who, pursuing the same method as was originally adopted for the earlier computations, have revised the whole work, recalculating every figure, extending the tables by interpolation, and carrying them up to a still higher pressure than was originally proposed. The tables here presented range from 20 pounds per square inch, (1.4 kgs. per sq. cm.) up to 100,000 pounds per square inch (7,030.83 kgs. per sq. cm.) the maximum probably falling far beyond the range of possible application, its temperature exceeding that at which the metals retain their tenacity, and, in some cases, exceeding their melting points. These high figures are not to be taken as exact. The relation of temperature to pressure is obtained by the use of Rankine's equation, of which it can only be said that it is wonderfully exact throughout the range of pressures within which experiment has extended, and within which it can be verified. The values estimated and tabulated are probably quite exact enough for the present purposes of even the military engineer and ordnance officer. The form of the equation, and of the curve representing the law of variation of pressure with temperature, indicates that, if exact at the familiar pressures and temperatures, it is not likely to be inexact at higher pressures. The curve, at its upper extremity, becomes nearly rectilinear.

The table which follows presents the values of the pressures in pounds per square inch above a vacuum, the corresponding reading of the steam gauge (allowing a barometric pressure of 14.7 pounds per square inch), the same pressures reckoned in atmospheres, the corresponding temperatures as given by the Centigrade and the Fahrenheit thermometers, and as reckoned both from the usual and the absolute zeros. The amount of the explosive energy of a unit weight of water, of the latent heat in a unit weight of steam, and the total available heat energy of the steam, are given for each of the stated temperatures and pressures throughout the whole range in British measures, atmospheric pressures being assumed to limit expansion. The values of the latent heats are taken from Regnault, for moderate pressures, and are calculated for the higher pressures, beyond the range of experiment, by the use of Rankine's modification of Regnault's formula.

TABLE I.

Total Available Energy in Water and Steam.

Pressure above a vacuum in pounds per square inch.	Same pressure as indicated by steam gauge, allowing 14.7 lbs. for atmospheric pressure.	Absolute pressure in atmospheres.	Number of British Thermal units required for the evaporation of one pound of water, known as latent heat of evaporation, H.	Temperature in degrees Fahrenheit of the steam and of the water from which it is evaporated.	Temperature in degrees Centigrade of the steam and of the water from which it is evaporated.	Corresponding absolute temperature in degrees Fahrenheit.	Corresponding absolute temperature in degrees Centigrade.	Amount of energy contained in one pound of water which may be liberated by explosion or expansion to 212° Fahr.	Corresponding amount of energy contained in the latent heat of evaporation.	Total amount of energy contained in one pound of steam at corresponding temperatures and pressures.
20	5.3	1.36	954.415	227.9	108.8	689.0	382.8	145.9	16872.9	17018.8
25	10.3	1.70	945.825	240.0	115.5	701.2	389.5	439.7	29156.8	29596.5
30	15.3	2.04	938.925	250.2	121.2	711.4	395.2	813.5	38921.9	39735.4
35	20.3	2.38	932.1523	259.1	126.1	720.3	400.1	1223.4	47054.9	48278.3
40	25.3	2.72	926.4728	267.1	130.1	728.3	404 6	1645.7	54111.7	55757.4
45	30.3	3.06	921.3343	274.2	134.5	735.4	408.5	2112.9	60158.1	62271.0
50	35.3	3.40	916.6316	280.8	138.2	742.0	412.2	2550.4	65613.8	68164.2
55	40.3	3.74	912.2906	286.8	141.5	748.0	415.5	2999.9	70428.7	73428.6
60	45.3	4.08	908.2472	292.5	144.7	753.7	418.7	3449.2	74884.6	78333.8
65	50.3	4.42	904.4621	297.7	147.6	758.9	421.6	3899.8	78850.5	82750.3
70	55.3	4.76	900.8991	302.7	150.4	763.9	424.4	4361.1	82577.7	86938.8
75	60.3	5.10	897.5269	307 3	152.9	768.5	426.9	4815.8	85923.6	90739.4
80	65.3	5.44	894.3304	311.8	155 4	773.0	429.4	5206.5	89138.7	94345.2
85	70.3	5.78	891.2862	316.0	157.7	777.2	431.7	5638.9	92073.3	97712.2
80	75.3	6.12	888.3758	320.0	160.0	781.2	434.0	6058.1	94814.7	100872.8
95	80.3	6.46	885.5887	323.8	162.1	785.0	436.1	6474.2	97447.2	103921.4
100	85.3	6.80	882.9144	327.5	164.1	788.7	438.1	6885.2	99787.6	106672.8
105	90.3	7.14	880.3429	331.1	166.1	792.3	440.1	7290.3	102163.3	109453.6
110	95.3	7.48	877.8653	334.5	168.0	795.7	442.0	7689.0	104334.9	112023.9
115	100.3	7.82	875.4721	337.8	169.8	799.0	443.8	8087.3	106421.7	114509.0
120	105.3	8.16	873.1555	340.9	171.6	802.1	445.6	8483.1	108325.4	116808.5
125	110.3	8.50	870.9115	344.0	173.3	805.2	447.3	8864.9	110219.9	119084.8
130	115.3	8.84	868.7351	347.0	175.0	808.2	449.0	9252.6	112025.6	121278.2

135	120.3	9.18	866.6223	349.9	176.6	811.1	450.6	9627.0	113745.7	123372.7
140	125.3	9.52	864.5661	352.7	178.1	813.9	452.1	9992.6	115382.1	125374.7
145	130.3	9.86	862.5679	355.5	179.7	816.7	453.7	10361.0	117003.5	127364.5
150	135.3	10.20	860.6213	358.1	181.1	819.3	455.1	10536.5	118477.2	129003.7
155	140.3	10.54	858.7276	360.7	182.6	821.9	456.6	11085.9	119939.4	131025.3
160	145.3	10.88	856.8740	363.2	184.0	824.4	458.0	11444.2	121323.6	132767.8
165	150.3	11.22	855.0654	365.7	185.4	826.9	459.4	11823.4	122697.8	134521.2
170	155.3	11.56	853.2942	368.1	186.7	829.3	460.7	12141.3	123995.5	136136.8
175	160.3	11.90	851.5670	370.5	188.0	831.7	462.0	12508.7	125284.7	137793.4
180	165.3	12.24	849.8698	372.8	189.3	834.0	463.3	12821.4	126499.1	139320.5
185	170.3	12.58	848.2086	375.0	190.5	836.2	464.5	13182.0	127642.4	140824.4
190	175.3	12.92	846.5844	377.2	191.7	838.4	465.7	13367.1	128778.8	142145.9
195	180.3	13.26	844.9938	379.4	193.0	840.6	467.0	13844.1	129908.3	143752.4
200	185.3	13.60	843.4326	381.5	194.1	842.7	468.1	14153.3	130967.4	145120.7
210	195.3	14.28	840.3967	385.6	196.4	846.8	470.4	14830.8	133003.2	147834.0
220	205.3	14.66	838.5864	389.8	198.7	851.0	472.7	15463.1	136003.3	151466.4
230	215.3	15.64	833.9691	394.2	201.2	855.4	475.2	16180.3	137134.2	153314.7
240	225.3	16.32	832.6419	397.9	203.3	859.1	477.3	16790.2	139094.3	155884.5
250	235.3	17.00	830.3630	401.0	205.0	862.2	479.0	17314.4	140516.0	157830.4
500	485.3	34.01	780.8592	467.6	242.0	898.8	516.0	30055.5	165892.7	195948.2
1000	985.3	68.02	720.4350	546.8	286.0	1008.0	560.0	48671.5	179212.0	227883.5
2000	1985.3	136.05	643.9049	643.7	339.8	1104.9	613.8	75777.2	194221.3	269998.5
3000	2985.3	204.08	590.8638	708.3	375.7	1169.5	649.7	96116.3	192555.0	289671.3
4000	3985.3	272.10	544.6774	763.7	405.9	1223.9	679.9	114498.6	189201.7	303700.3
5000	4985.3	340.13	505.7339	807.8	431.0	1269.0	705.1	130494.2	183305.9	313800.1
6000	5985.3	408.16	471.8473	845.9	452.1	1307.1	726.1	144413.4	176656.9	321070.3
7000	6985.3	486.19	439.9985	881.2	471.7	1342.4	745.7	157914.2	169333.3	327277.5
8000	7985.3	544.21	409.4533	914.3	490.1	1375.5	764.1	170832.0	161392.6	332224.6
9000	8985.3	612.24	382.6347	945.2	507.3	1406.4	781.5	183998.6	153998.0	337142.6
10000	9985.3	680.27	355.2491	971.4	521.8	1432.6	795.8	193787.0	145376.8	339164.2
100000	99985.3	6802.72	305.3040	2163.4	1184.1	2624.6	1458.1			

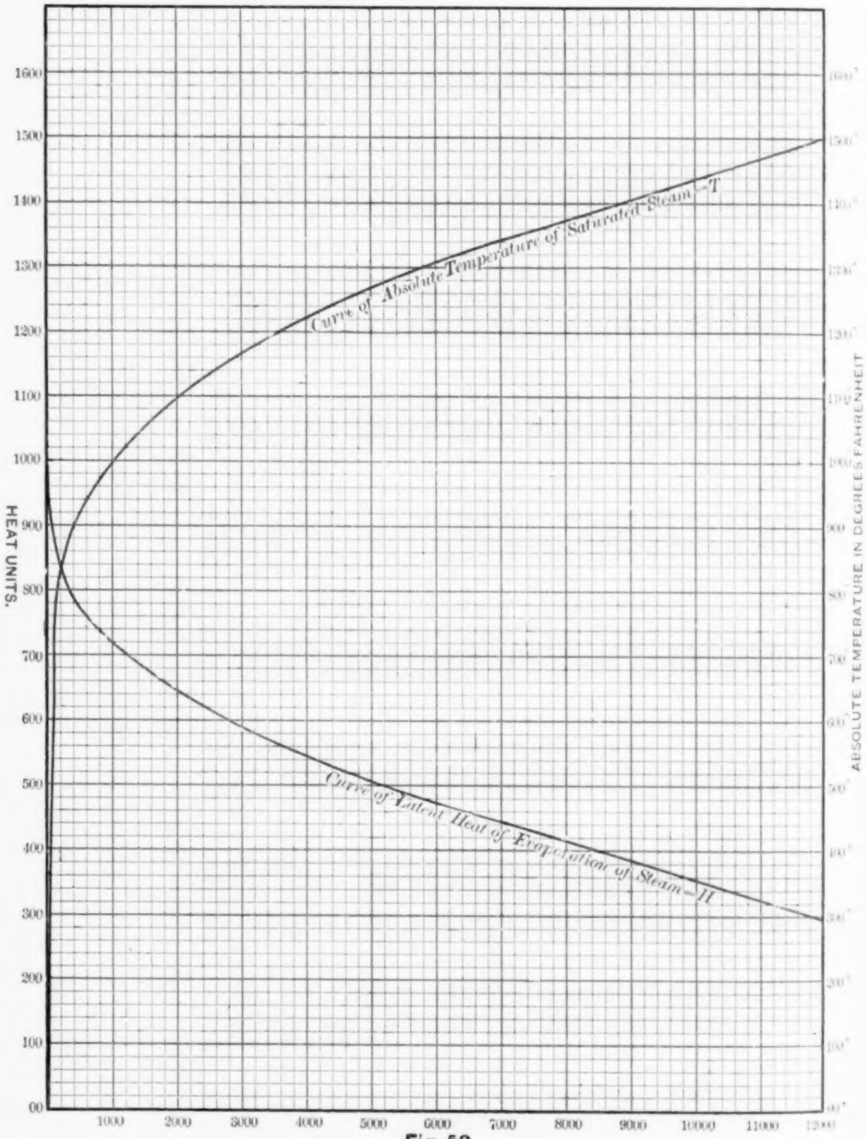


Fig. 58

Studying the table, the most remarkable fact noted at the lower pressures is the enormous difference in the amounts of energy, in available form, contained in the water and in the steam, and between the energy of sensible heat and that of latent heat, the sum of which constitutes the total energy of the steam. At 20

pounds per square inch above zero (1.36 atmos.), the water contains but 145.9 foot-pounds per pound; while the latent heat is equivalent to 16,872.9 foot-pounds, or more than 115 times as much; *i. e.*, the steam yields 116 times as much energy in the form of latent heat per pound, as does the water from which it is formed, at the same temperature. The temperature is low; but the amount of energy expended in the production of the molecular change resulting in the conversion of the water into steam is very great, in consequence of the enormous expansion then taking place. At 50 pounds, the ratio is 20 to 1; at 100 pounds per square inch, it is 14 to 1, at 500 it is 5 to 1; while at 5,000 pounds the energy of latent heat is but 1.4 that of the sensible heat. The two quantities become equal at about 7,500 pounds. At the highest temperature and pressure tabled, the same law would make the latent heat negative; it is of course uncertain what is the fact at that point.

At 50 pounds per square inch the energy of heated water is 2550.4 foot-pounds, while that of the steam is 68,184, or enough to raise its own weight to a height in each case of a half mile or of 12 miles. At 75 pounds the figures are 4,816 and 90,739, or equivalent to the work demanded to raise the unit weight to a height of four-fifths, and of about 17 miles, respectively. At 100 pounds the heights are over one mile for the water, and above 20 miles for the steam. The latent heat is not, however, all effective.

Plotting the tabulated figures and determining the form of the curve representing the law of variation of each set, we obtain the peculiar set of diagrams exhibited in the accompanying engraving. In Figure 58 are seen the curves of absolute temperature and of latent heat as varying with variation of pressure. They are smooth and beautifully formed lines, having no relation to any of the familiar curves of the text-books on co-ordinate geometry. In Figure 59 are given the curves of available energy of the water of latent heat, and of steam. The first and third have evident kinship with the two curves given in the preceding illustration; but the curve of energy of latent heat is of an entirely different kind, and is not only peculiar in its variation in radius of curvature, but also in the fact of presenting a maximum ordinate at an early point in its course. This maximum is found at a pressure of about one ton per square inch, a pressure easily attainable by the engineer.

Examining the equations of those curves it is seen that they have no relation to the conic sections, and that the curve, the peculiarities of which are here noted, is symmetrical about one of its abscissas, and that it must have, if the expression holds for such press-

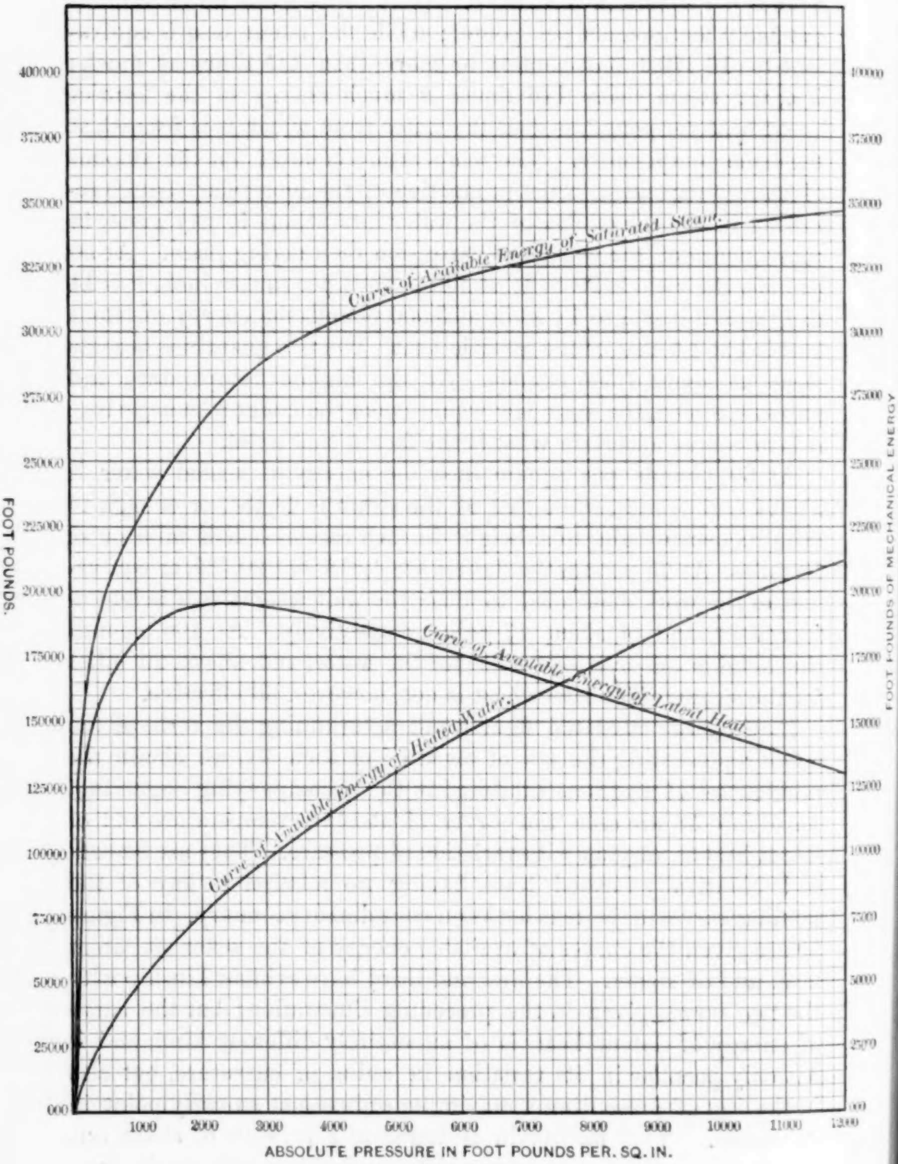


FIG. 59.

ures, another point of contrary flexure at some enormously high pressure and temperature. The formula is not, however, a "rational" one, and it is by no means certain that the curve is of the character indicated; although it is exceedingly probable that it may

be. The presence of this characteristic point, should experiment finally confirm the deduction here made, will be likely to prove interesting, and it may be important; its discovery may possibly prove to be useful.

The curve of energy of steam is simply the curve obtained by the superposition of one of the two preceding curves upon the other. It rises rapidly at first, with increase of temperature, then gradually rises more slowly, turning gracefully to the right, and finally becoming nearly rectilinear. The curve of available energy of heated water exhibits similar characteristics; but its curvature is more gradual and more uniform.

Comparing the energy of water and of steam in the steam boiler with that of gunpowder, as used in ordnance, it will be found that at high pressures the former become possible rivals of the latter. The energy of gunpowder is somewhat variable with composition and perfection of manufacture, and is very variable in actual use, in consequence of the losses in ordnance due to leakage, failure of combustion, or retarded combustion in the gun. Taking its value at what the writer would consider a fair figure, 250,000 foot-pounds per pound, it is seen that, as found by Airy, a cubic foot of heated water, under a pressure of 60 or 70 pounds per square inch, has about the same energy as one pound of gunpowder. The gunpowder exploded has energy sufficient to raise its own weight to a height of nearly 50 miles; while the water has enough to raise its weight about one-sixtieth that height. At a low red heat water has about 40 times this latter amount of energy in a form to be so expended. One pound of steam, at 60 pounds pressure, yields about one-third the energy of a pound of gunpowder. At 100 pounds it has as much energy as two-fifths of a pound of powder, and at higher pressures its energy increases very slowly.

SECTION II.—*Explosive Energy of Boilers.*

In illustration of the results of application of the computations which have been given in the preceding section of this paper, and for the purpose of obtaining some idea of the amount of destructive energy stored in steam-boilers of familiar forms, such as the engineer is constantly called upon to deal with, and such as the public are continually endangered by, Table II. has been calculated. This table is made up, with the assistance of Professor C. A. Carr, from notes of dimensions of boilers designed, or managed, at various times, by the writer, or in other ways having special interest to him. They include nearly all of the forms in common use, and are representative of familiar and ordinary practice.

TABLE II.

TOTAL STORED * ENERGY OF STEAM BOILERS.

TYPE.	AREA OF		PRESSURE. LBS. PER SQ. IN.	RATED POW'R. H. P.	WEIGHT OF			STORED ENERGY IN (AVAILABLE)			ENERGY PER LB. OF		MAX. HEIGHT OF PROJECTION.		INITIAL VELOC'Y.	
	G. S.	H. S.			BOILER.	WATER.	STEAM.	WATER.	STEAM.	TOTAL.	BOILER.	TOT. W'T	BOILER.	TOTAL.	BOILER.	TOTAL.
	SQ. FEET.				LBS.			FOOT LBS.			FOOT LBS.		FEET		FEET PER SEC.	
1 Plain Cylinder	15	120	100	10	2500	5764	11.325	46,605,200	676,698	47,281,898	18913	5714	18913	5714	1103	606
2 Cornish	36	730	30	60	16950	27471	31.45	57,570,750	709,310	58,260,060	3431	1314	3431	1314	471	290
3 Two Flue Cylinder	20	400	150	35	6775	6840	37.04	80,572,050	2,377,357	82,949,407	12243	6076	12243	6076	888	625
4 Plain Tubular	30	851.97	75	60	9500	8255	20.84	50,008,790	1,022,731	51,031,521	5372	2871	5372	2871	588	430
5 Locomotive	22	1070	125	525	19400	5260	21.67	52,561.075	1,483,896	54,044,971	2786	2189	2786	2189	423	375
6 "	30	1350	125	650	25000	6920	31.19	69,148,790	2,135,802	71,284,592	2851	2231	2851	2231	428	379
7 "	20	1200	125	600	20565	6450	25.65	64,452,270	1,766,447	66,218,717	3219	2448	3219	2448	455	397
8 "	15	875	125	425	14020	6330	19.02	64,253,160	1,302.431	65,555,591	4677	3213	4677	3213	549	455
9 Scotch Marine	32	768	75	300	27045	11765	29.8	71,272,370	1,462,430	72,734,800	2689	1873	2689	1873	416	348
10 "	50.5	1119.5	75	350	37972	17730	47.2	107,408,340	2,316,392	109,724,732	2889	1968	2889	1968	431	356
11 Flue and Return Tubular.	72.5	2324	30	200	56000	42845	69.81	90,531,490	1,570,517	92,101,987	1644	931	1644	931	325	245
12 "	72	1755	30	180	56000	48570	73.07	102,628,410	1,643,854	104,272,264	1862	996	1862	996	346	253
13 Water Tube	70	2806	100	250	34450	21325	35.31	172,455,270	2,108,110	174,563,380	5067	3073	5067	3073	571	445
14 "	100	3000	100	250	45000	28115	58.5	227,366,000	3,513,830	230,879,830	5130	3155	5130	3155	575	450
15 "	100	3000	100	250	54000	13410	23.64	108,346,670	1,311,377	109,624,283	2030	1626	2030	1626	361	323

* This "stored" energy is less than that available in the non-condensing engine by the amount of the latent heat of external work $(p_1 - p_2)\,v$.

No. 1 is the common, simple, plain cylindrical boiler. It is often adopted when the cheapness of fuel or the impurity of the water-supply renders it unadvisable to use the more complex, though more efficient, kinds. It is the cheapest and simplest in form of all the boilers. The boiler here taken was designed by the writer many years ago for a mill so situated as to make this the best form for adoption, and for the reasons above given. It is thirty inches in diameter, thirty feet long, and is rated at ten H. P., although such a boiler is often forced up to double that capacity. The boiler weighs a little over a ton, and contains more than twice its weight of water. The water, at a temperature corresponding to that of steam at 100 pounds pressure per square inch, contains over 46,600,000 foot-pounds of available explosive energy, while the steam, which has but one-fifth of one per cent. of the weight of the water, stores about 700,000 foot-pounds, giving a total of 47,000,000 foot-pounds, nearly, or sufficient to raise one pound nearly 10,000 miles. This is sufficient to throw the boiler 19,000 feet high, or nearly four miles, and with an initial velocity of projection of 1,100 feet per second.

Comparing this with the succeeding cases, it is seen that this is the most destructive form of boiler on the whole list. Its simplicity and its strength of form make it an exceedingly safe boiler, so long as it is kept in good order and properly managed; but, if through phenomenal ignorance or recklessness on the part of proprietor or attendant, the boiler is exploded, the consequences are usually exceptionally disastrous.

The explosion of a boiler of this form and of the proportions here given, in the year 1843, in the establishment of Messrs. R. L. Thurston & Co., at Providence, R. I., through mismanagement, is well remembered by the writer. The boiler-house was entirely destroyed, the main building seriously damaged, and a large expense was incurred in the purchase of new tools to replace those destroyed. No lives were lost, as the explosion fortunately occurred after the workmen had left the building. A similar explosion of a boiler of this size occurred some years later, within sight of the writer, which drove one end of the exploding boiler through a 16-inch wall, and several hundred feet through the air, cutting off an elm tree high above the ground, where it measured 9 inches in diameter, partly destroying a house in its further flight, and fell in the street beyond, where it was found *red hot* immediately after striking the earth. Long after the writer reached the spot, al-

though a heavy rain was falling, it was too hot to be touched, and was finally, nearly two hours later, cooled off by a stream of water from a hose, in order that it might be moved and inspected. It had been overheated, in consequence of low water, and cold feed had then been turned into it. The boiler was in very good order, but four years old, and was considered safe for 110 pounds. The engineer was seriously injured, and a pedestrian passing at the instant of the explosion was buried in the ruins of the falling walls and killed. The energy of this explosion was very much less than that stored in the boiler when in regular work.

No. 2 was a "Cornish" boiler designed by the writer, about 1860, and set to be fired under the shell. It was 6 feet by 36, and contained a 36-inch flue. The shell and flue were both of iron $\frac{3}{8}$-inch in thickness. The boiler was tested up to 60 pounds, at which pressure the flue showed some indications of alteration of form. It was strengthened by stay-rings, and the boiler was worked at 30 pounds. The boiler contained about 12 tons of water, weighed itself $7\frac{1}{4}$ tons, and the volume of steam in its steam space weighed but $31\frac{1}{2}$ pounds. The stored available energies were about 57,600,000 foot-pounds, and about 700,000 of foot-pounds in the water and steam, respectively, a total of nearly 60,000,000. This was sufficient to throw the boiler to the height of 3,400 feet, or over three-fifths of a mile.

Comparing this with the preceding, it is seen that the introduction of the single flue, of half the diameter of the boiler, and the reduced pressure, have reduced the relative destructive power to but little more than one-sixth that of the preceding form.

No. 3 is a "two-flue" or Lancashire boiler, similar in form and in proportions to many in use on the steamboats plying on our Western rivers, and which have acquired a very unenviable reputation by their occasional display of energy when carelessly handled. That here taken in illustration was designed by the writer, 42 inches in diameter, with two 14-inch flues of $\frac{3}{8}$ iron, and is here taken as working at a pressure, as permitted by law, of 150 pounds per square inch. It is rated at 35 horse-power, but such a boiler is often driven far above this figure. The boiler contains about its own weight, 3 tons, of water, and but 37 pounds of steam. The stored available energy is 83,000,000 foot-pounds, of which the steam contains but a little above five per cent. Its explosion would uncage sufficient energy to throw the boiler nearly $2\frac{1}{4}$ miles high, with an initial velocity of 900 feet per second. Both this boiler

and the plain cylinder are thus seen to have a projectile effect only to be compared to that of ordnance.

A boiler of this class, which the writer was called upon to inspect after explosion, had formed one of a "battery" of ten or twelve, and was set next the outside boiler of the lot. Its explosion threw the latter entirely out of the boiler-house into an adjoining yard, displaced the boiler on the opposite side, and demolished the boiler-house completely. The exploding boiler was torn into many pieces. The shell was torn into a helical ribbon, which was unwound from end to end. The furnace end of the boiler flew across the space in front of its house, tore down the side of a "kier-house," and demolished the kiers, nearly killing the kier-house attendant, who was standing between two kiers. The opposite end of the boiler was thrown through the air, describing a trajectory having an altitude of fifty feet, and a range of several hundred, doing much damage to property *en route*, finally landing in a neighboring field. The furnace front was found by the writer on the top of a hill, a quarter of a mile, nearly, from the boiler-house. The fireman, who was on the top of the boiler at the instant of the explosion, endeavoring to open a steam connection to relieve the boiler, then containing an excess of steam and a deficiency of water, was thrown over the roof of the mill, and his body was picked up in the field on the other side, and carried away in a packing-box measuring about two feet on each side. Cause:—low water and consequent overheating, and the introduction of feed before hauling fires and cooling down. The energy expended was much less than that calculated as above.

No. 4 is the common plain tubular boiler, substantially as designed by the writer at about the same time with those already described, and of the same dimensions as that adopted as a standard by the Hartford Steam Boiler Insurance Co.* It is a favorite form of boiler, and deservedly so, in the opinion of the writer, with all makers and users of shell boilers. That here taken is 60 inches in diameter, containing 66 3-inch tubes, and is 15 feet long. The general testimony of the best designers of this type, so far as the writer has been able to obtain definite opinions, as well as the observation and the experience of the writer himself, indicate that these proportions are usually thoroughly satisfactory. A length of tube of from 50 to 60 diameters, and liberal spacing, seem to be especially advantageous. The specimen here chosen has 850 feet

* *The Locomotive*, Sept., 1884.

of heating and 30 feet of grate surface, is rated at 60 horse-power, but is oftener driven up to 75, weighs 9,500 pounds, and contains nearly its own weight of water, but only 21 pounds of steam, when under a pressure of 75 pounds per square inch, which is below its safe allowance. It stores 51,000,000 foot-pounds of energy, of which but 4 per cent. is in the steam, and this is enough to drive the boiler just about one mile into the air, with an initial velocity of nearly 600 feet per second. The common upright tubular boiler may be classed with No. 4.

Nos. 5–8 are two of the Baldwin and two of the Cooke locomotive boilers, of which drawings and weights are furnished by the builders. They are of different sizes and both freight and passenger engines. The powers are probably rated low. They range from 15 to 50 square feet in area of grate, and from 875 to 1350 square feet of heating surface. In weight, the range is much less, running from 2½ to a little above 3 tons of water, and from 20 to 30 pounds of steam, assuming all to carry 125 pounds pressure. The boilers are seen to weigh from 2½ to 3 times as much as the water. These proportions differ considerably from those of the stationary boilers which have been already considered. The stored energy averages about 70,000,000 foot-pounds and the heights and velocities of projection not far from 3000 and 500 feet; although, in one case, they became nearly one mile, and 550 feet respectively. The total energy is only exceeded, among the stationary boilers, by the two-flued boiler at 150 pounds pressure.

The violence of the explosion of the locomotive is naturally most terrible, exceeding, as it does, that of ordnance fired with a charge of 150 pounds of powder of best quality, or perhaps 250 pounds of ordinary quality fired in the usual way.* On the occasion of such an explosion which the writer was called upon to investigate, in the course of his professional practice, the engine was hauling a train of coal cars weighing about 1000 tons. The steam had been shut off from the cylinders a few minutes before, as the train passed over the crest of an incline and started down the hill, and the throttle again opened a few moments before the explosion. The explosion killed the engineer, the fireman, and a brakeman, tore the fire-box to pieces, threw the engine from the track, turning it completely around, broke up the running parts of the machinery, and made very complete destruction of the whole engine. There was

* The theoretical effect of good gunpowder is about 500 foot-tons per pound, according to Noble and Abel.

no indication, that the writer could detect, of low water; and he attributed the accident to weakening of the fire-box sheets at the lower parts of the water-legs by corrosion. The use of water-grates, the insertion of which produced some loss of strength at the fire-box, may have had something to do with it, however. The bodies of the engineer and fireman were found several hundred feet from the wreck, the former among the branches of a tree by the side of the track. This violence of projection of smaller masses would seem to indicate the concentration of the energy of the heat stored in the boiler, when converted into mechanical energy, upon the front of the boiler, and its application largely to the impulsion of adjacent bodies. The range of projection was, in one case, fully equal to the calculated range. The energy expended is here the full amount calculated.

Nos. 9 and 10 are marine boilers of the Scotch or "drum" form. These boilers have come into use by the usual process of selection, with the gradual increase of steam pressures occurring during the past generation as an accompaniment of the introduction of the compound engine and high ratios of expansion. The selected examples are designed for use in the new vessels of the U. S. Navy. The dimensions are obtained from the Navy Department, as figured by the Chief Draughtsman, Mr. Geo. B. Whiting. The first is that designed for the "Nipsic," the second for the "Despatch." They are of 300 and 350 horse power, and contain, respectively, 73,000,000 and 110,000,000 of foot-pounds of available energy, or about 3,000 foot-pounds per pound of boiler, and sufficient to give a height and velocity of projection of 3,000 and above 400 feet. These boilers are worked at a lower pressure than locomotive boilers; but the pressure is gradually and constantly increasing from decade to decade, and the amount of explosive energy carried in our modern steam vessels is thus seen to be already equal to that of our locomotives, and in some cases already considerably exceeds that which they would carry were they supplied with boilers of the locomotive type and worked at locomotive pressures. The explosion of the locomotive boiler endangers comparatively few lives and seldom does serious injury to property, outside the engine itself. The explosion of one of these marine boilers while at sea would be likely to be destructive of many lives, if not of the vessel itself and all on board.

Nos. 11 and 12 are boilers of the older types, such as are still to be seen in steamboats plying upon the Hudson and other of our

rivers, and in New York harbor and bay. No. 11 is a return tubular boiler having a shell ten feet in diameter by 23 feet long, 2 furnaces each $7\frac{1}{2}$ feet deep, 8 15-inch and 2 9-inch flues, and 85 return tubes, $4\frac{1}{2}$ inches by 15 feet. The boiler weighs 25 tons, contains nearly 20 tons of water and 70 pounds of steam, and at 30 pounds pressure stores 92,000,000 foot-pounds of available energy, of which $2\frac{1}{2}$ per cent. resides in the steam. This is enough to hoist the boiler one-third of a mile with a velocity of projection of 330 feet per second. The second of these two boilers is of the same weight, also of about 200 horse power, but carries a little more water and steam and stores 104,000,000 foot-pounds of energy, or enough to raise it 1,900 feet. This was a return-flue boiler, 33 feet long and having a shell $8\frac{3}{4}$ feet in diameter, flues $8\frac{1}{2}$ to 15 inches in diameter, according to location. These boilers were designed, years ago, by Messrs. Fletcher & Harrison (now the W. & A. Fletcher Co.) of New York City. It was a boiler of the return-flue variety, to which that just described belongs, that exploded in the "Westfield" ferry boat, July 30th, 1871, causing the death of about 100 persons and wounding as many more. The writer was employed to investigate the case for the officials upon whom the duty was legally and technically incumbent. It was found that the cause of the explosion was the extensive corrosion of one of the girth seams of the shell. The accident occurred when the pressure was about that ordinarily carried and considerably less than that at which the boiler had been tested but a short time before. The energy liberated was therefore about the same as would be calculated as above from the known dimensions and capacity of the boiler. The destruction of the boiler itself, its displacement, and the destruction of that part of the boat adjacent to it, were minor effects of the accident.*

A boiler of the return tubular class was tested to the bursting point, under steam, by Mr. F. B. Stevens, at Sandy Hook, November, 1871. The water was up to the water-line and the energy liberated was thus the full amount calculated. As then reported by the writer,† "when a pressure of 50 pounds was reached, a report was heard which was probably caused by the breaking of one or more braces, and at $53\frac{1}{2}$ pounds, the boiler was seen to explode with terrible force. The whole enclosure was obscured by the vast masses of steam liberated; the air was dotted with the

* Journal of the Franklin Institute, September, 1871. R. H. T.

† Journal of the Franklin Institute, Jan., 1872.

flying fragments, the largest of which, the steam drum, rising to a height variously estimated at from 200 to 400 feet, fell at a distance of 450 feet from its original position. The sound of the explosion resembled that of a heavy cannon. The boiler was torn into many pieces, and comparatively few fell back upon their original position." This boiler had been tested by hydrostatic pressure, before its explosion, up to a pressure exceeding by $5\frac{1}{2}$ pounds that at which the explosion occurred.

The writer subsequently estimated the amount of total energy stored in this boiler and analyzed the effects of the explosion, coming to the conclusions: *

"(1.) That it is very certain that the energy of this explosion, and all of its tremendous effects, were principally due to the simple expansion of a mass of steam suddenly liberated, at a moderate pressure, by the general disruption of a boiler of very uniform but feeble strength.

"(2.) That in this case, the liberation of the steam throughout the mass of water contained in the boiler, and which took place by the evaporation of one pound in every thirteen of the water, and which resulted in setting free nearly 70,000 cubic feet of steam, would not seem to have taken place so promptly as greatly to intensify the effects of the explosion.

"(3.) It would seem very doubtful whether Zerah Colburn's hypothesis, which explains the violent ruptures of steam boilers by the supposition that the steam liberated from the mass of water, in cases of explosion, carries with it and violently projects against those parts of the shell immediately adjacent to the point of primary rupture, large quantities of water, which, by their impact, extend the break and increase the destructive effect, can have had an illustration in the case under consideration."

"We have no right to conclude that such an action as Colburn described may not occur in many cases of explosion; on the contrary, the simple experiment described in all text-books on natural philosophy, in which water in a closed vessel, and near the boiling point, is caused to enter into violent ebullition by the reduction of pressure following the application of cold to the upper part of the vessel, exhibits very plainly the probability of an action taking place such as Colburn describes." . . . . "There can hardly be a doubt that cases do occur in which the same action greatly increases the destructive effect of boiler explosions."

* Journal of the Franklin Institute, Feb., 1872.

But the more recent experiments of Mr. Lawson at Pittsburgh seem to the writer to indicate very strongly, if not absolutely to prove, that the Colburn theory has a foundation in fact, and that "not only may explosions be intensified in violence, but that they may be precipitated, by the action of the stored energy of the water contained in the boiler." It is probably the conviction of the majority of engineers familiar with steam boilers that the danger is pretty nearly proportional to the weight of water present. The boiler exploded at Sandy Hook, as above, weighed 40,000 pounds, contained 30,000 pounds of water and 150 pounds of steam, stored over 2,500,000 of thermal units, measured from the boiling point up to 300° Fahr., equivalent to above 2,000,000,000 foot-pounds of mechanical energy, or enough to raise the whole mass more than five miles. Of this only a fraction was available, however, as shown in Table II.

The last three boilers on the list in Table II. are of a type which has come into common use only during the last ten or fifteen years. They are water-tube boilers, and all of what is popularly known as the "sectional," or "safety" class. Where a boiler is exploded, the disruption may be either general, as in some of the cases cited above, or it may be local, affecting only a limited portion of the structure. It is evident that the localization of the injury is desirable as a means of limiting the rate of discharge of the stored available energy, and thus reducing the damage resulting from the accident. It was pointed out as long ago as 1805, by the greatest engineer of this country, at that time—Col. John Stevens, of Hoboken—that the construction of boilers consisting of water-tubes, principally, afforded a means of securing comparative safety from explosions, and a patent was issued to him by the British Patent Office, at that date, for a boiler resembling in its general construction the modern "safety" boiler. In the specification, communicated to the office by his son, John C. Stevens, the original of which is in the hands of the writer, Col. Stevens explains this principle of subdivision of the mass of water and of steam in boilers, as a means of insuring against destructive explosion as clearly as it has ever been explained by his recent followers. All of the later forms of boiler belonging to this class have followed the same general plan. The writer has selected the forms here described mainly because of their being most familiar to him. He has been engaged while preparing this paper in directing the introduction of 250 horse-power of one type under a very large and valuable building in New York City,

where he felt unwilling to take the risk of employing a shell boiler; he has had a boiler of another of these forms under his feet, when in his lecture-room, for more than a dozen years, where the location of a shell boiler would have been a continual source of apprehension; and he has experimented with still another of the selected forms sufficiently to feel thoroughly at home with it, and to feel the same confidence in its safety that he has in the others. Every prudent engineer is careful to keep a shell boiler well inspected and well insured, and knows that, so cared for, the risk in their use is reduced to a very insignificant quantity; yet the writer, and probably every other engineer, finds it very satisfactory to be able to feel that any boiler that he may be compelled to place under a building, or where many lives may be endangered by its explosion, is so constructed that, even were explosion to occur, it would be productive of minimum and probably small damage. The writer has not hesitated, however, where great differences of cost have entered into the case, and where the boilers could be set in a separate boiler-house, to advise the use of the shell boiler. By proper construction and with careful management and systematic inspection, the danger and risk are reduced to a very small amount.

The "sectional" boilers are here seen to have, for 250 horse-power each, weights ranging from about 35,000 to 55,000 pounds, to contain from 15,000 to 30,000 pounds of water and from 25 to 58 pounds of steam, to store from 110,000,000 to 230,000,000 foot-pounds of energy, equal to from 2,000 to 5,000 foot-pounds per pound of boiler. The stored available energy is thus usually less than that of any of the other stationary boilers, and not very far from the amount stored, pound for pound, by the plain tubular boiler, the best of the older forms. It is evident that their admitted safety from destructive explosion does not come from this relation, however, but from the division of the contents into small portions, and especially from those details of construction which make it tolerably certain that any rupture shall be local. A violent explosion can only come of the general disruption of a boiler and the liberation at once of large masses of steam and water.

In the year 1872 the writer, preparing the report of a committee conducting tests of steam boilers at the exhibition of the American Institute for 1871, with the approval of the committee, wrote,*

"In this class, of which there are many different kinds in the market, the water space, and frequently the steam space, of the

* Journal of the Franklin Institute, Feb. 1872.

boiler is contained in a large number of comparatively small compartments, each of which is very strong, and the explosion of which is not likely to result in that widespread destruction of property, and that great loss of life which so frequently follow the explosion of the older and more common forms of steam boiler.

"Your committee feel confident that the introduction of this class of steam boilers will do much toward the removal of the cause of that universal feeling of distrust which renders the presence of a steam boiler so objectionable in every locality. The difficulties in inspecting these boilers thoroughly, in regulating their action, and other faults of the class, are gradually being overcome, and the committee look forward with confidence to the time when their use will become general, to the exclusion of the older and more dangerous forms of boilers."

The writer is confident that this is still the sentiment of engineers generally, and the time to which that committee then looked forward with such interest is rapidly approaching. The figures just given and the comparisons made in this paper, may aid, somewhat, in awakening engineers to a realization of the importance of carefully considering the magnitude and the dangers of the wonderful force with which they have to deal and to the importance of finding ways of making its use satisfactorily safe.

It should be noted that equations (A) and (B) give the quantity of energy available from unity of weight of the fluid, expanding from the initial temperature and pressure down to the temperature of steam of atmospheric pressure, as used in a non-condensing engine. The energy produced by that part of the operation represented by the expansion-line, in the boiler explosion, is obtained by deducting from this total the product of initial pressure, above the atmosphere, by the initial volume. Table I., of "available" energy, represents the total; Table II., that of "stored" energy, the latter part. The former is the measure of the maximum work possible in a non-conducting cylinder; it represents a limit which may be approached, but which can never be reached in practice.

The energy of steam alone, as stored in the boiler, is given by column 10 of Table II. It has been seen that it forms but a small and unimportant fraction of the total stored energy of the boiler. Table III.* exhibits the effect of this portion of the total energy, if considered as acting alone.

* Table III. and related text were not ready at the time of reading the preceding part, and did not, therefore, appear in the copies then distributed.

TABLE III.

STORED ENERGY IN THE STEAM SPACE OF BOILERS.

Type.	Energy, Total.	Stored in Steam ft. lbs.) per lb. of Boiler.	Height of Projection.	Initial Velocity per sec.
1. Plain Cylinder..........	676,693	271	271 ft.	132 ft.
2. Cornish	709,310	42	42 "	32 "
3. Two-flue Cylinder.... ..	2,377,357	351	351 "	150 "
4. Plain tubular	1 022,731	108	108 "	83 "
5. Locomotive.............	1,483,896	76	76 "	69 "
6. "	2,135,802	85	85 "	74 "
7. "	1,766,447	86	86 "	74 "
8. "	1,302,431	107	107 "	83 "
9. Scotch Marine.....	1,462,430	54	54 "	59 "
10. " "	2,316,392	61	61 "	62 "
11. Flue and Return Tube...	1,570,517	28	28 "	42 "
12 " " " ...	1,643,854	29	29 "	43 "
13. Water-tube.............	2,108,110	61	61 "	59 "
14. "	3,513,830	79	79 "	71 "
15. "	1,311,377	24	24 "	39 "

The study of this table is exceedingly interesting, if made with comparison of the figures already given, and with the facts stated above. It is seen that the height of projection, by the action of steam alone, under the most favorable circumstances, is not only small, insignificant indeed, in comparison with the height due the total stored energy of the boiler, but is entirely too small to account for the terrific results of explosions frequently taking place. The figures of Table III. are those for the stored energy of steam in the working boiler; they may be doubled, or even trebled, for cases of low water; they still remain, however, comparatively insignificant. While they may account for the explosion-effects seen at the Sandy Hook experiment, it is by no means certain to the mind of the writer that they do so; it is very evident that they are not sufficient to account for the more violent explosions described. Either the energy stored in the water has been, in such cases, thus effective, or other sources of energy must be sought. These calculations, therefore, may, on the whole, be taken as strong evidence, if not absolute proof, of the correctness of the Colburn Theory of Steam-Boiler Explosions.

CLXVI.

RECENT IMPROVEMENTS IN DRAWING-BOARDS.

BY THEODORE BERGNER, PHILADELPHIA, PA.

It is a fact universally acknowledged among engineers and draughtsmen that the drawing-board and T-square are, in their dependence upon one another, and even at the best of their various modifications of construction, only imperfect and unreliable means for drawing absolutely parallel lines with ordinary care.

Nearly all designers perhaps have at times experienced the truth of this statement when taking up their drawing instruments on some Monday morning, eager to unburden the busy brain of some newly matured scheme by its graphical demonstration upon a drawing then in progress, the lines of which had agreed with the relative conditions of the board and T-square until they were left on the previous Saturday afternoon, but which over Sunday had become so sadly distorted by a change of temperature or of humidity of the atmosphere that the fresh ardor was seriously checked by the discovery of this fiendish power of matter over mind.

Among the attempts to make the board and T-square more liable, various modifications of construction have been devised: many kinds of wood have been tried, selected with utmost care and subjected to every conceivable manner of seasoning, and when all of these efforts failed to make those mutually dependent implements thoroughly reliable in their fundamental requirements for reasonably accurate work, resort was even had to expensive steel T-squares and to boards with steel edges.

These, if leading to greater accuracy on the one hand, have to be handled with great care, while the weight and sluggish friction of such a metal T-square or blade interferes seriously with the quick handling and ready yield to slight pressure essential for the uniform gradation of narrow spaces between the lines.

The subject to which your attention is now invited is the result of persistent efforts to arrive at more absolute accuracy in the drawing of parallel lines, and this by making the action of a parallel ruling device quite independent of the edges of the drawing-board, upon the relative degree of truth of which the T-square is dependent.

One of the earlier efforts to arrive at a really practical solution of this problem formed the inanimate subject of a patent granted to the writer on the 18th of April, 1871. It promptly failed to satisfy others less sanguine, and soon also myself, in its application to practice in the drawing-room. Having disappointed even the modest hopes built upon it, it soon went over to the overwhelming majority of still-born patents.

I finally succeeded in accomplishing my object in a very simple and effective manner by means of an endless cord, stretched crosswise over four little grooved pulleys provided at the corners of the board in the manner shown at Figs. 60 and 62 of the drawings illustrating these present remarks.

Before proceeding with the description, I will here immediately anticipate and endeavor to dispel the honest doubts which I know will at once arise in the minds of those present who have not yet had occasion to use or to test such boards, by acknowledging to you my own earnest misgivings and fears as to the practical efficiency of a frail cord for my purpose until the first trial encouraged me in its adoption and until its daily practical use had with time fully demonstrated its thorough reliability under the most adverse conditions, such as sudden changes of temperature or humidity of the atmosphere.

By a brief practical demonstration which I propose to exhibit before your eyes presently on a large upright board, I trust to be able to accomplish more toward carrying conviction to your minds than by mere words. To these I will, however, add, that in my own experience and during several years of steady use of these boards in my offices, I have not only never had occasion to renew any of these cords, but have on the contrary found them rather to improve with time, inasmuch as they become "set," as it were, or more and more inelastic the longer they are in use.

It has been a surprise to those who have had practice in the use of these boards how really slack the cords may be allowed to become without materially impairing the accuracy of the work. For such neglect of the proper condition of the cord there is, however, no excuse, in view of the handy means provided for adjusting the tension of the cord.

The drawing-board I prefer to make, for several good reasons, of a thin top slab of wood A, secured upon a rectangular, open frame a a. By crossing the cord c underneath the slab A, the two portions of this cord which run parallel with two opposite edges of

the board are made to move in the same direction, or, in other words, if the portion of the cord on the right-hand side of Fig. 60 be moved a distance of one inch upward, in the direction of its ar-

Fig.60.

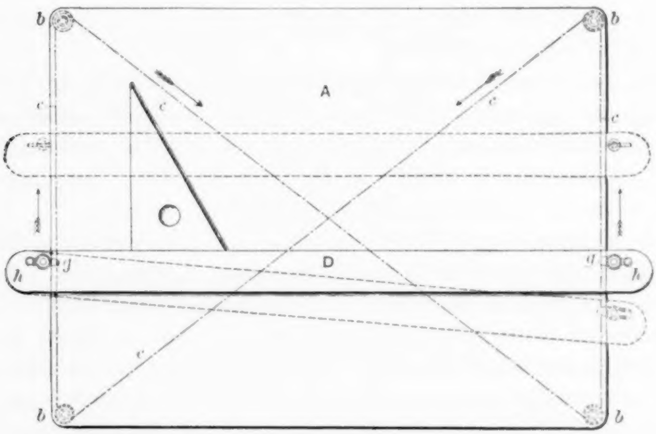


Fig.61.

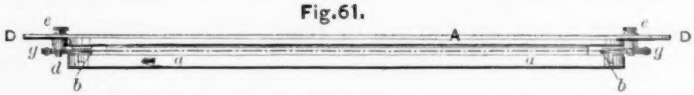
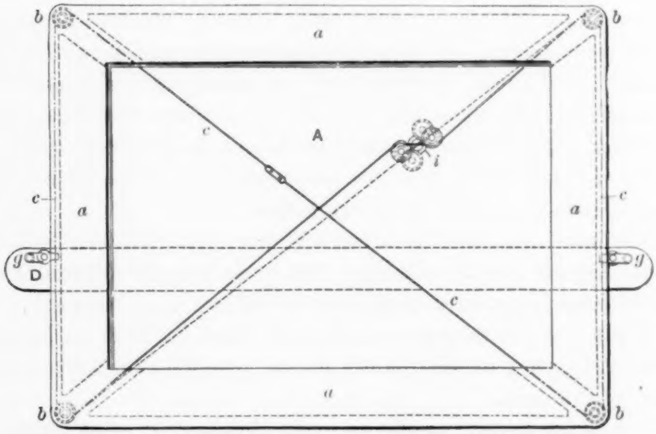


Fig.62.



row, the left-hand portion of the cord must advance the same distance in the same upward direction, as indicated by *its* arrow This equal amount and direction of movement of the cord *c* at op

posite sides of the board is imparted to the blade or parallel ruler *D* by securing the ends of the latter to the cord in the manner shown in Figs. 65 and 67, and which I will briefly describe. The ruler *D* carries at each end, at its under side, by means of a screw *d* and milled nut *e*, the cylindrical block *f* and the elastic metal clamp *g*. The jaws of this little clamp are open at the end next to the board, and when tightened in this position take a firm hold of the cord, in the manner indicated at Fig. 65. This tightening of the clamp *g* by means of screw *d* and nut *e* secures at the same time the block *f* firmly to the ruler *D*. On the other hand the release of clamp *g*, also loosens the block *f* from the ruler and permits its outward

Fig.66.

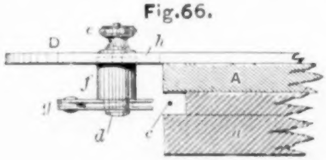


Fig.65.

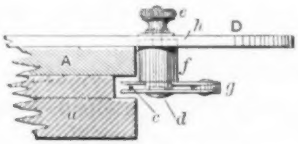


Fig.63.

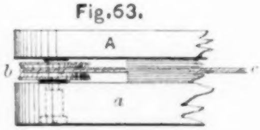


Fig.64.

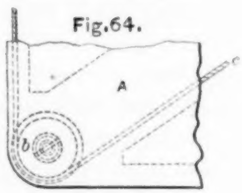
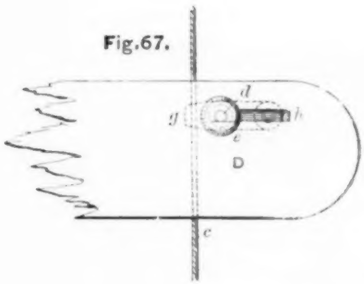


Fig.67.



withdrawal in the slot *h* (as in Fig. 66), so as to liberate the cord entirely from the clamps *g g*, when it is desired to detach the ruler from the board.

The accurate parallelism of motion obtained in this way is, as will readily appear, in nowise dependent upon the condition of the board, the edges of which may be considerably out of truth without in the least impairing the functions of the cord *c* and ruler *D*.

As a test of the degree of accuracy obtainable by this device without special care, or even with such manipulation as would presumably tend to adverse results, two blank cards may be fastened at opposite ends of the board at hand, the endless cord of which is

over 17 feet in length, and, as you will observe, of very moderate tension. If now a series of lines be drawn across these cards, these lines shall prove to be parallel, although I will, in moving the blade, alternately take hold of it at extreme ends (that is, having drawn a line, I will for the next line move the blade by its right end, then by its left end, and so change about for a series of lines). The true parallelism of these may now be observed by bringing the edges of the two cards together. (The ruled cards were taken from the board and produced for inspection.)

Any required degree of inclination of the ruler in either direction from the horizontal line may be given, and correctly maintained, by securing its two clamps, *g g*, to the cord *c* at such relative points as will produce the desired angular position of the ruling edge.

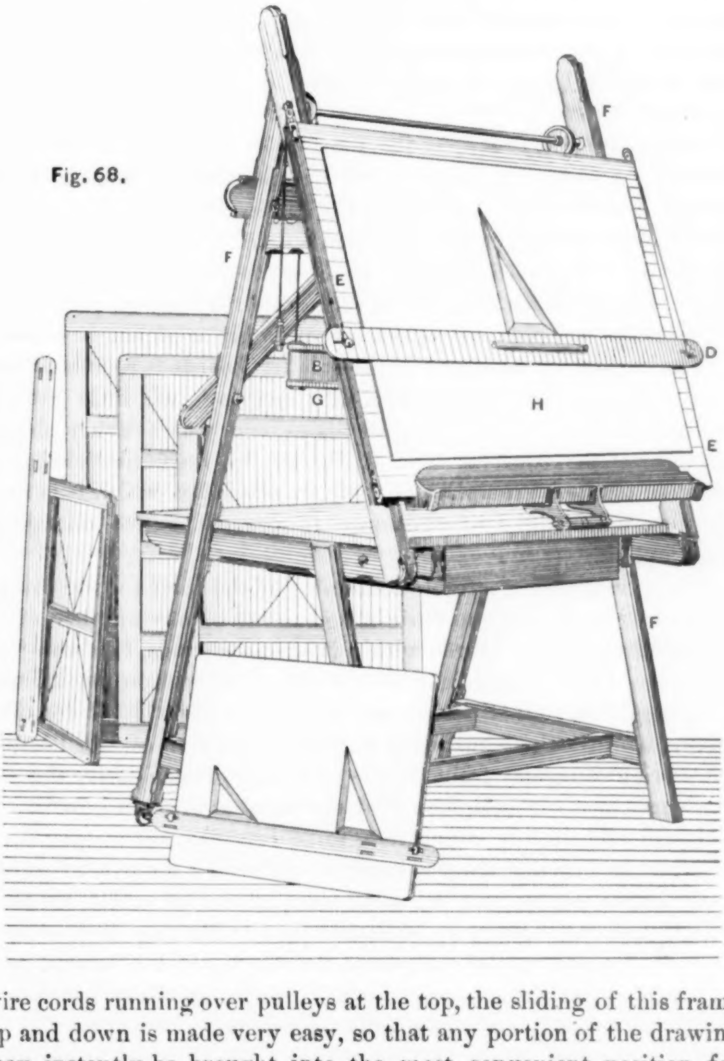
Considerable end play of the ruler is not only admissible without impairing true parallelism of lines, but some play is actually needed to compensate for inaccuracy of the board by permitting the blocks *f* to slide freely past the widest part of the board, as its edges are likely to be neither straight nor parallel.

To a feeling of security and full reliance upon the correct action of this ruler, which the draughtsman soon experiences, comes that sense of ease with which he finds he can move his blade up and down, taking hold of it for that purpose at any most convenient point. It soon makes him forget that unceasing care with which his left hand always had to bear upon the blade, and at the same time press the head of his T-square against the guiding edge of the board, while his right hand was drawing the line along the blade, which he found more and more yielding and insecure the farther the work was remote from the head-block of his T-square.

After the practicability of the new parallel motion was assured, very little reflection naturally suggested the adaptation of such boards to an upright position, and this change was, especially for large work, soon found to offer such remarkable advantages, both from a sanitary and technical stand-point, that it was promptly adopted in a number of large engineering offices. While I have no medals and diplomas from Humane Societies, attesting to the saving of lives of any of my fellow-men, I am in possession of several very warm letters of thanks from persons whose health had been greatly impaired by persistent occupation at the drawing board, and who with the change to a more healthful position have experienced great relief and a return to normal strength and vigor.

The plate, Fig. 68, represents a clear, comprehensive outline of this upright class of the improved boards. In this construction a frame, *E*, is mounted upon a stand or easel, *F*, at an inclination of about 20°. By means of a counterweight, *G*, in the rear and two

Fig. 68.



wire cords running over pulleys at the top, the sliding of this frame up and down is made very easy, so that any portion of the drawing may instantly be brought into the most convenient position for work upon it.

The parts for the parallel motion of the blade are fitted to this

frame, which is so recessed as to receive a plain board *H*, that can be instantly removed, and another one of same size or of smaller dimensions put in its place, so that any number of boards for a whole series of views of a complicated machine or structure, or for different plans in progress, may be in hand simultaneously and readily interchanged. On these upright frames the blade, *D*, is necessarily also counterweighted, as shown, to make it slide with the least resistance in either direction, and to hold it in any position.

In these vertical boards the tension of the cord is adjusted by means of a button, over which the cord is wound at one end of the blade; a partial turn of this button in one direction increases the tension and in the opposite direction slackens the cord. The clamp at the other end of the blade provides for the adjustment of the latter, so as to place it either in its normal, horizontal position, or to set it for any required amount of obliquity in either direction.

The tension of the cord in the smaller boards is regulated with ease by a button plate, *i* (Fig. 62), carrying two little grooved rollers, between which the cord passes, and which either slacken or tighten the cord (as seen at Fig. 62) by respective change of position of the button plate, *i*, about its axis, upon which it is made to turn with sufficient resistance to prevent undue change of position.

The advantages of a frame combining all the parts for the parallel motion of the blade, and permitting the use of any number of interchangeable plain boards within it, are so obvious, that this construction has recently been also adopted for use on the ordinary drawing-table, so that those who, from force of habit or for other reasons, prefer the accustomed horizontal position of the board to the upright one, may from the use of a single frame derive the conveniences of ready interchange of any number of plain boards.

I will add, in this connection, some remarks which may be of interest, as bearing upon a peculiar material I discovered during early experiments, and the superior qualities of which led to its adoption in the making up of these boards. Having decided upon the use of a thin slab of soft wood for the top of the boards, with a strong, open frame underneath, through openings or slots in which the crossed cord was readily passed, and which frame, as the stronger member of the two, would keep the thin slab from warping, I experienced some difficulty in finding uniformly clear, straight-grained thin stuff, properly cut from the log, and so

thoroughly seasoned as to withstand satisfactorily the effects of varying temperatures, etc.

At about that time I was commissioned to visit the Paris Exposition of 1878 in charge of the extensive exhibit and other foreign interests of a large Philadelphia manufacturing establishment, and while there I observed in the American section the unique exhibit of a New York firm, consisting in part of a number of thin, neatly edged and planed boards of spruce, termed "Sounding-board Stuff;" and, secondly, of a series of completely made up sounding-boards of the various sizes and shapes required for the different styles of pianos. The perfectly uniform and superior quality of the wood at once attracted my close attention. It stood in an open rack, exposed during six months or more to every change of weather and season, and although its surfaces, clear and spotless at first, soon suffered in appearance by dust and by splashes of water from the sprinkling cans of the sweepers, every piece was sound and in good conditiou at the close of the Exposition.

Having had my longing eyes on this wood for months, I was highly pleased to find the exhibitor's representative willing to turn it and the shipping case over to me at a nominal figure. I had it packed for return to this country, and it formed part of that luckless government steamer the *Constitution*, which, after several narrow escapes from shipwreck, once grounding on the Irish coast, and then, after a succession of gales and loss of her rudder, making port entirely out of her course at Lisbon, Portugal, for coal and repairs, finally discharged her homeward bound and badly shaken and damaged exhibits at New York.

Soon after receipt of my precious freight in Philadelphia, I had the whole of it made up into drawing boards, with such success that the same quality of material, from the same source, has been procured for the boards ever since. It is simply faultless.

Upon further inquiry into the source and manner of preparation of this material, I found that the manufacturer is not only very largely engaged in supplying many piano-makers of this country either with made-up sounding-boards or with the stuff for them, but that he exports largely to Europe, having a branch house at Leipzig, Germany.

On his extensive tracts of forest in the best spruce region of this State this immense business has been organized upon the basis of long practical experience and science combined. Of the faultless trees selected, only the butts are used, which secures a clear and

straight-grained article. All such logs are invariably quartered first and then sawed $\frac{5}{8}$-inch thick, the cuts running as nearly parallel as possible to the radius of the tree.

After the boards have been exposed to the frost and open air under sheds for several months, they receive a thorough additional seasoning in steam-heated rooms, and are then planed on both sides down to $\frac{7}{16}''$ or $\frac{3}{8}''$ and edged. Then commences a careful classification of the boards. They are first assorted with regard to their color, grain, and clearness, then again according to length and width, so that finally each lot represents in every respect a perfectly uniform standard of quality. The corner-pieces of the quarter logs which are not wide enough for boards are cut into bars for sounding-boards, or are used for making other minor parts of the piano, for which spruce is suitable.

DISCUSSION.

Mr. Oberlin Smith.—Mr. President, in illustration of the remark of Sidney Smith, that "those confounded ancients were always stealing our ideas," I will say that Mr. Bergner borrowed some original ideas of mine and put them into practical use in this drawing-board of his years before I ever thought of them. A few months ago I wanted to put up a board for my own use, and I hit upon this idea. I put the grooved pulleys at the corners and stretched a small steel wire, crossed underneath (just as in Mr. Bergner's imitation), to which I attached the ends of the square blade. I must say, however, that Mr. Bergner's is better than mine. He not only basely imitated mine, but had the effrontery to improve it very much—and all this before I had time to think of it. As soon as I need another drawing-board, I will get one of his. I think it is the best thing ever gotten up for a drawing office. It is, indeed, almost absolutely perfect for ordinary work.

Mr. Towne.—I am very glad to add a word of testimony to the merits of this drawing-board. I may say briefly that in the drawing office of the Yale & Towne Manufacturing Co. we have, I think, eight or ten of these boards in use, the oldest of them for five or six years, and with perfect satisfaction always. We have seven or eight draftsmen at work using them, and we would not think of dispensing with them.

CLXVII.

*LOCKS AND THEIR FAILINGS.**

Reported and Revised from an Address

BY A. C. HOBBS, BRIDGEPORT, CONN.

A LOCK is made most secure against picking when it is so constructed that the operator must take his chances of hitting the combination or at getting the exact form of the key. If he can get at these facts by observation and the use of his trained senses, he can make a key or open the lock if he is allowed sufficient time.

Now, the oldest lock—and when I say oldest I speak without much exact knowledge of it, but the lock we think is the oldest—is

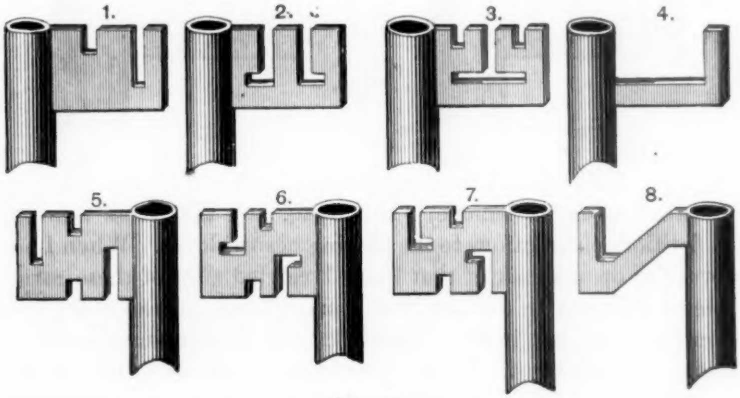


FIG. 69*a*.

what is known as the warded lock. Those locks are made with a series of obstructions placed in and about the key-hole, so that an accidental key although entering the key-hole will not turn, and some are made so that a strange key will not enter the hole. The key of a lock of that class will look like No. 1 in Fig. 69*a*. In fact, the key will tell really what is in the lock. Now suppose that

* I may have frequent occasion to refer to what I have personally done in the way of discovering and exposing the failings of locks. I am aware that in so doing I lay myself open to the charge of egotism, but on that point I must beg your indulgence, for I see no better way of carrying out my purpose of showing how easy it is for mechanical engineers and lock makers to be mistaken.—A. C. H.

blank was full in No. 1, you could not get it in. But if you had a little smoke on the key, you would find by pressure and rubbing where the obstructions were, and by filing away by little and little, it could enter the lock. No. 2 shows a different form, which not only has an obstruction, but here you have an L shape which you have to pass. You go still further and in No. 3 you have to pass this T shape. All you want is to clear the obstructions, and a skeleton like No. 4 will pass them all and open all the previous three locks. In the lower line you have still more complicated keys and still more difficulty. But you put your blank key in with a little wax, or smoke it, and step by step you can find these places out, and skeleton No. 8, which will pass all the other obstructions, will open all locks whose keys are like those on the lower line. That anchor

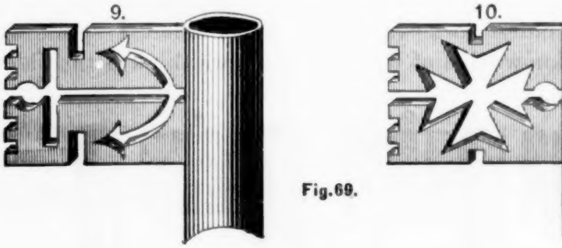


Fig. 69.

(Fig. 69), designed to give special complication (which is not to the windward to-day, I believe) [Laughter], and that cross represent but fictitious obstructions to the picking of such locks. When I used to want to open a lock of that kind, I carried a lot of these curious skeleton things with me like Nos. 4 and 8 ; but you can easily make them from the lock itself. The lock tells its own story by its obstruction to the keys. The whole security of the lock depends upon the difficulty of fitting something that will pass those obstructions, and locks of that kind have been made varying in size, varying in form, varying in workmanship. In the Athenæum in Boston, they have a picture of Pat Lyon, of Philadelphia. He was considered one of the finest workmen in the country ; and he made a warded lock for the Philadelphia Bank, and told the parties when he put it on, "if ever that lock is opened except by its true key you can charge it to me." [The lock was opened, and I will tell you more about that after a while.] Banks have depended on this type of lock, boxes of all kinds, trunks, bags, etc., have been locked with locks of that kind, and those locks are still continued in use.

The next thing seems to have been to supply a series of movable obstructions or pieces—they may be called tumblers, pins, or slides, or whatever you please—but there is a series of movable pieces, which as you turn the key are moved to certain positions and allow the lock to be unlocked. The point to be ascertained in these cases is how these pieces should be moved. The oldest lock I know of that kind is what is called "the Egyptian lock." Fig. 75 is taken from a lock which I saw illustrated in the British Museum. That is to go on the outside of the door, not the inside, and the front and back staples are all made of wood. If the right key is slipped in, you raise those pins up, and the bolt can be moved back

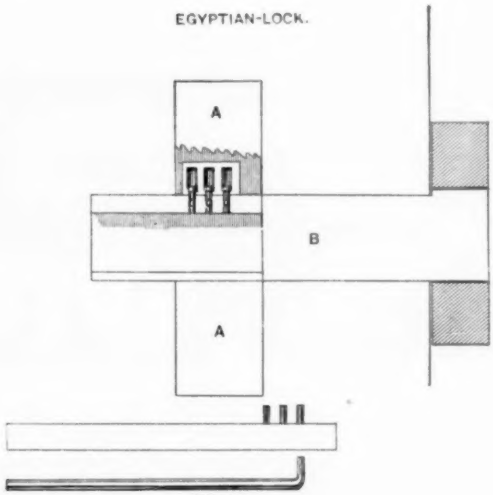


Fig. 75.

or forward as you please. The difficulty of opening that lock without the key is to ascertain the position of those pins and the distances through which they should be moved. The principle which has been working through many locks for years is developed fully in that early lock, and it can be made to show how such locks can be picked. If you were to put something in the key-hole and push these pins up they would drop down again, if that was all you did. But if you were to take hold of the front end of the bolt of the lock and push it, and then were to put in a piece of bent wire to lift the pins, you would find that one of those pins would be bound more than the others, and would be held up, and so in about the time I have been talking of it, you would open the lock.

You take a single pin while the pressure is on and push it up. It stays there until the last pin is up, and back goes the bolt. The same point is carried out in what is called the Stansbury ward locks (Fig. 73). That really had no wards or fixed obstructions, but it had a disk, and in the disk a series of holes. In those holes are a number of pins, forced forward by springs as shown below. The key is made so that its end view appears as in the cut, the series of dots representing pins. If you try to turn the disk back you can bring pressure on those pins one at a time and push them out. But what is more simple to do is to make a key, the form of the key being governed by the size of the key-hole, and put a little wax on the end of it. By pressing it on the disk it shows exactly

STANSBURY'S-LOCK.

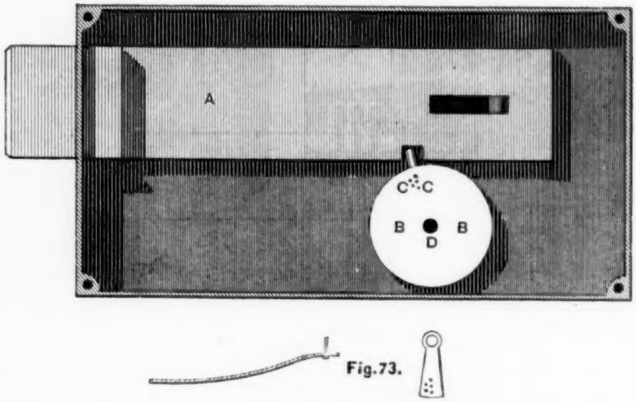
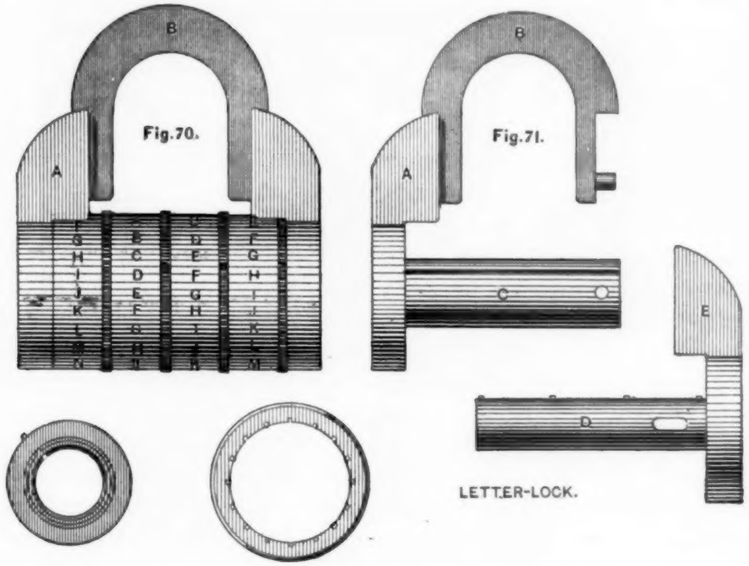


Fig. 73.

where the pins are, and you can by that means form a key to open the lock.

I went to a Philadelphia bank some years ago to sell them a lock. They were very well satisfied with the locks they had on, but told me that if I would open their door they would buy a lock, and that was just what I wanted them to say. On the outside door they had one of those Stansbury locks; on the inside, a lock of Pat. Lyon's, "the best in the world," and besides that they had another fastening. I took the measures of the key-hole, formed my key, and went the next morning, and in about eight minutes I opened the outside door. I went to the inside door, and with an instrument which I had made I turned the bolts back, but could not open the door. I locked them again and unlocked them. Still the door would not open. I used to carry a little box with me about fifteen

inches long, which had in it trays full of almost all kinds of honest instruments, though some call them burglar's tools. I also had a lot of watch springs. I took a watch spring and felt along the back of the door and felt the hinge, and went along and felt what they call the dog. I came to the centre of the top of door, and it stopped. I locked the lock and I felt the three bolts. I threw them back again with my key. I knew there was a bolt there which was not connected with the lock. I examined around on the inside of the outer door, and I could find nothing there. I felt in the bolt-holes of the outer door. There was nothing there; but near the outside of the door was a counter with scales on it for weighing



LETTER-LOCK.

their gold. I took hold of the case that covered the scales, tipped it up, and the door came open. So that all the security they had was from these warded locks, and on the inner door was that secret bolt. As the result of my operations I sold them a lock. Let me say that Pat. Lyon told them when that lock of his was put on, that if any one opened that lock without the key that he was the man that did it. They took him at his word, when the bank was robbed, and he was arrested and put in prison. He finally proved his innocence, and they found the burglars, and he recovered quite a large amount of damages; and they thought so much of Lyon after that, they had his picture put in the Boston Athenæum.

The next type of lock is what is known as the letter padlock (Fig. 70), and that has a great number of changes and combinations. It has a cylinder, and that has a slot in it. The portion D (Fig. 71) slides in the cylinder C, and a set screw turns into that slot in D, so that it cannot be drawn quite out. The first account of that lock is in a book published in Amsterdam, and it gives the date as 1682. They had a set of four rings, and on the rings was a series of letters, as you see in Fig. 70. They picked from those letters some combination of four, and the rings were put into the slide, and when those notches on the inside of the rings were

DIAL-LOCK.

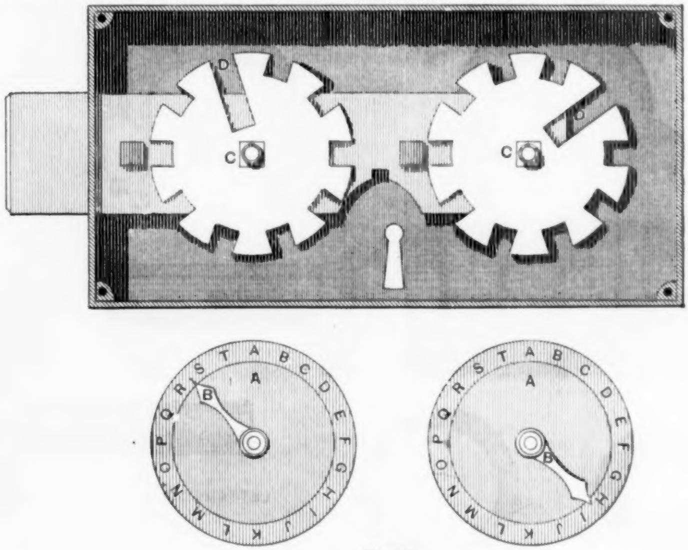


Fig. 72.

brought into the right relative positions they allowed the lock to open. They supposed the security of that lock depended on the chances of hitting the combination of letters. Those four rings will give something over eight thousand different combinations, and the chance of hitting the right one is very small. Fig. 70 was improved by Regnier, of Paris, in 1786, by putting a set of outer rings on. The position of the outer ring on the inner one, which carries the notches, can be varied as you please, so that you can change the combination every day if you wish. Now the trouble is to ascertain what the combination is on which the rings are set. If you undertook to run the chances, you might do it the first time, but

the chances would be very small, and if it were only to be opened in that way, it would be as secure a lock as could possibly be made. If you take hold of those rings you will find they are all perfectly free; but if you put something in between the ends of the lock that tends to push them apart, you will find that whatever strain is put on these shackles A and E to force them apart, comes on these pins. These rings bind, one after another, and feeling them as they bind, you turn them around until they are free, and by and by, before you are aware of it, your lock will drop open. Last year when I was here I told some stories about it.*

Fig. 72 shows a modification of the letter-padlock principle, and is what is called a dial lock. I had been in London about eight or ten days at the time of the Exhibition in 1851, when I received a letter from William Brown, St. James Hotel, to come and see him. I went to see him—William Brown, M. P., and head of the house of Brown, Shipley & Co., the bankers. He was very glad to see me, and, after talking over many things, finally he came around to locks. He had invented a lock, and he explained it very clearly to me. "Now," he said, "this dial, and these notches, and these thousands and thousands of combinations—it can be set in any way, and how would it be possible to open it?" I said, "Mr. Brown, I think your lock is very much like the letter-padlock, and if it is, it can be opened." "Oh," he said, "you do not understand me." I said, "Well, explain it again." He explained it again, and then said, "Do you think you can open such a lock as that?" I saw he was very well satisfied with his lock, and so, to evade his question, I simply said, "I should not like to say." He said, "Next time you come to Liverpool, come and see me." I did not think much more about Mr. Brown or his lock, thinking that if he was pleased with it there was no occasion for me to disabuse his mind. About four months after, I received a pamphlet containing the proceedings of the Archæological Society of Liverpool, and the last paper read was a paper by Sir William Brown on locks. He wound up in this way: "During the time of the Exhibition in 1851 I saw Mr. Hobbs, and I described this lock to him. At first he said he could open it, but after a more *thorough explanation* was made to him, so that he fully understood the lock, I again asked him the question, and his answer was so evasive that I have made up my mind he could not open it. Then I said, "Mr. Brown, your case wants seeing to by this time." I went to Liverpool, called at his banking-house, and he seemed very glad to

* Transactions A. S. M. E., Vol V., page 128

see me. He said, "I have just got a safe coming in that I want to show you." He showed me an old lock, as the new one was not then accessible. The lock had four dials, instead of those two as shown in Fig. 72. He locked it, and I began to explain to him what I should do if I were going to try to pick it. There was no key to fit such a key-hole as shown in the cut, but there was a wrench to put in and throw the bolts when the dials were turned in the proper position. I said, "You do not think anything of that wrench, do you?" "No," he says; "we just throw that on top of the safe at night." I said, "If I wanted to open this safe, I should take this wrench and put it in and feel the bolt while turning the pointers on the outer dials around." Presently the cashier came, saying to Mr. Brown that a gentleman wanted to speak to him. So he asked me to excuse him, and went away. Just as he turned away the bolts went back! Said I, "Mr. Brown, don't leave me with this door open. It has come open." He said, "How did you open it?" I said, "I do not know. I was turning the wheel around and it came open." He called the cashier to lock it again. I turned my back to the lock, and in about ten minutes opened it again. There were four of these studs and four of these dials. In each of the dials within the lock there was one deep slot, and several shallow slots. Now, as that inner dial was turned around, when it came to the shallow slot the bolt would start slightly back, and then I would feel around until I came to another slot. Then I turned them around until I came to the deep notches, and the bolt went back. The lock was quite as easily opened without knowing its combination as if you knew it. Mr. Brown was very well satisfied, perhaps, but not much pleased.

This is the foundation, I think, of all letter permutation locks—this letter padlock. They have all that peculiar feature. I am speaking now of 1851, or a little later. I do not speak of modern combination locks, of course. The locks I speak of all had that same defect. With a delicate touch a man could sit down and open them just about as well without a key as with one, or without any knowledge of the combinations on which they were locked.

The next form of locks with movable obstructions are what are called the tumbler locks. Most of them have pieces such as C (Fig. 78), pivoted on a pin. There is a stump, B, as it is called, and then what are called gatings in the tumblers. Those tumblers, when the lock is locked or unlocked, hang over the stump as shown, and the key is made with a series of steps which are called bits. That

bit at the end is called the bolt bit, which takes hold of the bolt to move it; but before that reaches its part of the bolt, the bits above it have taken each hold of a tumbler, raising it to a different point. This last bit then takes hold of the bolt and moves it back and forward.

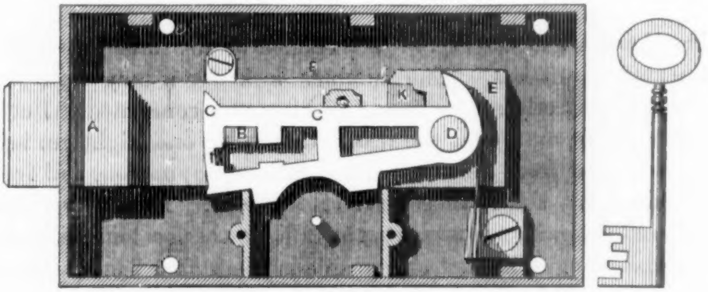
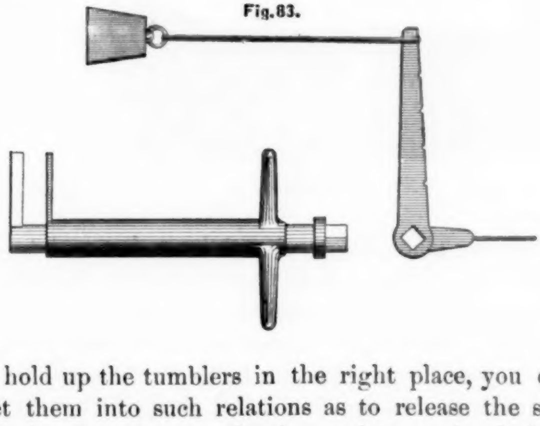


Fig. 78.

That is the general movement of all tumbler locks. If you were to undertake to feel those tumblers to tell whether they were right or wrong, without a pressure on the bolt, you would not feel anything. But if, by a proper instrument, you were to take hold of the bolt first, and you were then to feel these tumblers, you could very readily tell on which the pressure came, and by that pressure

Fig. 83.



acting to hold up the tumblers in the right place, you could very easily get them into such relations as to release the stump and open the lock. In order to make the work on such a lock as simple as possible, if you will notice the form of key shown in Fig. 83, you will see that there is a bolt bit, and there may be a series of bits of different lengths, turning freely on the shank of the bottom bit. Now, by putting this instrument into the lock, that bottom bit

16

goes against the bolt, and the other one is left perfectly free. We hang a weight upon the arm from the bolt bit, to get a pressure. We take hold of this movable bit, feeling the tumblers one after another, and sliding them up, the weight on the bolt bit keeping the stump bearing against the tumblers. By-and-by back goes the bolt.

The Andrews locks (Fig. 74) are opened on the same principle—the tentative process. The tumbler lock for a long time was considered perfectly safe so far as picking was concerned. But it was considered unsafe for the reason that some one might get hold of the key, and from the original key take an impression, and from that make a key and have control of the lock. Therefore it was considered desirable to have what is called a changeable lock and a changeable key. The first changeable-key lock that I know of was invented by Dr. Andrews, of Perth Amboy (Fig. 74). Those tum-

ANDREWS-LOCK

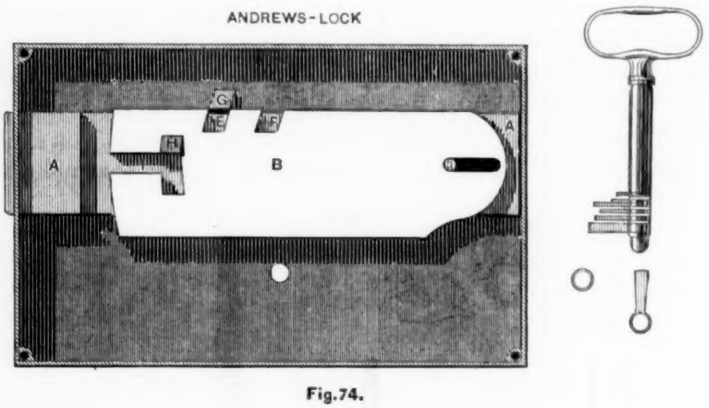


Fig. 74.

blers B are simply plates of steel and brass fastened to a pin D. Now the outer bit of that key, and the first bit, were always necessary to work the bolt. These bits would raise this outer tumbler up, it would catch on the pin G, and the other tumblers not being caught there would draw with the bolt, so you could use the key with the bolt bit and the first bit, and lock the lock with one tumbler. That lock was changeable so far as this feature went, that you could either lock it fully with all the tumblers or lock it with part, or if you wanted to change the key, lock and all, you could take your lock off the door, change your tumblers, and then change the bits of the key. When we put this tentative process on, how-

ever, the lock was just exactly where the rest were. It was opened in precisely the same way, and although the lock was changeable it was considered almost worthless.

In speaking about how these locks can be opened, it should be said that there is a great deal of difference in a man's taking a lock at his own workshop or house and opening it, as compared with the difficulty of operating on it in place. I have had a great deal of experience in this latter way when going about to different bankers, and yet I always found the best chance I had of selling a lock was when I could find a man who had full confidence in his locks. I went one morning to a bank in Smyrna, Delaware, and told them what I wanted. The cashier said, "Mr. Hobbs, we have had a meeting a few days ago in regard to our locks. We have examined them thoroughly, and do not want to purchase a lock. I am very busy this morning. I should be glad to see you some other time and talk to you, but I do not want to purchase a lock." There was another gentleman in the bank, who was second in authority, who said to me, "What kind of a lock have you got?" And I was very glad to take out one of my locks and show it to him. I saw exactly what locks he had on his doors; he had on the warded locks, and in talking to this gentleman I spoke very strongly about that class of lock, not being supposed to know that he had that kind on the bank. (!) The cashier I saw looking up occasionally from his book, and finally said, "What did you say about those warded locks?" I said, "Nothing particular. I was talking about locks I found in some places here." He said, "The lock you are speaking against is my lock." I said, "You do not mean to tell me you are secured by locks of that kind?" He said, "Let me see you try one of them." I went around, and about as quick as I can tell it to you the door was opened.

I want to tell you about the lock shown here in Fig. 84. It is the lock made by Robert Newell, of the firm of Day & Newell. I think that Mr. Yale and Mr. Newell were the two experts in making locks, as I was in picking them. They made one general mistake, and that was in adding obstructions. While they did not change the principle of the lock, they kept adding something to it. Mr. Newell found that his first lock could be opened by the tentative process. He went to work to make it impossible to do this. Then there was another objection to previous locks which I shall describe presently, allowing us to ascertain by the motion of the bolt whether we were making any progress, and he took pains to

cover that up. He understood there was some means by which we could go inside a lock and almost map it out, and he took pains to obviate that. He thought he had made an almost perfect lock, and

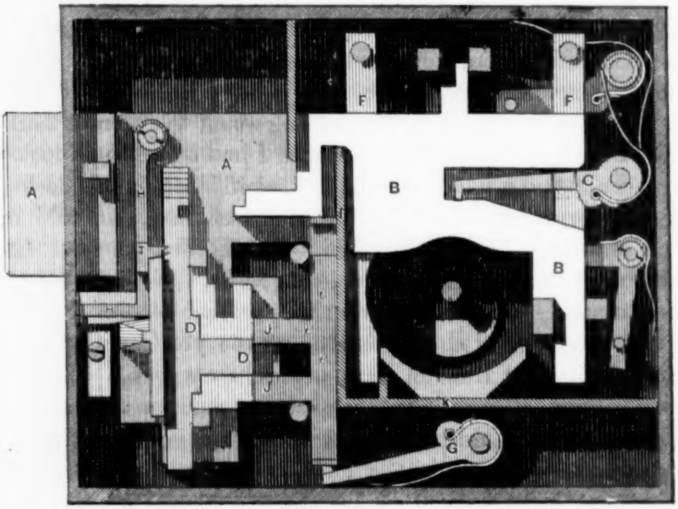


FIG. 84.

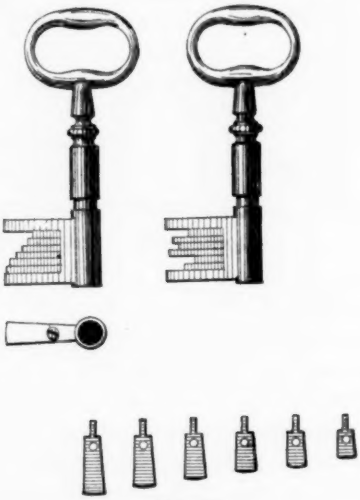


FIG. 85.

I thought so too. There were partitions in the lock; and arrangements are made so that, as the tumblers are raised by the key and the bolt is thrown, the tumblers are hooked together in place. The

piece which is to slide and lock the tumblers is raised by the spring shown in the cut. After the key is withdrawn, your tumblers come down to a perfect level, and you leave the form of the key on the outer parts of the tumblers, and if you apply pressure to the bolt for the purpose of pulling it back, or if you move this tumbler, the tumbler moves away independently, and leaves the contact with the bolt, and there is no possible means of feeling what the contact is. The idea of opening that lock by the tentative process is knocked out entirely. Everything was guarded against thoroughly. There was no way known of picking that lock at the time. There was a man by the name of Woodbridge, in Perth Amboy, in 1849. He knew all about these locks, but he wanted to make one that would be cheaper and better. So he made his lock with a series of tumblers similar to that; and it was so arranged that in the locking the tumblers were carried up, and the key, instead of taking hold of the bolt to throw it back, touched a tang on it, and if an attempt was made to withdraw that bolt before the tumblers were in an exact position, whatever you had in the key-hole then was caught, and you could not recover it. I was in Lancaster, Pennsylvania, where I had been putting locks on a bank, and one morning the cashier came in and said, "Mr. Hobbs, there is something for you." He showed me a New York paper which said that Mr. Woodbridge had put his lock in the Merchants' Exchange, and offered $500 to any one who could open it. I said, "That is my money." I went to New York and found that this reward was offered to open the lock which had been put on one of Herring's safes in the Merchants' Exchange. So confident was Mr. Woodbridge that nobody could open the lock, there being over a hundred million changes, that he offered this reward of $500 to any one who would open the lock in thirty days, and he could have the use of the key for one dollar an hour. I wrote a certificate, to put in the safe, saying I had had thirty days to open the safe, and could not do so in that time, and therefore I thought it a safe lock to purchase. He put in a check of his father's for $500, and the safe was locked up. "Now," I said, "Mr. Woodbridge, you do not own that money, it is a check of your father's. I do not like to take the money under these circumstances. Open your safe, take your check out and give me my certificate, and call it square." He said, "Go ahead." I got permission from the janitor to use the room at night. In the evening I had a man to bring pieces of plank there, as the key-hole was too low to work at. You all understand that if you put anything on a

plank and rock it, you can raise it with little help. Two gentlemen were standing behind me, and one said to the other, "I will bet a hundred dollars he opens that lock." "Why," said the other. "Don't you see the way he is raising that safe," was the reply. I had a key made with a channel deep enough to take a watch spring, and by putting that in the lock and setting it from tumbler to tumbler, I could run those watch springs up between the different tumblers. The first one that I ran up had a notch in the end, and I ran it up through until it struck the corner of the stump; then having a plate with a screw on it I got the angle, and it told me just exactly how much higher the obstruction was. I went to work then, after measuring and getting the measurements set down, and I then changed my watch-spring, and tried one with a little piece brazed on the end. I measured in this way all the proportions of the fourteen tumblers with all the accuracy I could. I got the exact lengths required for the bits, as closely as I could without the key. I did not take the key at night, or they would have charged for the twelve hours. I took the bolt bit off the key, so that if I had possibly made a false move I could get the key out; I turned the key around, which adjusted the tumblers, and I threw the bolt back with a piece of bent wire so that the door would have opened, and left the stump hanging in the gatings. I went up to Mr. Woodbridge's house and told him there was something the matter with his lock, and would he come to the Exchange at ten o'clock? I notified the three arbitrators to be there at ten o'clock. I had left the crooked wire in the keyhole. and it looked just exactly as though I had got it caught there. There was quite a crowd around the safe. Mr. Woodbridge came seeming very much pleased, and said, "What is the trouble?" I said, "There is something the matter with your lock." He said, "What is it?" I said, "It will not keep your door shut," and I drew it back. That lock had, in addition to what appears in the cut, one or two additional safeguards, so that every single point that was known in that lock was guarded.

There was one step that had been made in regard to picking these locks. After they knew about this tentative process, they made their locks so that you couldn't feel the tumblers at the same time you were trying to pull the bolt back. They got them fully guarded, so that this instrument was of no use, and the way that we tried to operate then was this: If you take hold of the bolt, supposing the lock has ten tumblers, and you put a given pressure on it, if you can

move any one of those tumblers which are resisting the pressure when you release it from the stump, the bolt will go back slightly, and however slight the motion may be, you can detect it with a good micrometrical measure—if it moves a thousandth of an inch—and you know you are making progress. Suppose that the key bits were all cut off, leaving only the bolt bit on. I have a series of wires that will screw into the shank. The micrometer is set so that when you get to the point at which it

BRAMAH-LOCK.

Fig. 78.

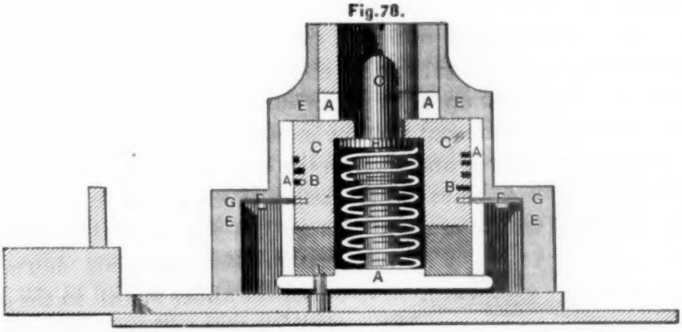


Fig. 79.

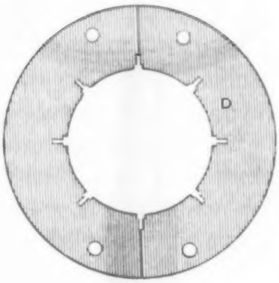
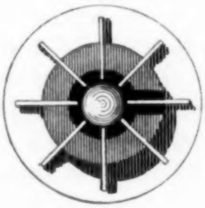


Fig. 81.



touches you are made aware of the least motion, so that going to one after another, noticing wherever you get any motion, you find out all about the lock and can open it. Suppose I begin on the outer tumbler. I move it away up; I go to the next, and the next, working backward and forward so that I can make a perfect key, and however secure the lock may have been against everything else, that process throws it out. I discovered that process about 1859 or 1860. I came back to New York in 1860, and I found that Mr. Linus Yale had about the same time discovered the same

thing, and he had altered many locks that he had made, to protect them against this danger.

I went to England in 1851, having learned that Bramah had a lock in his window, and had offered 200 guineas to any one who would open it. I went out there with the determination to open that lock. It was a large padlock hung up in the window, and I read the challenge. But I waited. I was a little nervous and didn't want to take hold of it too soon. Finally I went in one day and asked the man what it was. He said, "It is a lock." He said, "Are you a lockmaker?" I said, "No, I never made a lock, but I would like to see it." He took it out of the window, and I took my penknife and began to feel of the slides, C. Fig. 78, 79, and 81 will show you a sectional and an end view of the lock with the cap taken off. It shows an inner cylinder and an outer cylinder. The inner cylinder, as you put the key in, would press a disc down so as to relieve that spring, and then as the key came in straddling over those slides, the notches would bring those slides in position to pass the plate which you see. There is a nib on the key (Fig. 80) that presses that slide down, and when the slides are all in the right position that will turn the cylinder around and throw the lock back. The lock on which I operated gave me only three thirty-seconds of an inch between the drill-pin and the side. The force of the spring under that disc was fourteen pounds. This is shown with false notches. But in order to have those false notches effective, there are little notches cut out in the plate, so that when these false notches come around, it would slide and catch there. Now, in order to open that lock I had to operate in a space of one sixteenth of an inch. I had to take up one thirty-second to get a tube that would go down over this pin and press this disc out of the way. I went to the lock with the expectation that under each one of those slides was a spring. But I was mistaken. There were no

Fig. 80.

springs, and I had to change my tactics. I got a pair of pliers and worked them down until they could go into this hole here. I had a piece bent over so that I could work around that, and by putting the strain on the cylinder and bringing these slides into contact with the disc, I followed them up one by one, but I couldn't do it all in one afternoon. I took little bits of brass that I filed, showing the position of the slides 1, 2, 3, 4, 5, and 6. I took my instruments out and went back the next morning to start again. By the way, though it was a padlock, it was secured to a door. I had a little bit of iron with a little thumb-screw which I fastened to the door, and I used that to keep that disc down. The third time I went to the lock I succeeded in getting the slides all right, and turning it around partly unlocked it, but it stuck. That thumb-screw bore on the pin, and the turning of the pin in the way which the screw should turn to release it had released the screw. I went and thought over the difficulty, because what had made the trouble was that in doing this much, the part which I took hold of to get my pressure had slid around and got under the cap, and I had to contrive some other way to get the pressure on. I drilled a hole so that it came in about the right portion of the lock. I got a little bit of steel wire and screwed it in. I put a wedge in, and so I got the pressure and succeeded in opening the lock. The conditions were that in opening that lock, the lock should be left in perfect order, so that it could be locked and unlocked with the key. After having opened it, and fearing they might make some complaint if they found that little hole there, I got a little piece of brass wire, tapped the hole, and fitted the wire in, and after working it down I put a little sulphuric acid on, and I had an old place there instead of a new hole. The arbitrators came, and in their presence I locked and unlocked it three times in twenty minutes. After doing that, Mr. Bramah made a protest and said the lock was not opened according to the challenge. By the way, I forgot one thing that helped me along very much indeed. It so happened that the London *Times* had an article on the Exhibition describing a case of Hope's jewels. It said, "The case is locked with one of Chubb's locks, but if we understand rightly the American gentleman throws down the gauntlet and offers to open both Chubb's and Bramah's locks. That brought out both Chubb and Bramah, and Bramah said if the American gentleman will pick the lock, he shall receive the 200 guineas reward." The original challenge read thus: "The artist that will produce an instrument that will open

this lock shall receive 200 guineas reward the instant it is produced." Bramah said that I had not opened the lock according to the challenge. I had used instruments and not an instrument. I had the *Times* in my pocket. I produced it and said there was my answer to his argument. The arbitrators said I was entitled to the money.

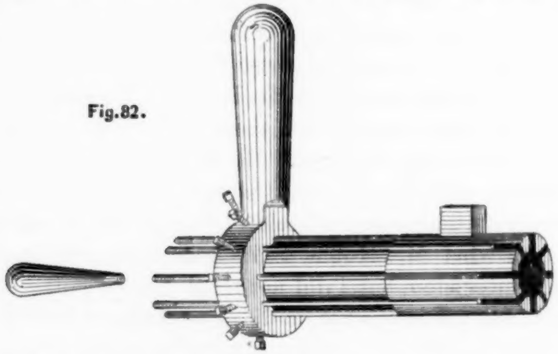
Early in the Exhibition I met a great many gentlemen interested in locks. Mr. Hensman, Chief Engineer of the Machinery Department of the Exhibition, said to me one day, "Mr. Hobbs, how is it that you can open these locks?" "Well," I said, "the best way for me to give you an idea of it, is to get a lock and show you." I went to Chubb's and bought a lock, paying ten shillings for it, and after having it in my possession some few days I went down to Mr. Hensman's room and said, "I have come to show you the structure of this lock, and upon what principle it can be opened." He said, "Wait a moment." He went out and came back presently with Robert Stephenson, Sir Humphrey Dilke, Roberts of Manchester, and some other gentlemen, and said, "Now, Mr. Hobbs, let us see that thing." I said, "No; I came here to show you the principle on which that lock could be picked; I did not come to make an exhibition, and I shall not do it." I said, "If you will have this understanding, that I do not make this as an exhibition of lock-picking, I will go ahead." "Very well," said they; that was done. I took the tumblers out of the lock one by one, showed them the whole structure, put it together, and then screwed it in a vise. I took my little instrument out of my pocket, and I locked and unlocked the lock several times in their presence. About a fortnight after that a paper was read before the Institute of Engineers of Birmingham, at the Society of Arts in London. Mr. Chubb and I were there. A gentleman from Wolverhampton, before that, came to see me and said, "Mr. Chubb is making a lock at Wolverhampton, and he is going to try and catch you." I said, "I am obliged to you, and I shall be very careful." As I said, Mr. Chubb was at this meeting, and he had one or two locks lying on the table. Among the locks was this one that I had at the exhibition. Mr. Hodge read a paper, and said something about the ease with which Chubb's locks could be picked. He sat down, and Mr. Chubb got up and said he had been very much abused. He said the "American gentleman has undertaken to pick one of my locks. He came to my store and bought the lock and had it in his possession eight days. Let me have one of his locks in my possession eight days

and I will make it safe to be picked." It was "hear, hear," all around. I was in very bad repute for a few minutes. Chubb went on in this way: "Gentlemen, I have brought with me a lock, and it lies on the table. If you will appoint a committee of gentlemen to take that lock and put it on any door, anywhere you please, and if Mr. Hobbs, or anybody else, can pick that lock, I will acknowledge that any lock I have made can be picked." It was "hear, hear," again. After Mr. Chubb got through I requested permission to make a few remarks. I told them that what Mr. Chubb had said about my buying the lock and having it in my possession eight days was true, every word of it. "What he said as to my having done something to the lock which made it easy to be picked—that may be true, but the lock lies on the table; he can examine it and see if it is made safe to be picked. Mr. Chubb says there is a lock lying on the table, and if the American gentleman, or anybody else, will open it, he will acknowledge that any lock he ever made can be picked. That lock has been made in Wolverhampton within three weeks, and it is made as Mr. Chubb never made a lock before. It is made for a trap to catch me. Now if Mr. Chubb will name any lock in the United Kingdom that he ever made and sold, I will invite any party of gentlemen, including Mr. Chubb, to go with me; if I do not open it I will pay the expenses, and if I do Mr. Chubb shall pay them." It was "hear, hear," on my side then. I went on, "there is a party of gentlemen here who saw the operation of opening the lock at Mr. Hensman's room, and I will call on them to tell what was said and done." Mr. Hensman was the first to get up, and he told the story. He said, "It is a simple mechanical operation; anybody can do it that understands it." Mr. Appold was an amateur mechanic; he said, "Gentlemen, I was very much pleased with what Mr. Hobbs said he was going to do, and I felt very much interested, but I did not think he was going to open the lock. But when I saw what he did, I understood it so well, that I went home and made an instrument with which I opened every one of the twelve Chubb locks which I have in my house." Then they began to say "hear, hear," for me. Mr. Stephenson made some remarks, and then Mr. Roberts, of Manchester, who was an old Scotchman—very emphatic and very sure about everything he said—got up and said, "When Mr. Hensman came to me and told me he wanted me to go and see Mr. Hobbs pick one of Mr. Chubb's locks, I went with him, but I did not believe he could do it. But when he took that lock to pieces and explained the parts, one after

another, I began to feel a little doubtful; but when he took that little thing out of his pocket and held it up, and told me that that was what he was going to open it with, I said he could do it—he did do it—and anybody could do it." There was a dinner given by the Institute of Civil Engineers that evening at the Freemasons' Tavern. It is a custom there, that if a gentleman comes to a dinner he finds his card on his plate. There was a plate with A. C. Hobbs' name on it, and one with Chubb's name. But the latter plate was not used that evening. He did not come.

Some months after I got through with the picking of the Bramah lock, I made the remark that I expected to find springs under the slides. They thought the matter over, and made up their minds that if they had springs under the slides it could not be opened. So they changed the lock, and had one put on a bank at Cheapside. I went in there one day and said, "I am very sorry you have got this lock. The manager of the bank said, "Do you think you can open it?" I said, "I think so." I took the measure of the lock. It was a lock with eight tumblers, and it had a spring under each slide. The key was the same as before, with steps in, of different lengths. I went to work and made an adjustable key, like that shown in Fig. 82. Those slots are all so deep that when you

Fig. 82.



put that into the lock it presses the discs down and does not touch the slides. I then loosened those screws and the wires dropped down on to the slides. I took hold of the handle and put a pressure on this cylinder, the same as before, and the spring resting against the slides; if I pushed it down the spring would push it up again, if it did not bear against this plate; and so I worked them down one at a time, and screwed the wires fast. After I had got the key formed in the lock, I called the cashier and said, "See if you can

open it with this thing." He put it in and locked and unlocked it just as well as he could with his key. So that by putting the springs in to make it more secure, he made it so that one instrument would answer to open it.

After it was proved thoroughly that the Bramah lock could be picked, a man by the name of Cotterill made a lock (Fig. 77). The

COTTERILL'S-LOCK.

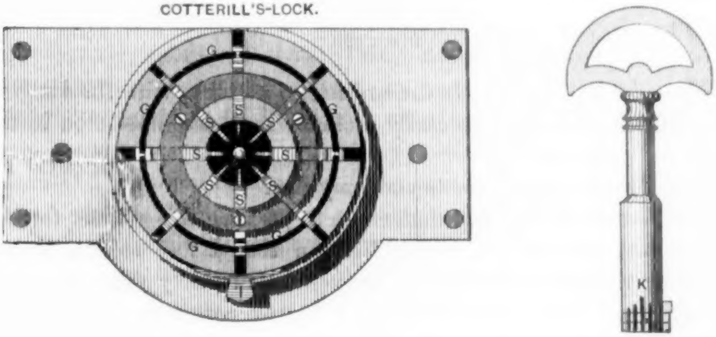


Fig. 77.

end of the key, instead of having slots, had notches in it, and was made as shown in the cut. The slides radiate from the center, instead of up and down. You put that in and it opens just the same as with the other. It leaves it just about in that position.

As I said, at the beginning the greatest possible security that can be got in a lock is to make it so that a man cannot, by any possible means, begin at zero and work up. If he has got to run the chances of raising these tumblers, all right—it is impossible to do it. When you get up to eight slides, or movable pieces, there are 40,320 combinations against you, as shown in the accompanying table; and if you get up to twelve, you have 479,001,600.

TABLE OF COMBINATIONS.

1 × 1	9 × 362,880
2 × 2	10 × 3,628,800
3 × 6	11 × 39,916,800
4 × 24	12 × 479,001,600
5 × 120	13 × 6,227,020,800
6 × 720	14 × 87,171,291,200
7 × 5,040	15 × 1,307,674,368,000
8 × 40,320	

Mr. Newell made many changes. Mr. Yale made many changes. I suppose some of these days we shall have an interesting paper on

those modern locks, and how the changes have been made. The locks shown on my diagrams were all made prior to 1851, and for that reason, perhaps, I might have felt more at home in presenting these matters before an antiquarian society; but, such being the case, it leaves room for such members of this Society as are familiar with modern locks to continue the subject at some future meeting.

DISCUSSION.

Mr. Oberlin Smith.—Is the ordinary safe lock, with the knob in the centre of the lock, as easily pickable as those illustrated in the paper?

Mr. Hobbs.—That goes beyond my day.

Mr. Towne.—I will not undertake to prolong this subject farther than to express the personal obligation that I feel to Mr. Hobbs for so interesting a description of events that thirty years ago were looked upon with interest, literally by two continents. England and America were then both keenly alive to the work being done at the exhibition, and Mr. Hobbs was the foremost and central figure in that work. On another occasion, when our time is less short than it is this afternoon, I should be glad to say a few words in the same direction.

I want to say, without attempting to answer Mr. Smith's question, that the progress of the art of lock-making since the time of which Mr. Hobbs has been speaking, has been in a diametrically opposite direction from what he has shown us. In most of the mechanic arts the process of development is one of building up. We take what has been done before and go on to improve and build on that as a foundation; but it is almost literally true, that in the art of lock-making the things that stand to-day are not based upon what is illustrated in the paper before us, but are absolutely and entirely new. All the past has been wiped out, and a new method of work has grown up since then. That new method has one great principle of difference from the old. As Mr. Hobbs has told us, the effort in the old locks, in order to guard against picking, was to keep adding on. A defect would be developed, and some addition would be made to the lock to meet that defect. Another defect would be discovered, and another addition would be made to meet it, until at length the locks took the complex form you see in the Newell lock. The old Treasury lock of Mr. Yale was even more complicated than that. To-day, on the contrary, absolute simplicity

is the chief merit of all locks, and is the basis of their security. Literally, where there is one piece in the locks of to-day, you would find probably ten in such locks as Mr. Hobbs has been telling us about. The direction of growth has certainly been a desirable one —the reduction of a given mechanical problem to its simplest possible terms.

In answer to Mr. Smith's question, let me say, that the combination locks of to-day, operated by a dial from the outside—any of the reasonably modern locks of that kind, of good repute and well made—are practically unpickable. Locks of that construction have been picked, and also locks which, up to the time of their picking, were thought absolutely invulnerable; but the method of picking them was a modified application of the principles Mr. Hobbs has explained, and the time of their picking was soon after this period Mr. Hobbs has told us about. The application of that method to those forms of locks is now well understood and perfectly guarded against; and any good dial lock of modern construction is practically proof against picking.

CLXVIII.

REPORT OF COMMITTEE ON A STANDARD METHOD OF STEAM-BOILER TRIALS.

To the American Society of Mechanical Engineers.

GENTLEMEN:

Your Committee, to whom was intrusted the consideration of the subject of Standard Methods of Testing of Steam-boilers, and the duty of preparing a Code of Regulations for such tests, have the honor to present the following report:—

1. The importance of establishing a method of trial of steam-boilers that should determine their steaming capacity under any given set of conditions, and their economy in the use of fuel, is so thoroughly understood and so definitely recognized by engineers engaged in the design and construction, or management and use of them, that it has been thought, by all, that some system of testing should be settled upon, for general use, which may be relied upon to give all the facts needed in relation to the performance of boilers, with substantial accuracy, and yet with least possible expenditure of time and money, and a method which may be adopted by any fairly skilful engineer, without the use, so far as it can be avoided, of unusual forms of apparatus.

It has been the duty of your committee to examine carefully the methods of testing boilers now practised, to consider to what extent they present advantages or disadvantages, and finally to frame a Code of Instructions, embodying what they consider to be the best methods of experiment and the most satisfactory plan of working up and stating results. In this labor they have met with all the difficulties which usually attend an attempt to reconcile the opposing views of those who are acknowledged to be authorities on the subject, and to combine the various advantages possessed by systems in use among such members of the profession. Their object has been, not to prescribe a regulation method of test that shall be considered as representing the most complete possible system, and as giving results exact to the degree that would be satisfactory in purely scientific work, but to propose a code for daily use by the practising engineer which may be relied upon for

substantial accuracy to limits of error within the range of commercial requirements, one that may be adopted by any engineer deserving of a place within the ranks of the profession, and one that may be followed closely under ordinary circumstances of every-day experience.

It has, however, also been attempted to present, independently, a view of the refinements of recent practice in this matter which may be of service to the engineer who finds it desirable and possible to attempt work of scientific exactness, and of the utmost possible completeness.

2. The object of a trial of a steam-boiler, as your committee understands it, is to determine with great precision what is the quantity of steam that a boiler can supply continuously and regularly under definitely prescribed conditions; what is the condition, and therefore the commercial value, of that steam; what is the amount of fuel demanded to produce that steam-supply; what is the character of the combustion, and what are the actual conditions of operation of the boiler when at work, all of which should be presented in a report stating the results thus determined. The conditions prescribed for one trial may differ greatly from those demanded for another trial of the same, or of another boiler, and those differences of circumstances are often the essential matters to be studied, and their effect noted upon the performance of the boiler which is the subject of the report. In any case, however, it is assumed that the conditions under which the boiler is to be worked are to be definitely stated, and the engineer conducting the experiments is expected to ascertain as exactly as possible the facts which go to determine the performance of the boiler, and to state them with accuracy, conciseness, and thoroughness.

In the attempt to ascertain those facts by observation of the actual performance of the boiler, the engineer meets with some serious difficulties, and finds it necessary to use the most perfect apparatus, and to exercise the utmost care and skill. In even so simple a matter as the weighing of coal and the measurement of water, errors are often found where least expected, and they may make their appearance even in the work of painstaking and experienced practitioners. In conducting a steam-boiler trial, the weight of the water supplied to the boiler must be exactly determined; the weight of the fuel consumed must be similarly obtained; the state of the steam made must be determined, and those quantities must be noted at such frequent intervals, during

the test, that the log will exhibit every irregularity of operation, and its effect upon the performance of the apparatus. To secure thoroughly satisfactory results, it is also necessary to know whether the combustion is perfect or imperfect, and to what extent the character of the combustion, as well as the other conditions and facts noted, are due to the excellences or the defects of the boiler, and what to external conditions.

3. In the tests of boilers made in earlier times, these determinations were made with comparative crudeness of method, and the results of such methods were such as would be considered to-day grossly inaccurate. The coal consumed was in large part estimated, and no pains were taken to ascertain the amount of unevaporated water carried over with the steam. It thus often happened that results were reported that were far beyond the utmost possible efficiency; the evaporation of water was sometimes reported at a higher figure than theoretical perfection would yield; and it has only been within a very recent period that it has been possible to judge what is the real performance of the standard types of steam-boiler, under ordinary circumstances, from the reports published, in many cases, as the work of engineers of reputation.

A great change has been gradually taking place both in the sentiments and in the practice of engineers engaged in this department of professional work, and it has come to be considered that the exact determination of power and economy of a steam-boiler demands the exercise of all the care, skill and perfection of method, and of apparatus, required in the prosecution of any purely scientific investigation. It is now demanded that the weights of fuel and of water, the perfection of the combustion, the quality of the steam, and the temperatures of feed water and of furnace flue, shall be determined with an accuracy that shall be within the limits of error of good instruments; that, wherever possible, a system shall be adopted which shall permit of checking and verification of the reported results, and which shall make it as nearly as possible certain that no error can enter the work without prompt detection and correction. It is further demanded that all important work of this kind shall be done in substantially the same way, in order that comparisons may be easily made without the necessity of going through long and troublesome calculations in the effort to reduce the reports to be compared to a common basis.

4. This sentiment, and these demands, can evidently be complied with only by the establishment of some standard unit of measure

of the power of the boiler, and of evaporative efficiency, and some definite and standard method of conducting the test. This standard unit of measure must be simple, easily defined, and convenient in application; the standard method of trial of boilers must be prescribed by a code of rules so concise and yet so definite that every member of the profession may be able to adopt them. The scheme must also be so complete that, if carefully and exactly followed, the precise value of the boiler may be ascertained with certainty. The method of record of facts determined must be such as will exhibit all the essential quantities in tabular form, and unobscured by the introduction of unessential figures.

5. Such a code of rules has been proposed by a joint committee of the Union of German Engineers and of the Central Union of Associations for the Care of Steam Boilers, and this set of regulations may be considered as the embodiment of the best ideas of our Continental colleagues on this subject. Your committee have examined this document with care, and find themselves in full accord with its proposers in the main, while obliged to offer some modifications of the scheme which are thought to make it more effective and more acceptable to American engineers. The Code of Rules for Use in Trials of Steam-Boilers which your committee proposes is herewith submitted and will be found appended to this report.

6. The first provision of the code is that the object of the test to be made shall be precisely stated, and carefully kept in view during the whole trial, and during the preparation of the report. This object may be the determination of the steaming capacity, of the maximum efficiency, or of the quality of steam supplied by the boiler under specified conditions; or it may be the comparison of the qualities of various fuels. These objects cannot all be attained at one time, and maximum steaming capacity and maximum economy of fuel are, almost invariably, if not always, the result of incompatible conditions. The method of handling the steam generator will therefore differ as one or the other of these objects is to be sought.

It is next provided that the boiler to be tested shall be exactly measured, in order that data may be obtained for subsequent calculations. These measurements should be taken before the trial, not only because that is usually the most convenient time, but also because this preliminary measurement may sometimes lead to the discovery of defects of construction, as well as of proportions, that may suggest modifications of the plan of test previously laid down.

The boiler is then to be put in the best possible order, in every respect, so that its observed merits or defects may not be obscured by accidental conditions having no relation to such merits or defects.

7. It is provided that an understanding shall be reached, before the trial, in regard to the kind of fuel to be used. Neglect of this precaution sometimes leads to needless misunderstandings, and avoidable criticism of the results reported. It is proposed that, where no reason of controlling importance exists to the contrary, the best obtainable coal shall be selected, for the reason that it is thought that a boiler can be better judged, and the results of its trial may be more satisfactorily compared with similar trials of other boilers when the very best work of which it is capable is done by it. The differences between separate lots of the best coals are less than the differences between separate lots of inferior fuels, and the comparison is thus less difficult where the former are used. To secure still more exact knowledge of the influence of the quality of the fuel upon the performance of the boiler, it is considered advisable to have an analysis made of the coal used in all cases in which it can be done.

8. The establishment of the correctness of all the apparatus to be employed in the test is the first of the preliminaries to their use. The standardization of the instruments is a matter of supreme importance, since upon their accuracy the whole work of the engineer is dependent. It is also a work demanding, in most cases, unusual skill and care, and, to be satisfactory, must generally be performed either at the manufacturer's or at the office of the engineer conducting the trial. The scales can usually be standardized by the official sealer of weights and measures, and sealed by him; the water meters, if used, can be readily tested by the use of the scales so sealed; the thermometers are, as a rule, best tested by their makers, and should be sent to the maker for test immediately before and directly after the test. The engineer often has a carefully preserved standard with which they may be compared in his own office. The same remarks apply to the examination of the gauges used, which should be standardized both before and after their use. The apparatus used in connection with the calorimeter, in the determination of the quality of the steam made, demand exceptional care in this process; they are rarely of sufficient delicacy and accuracy to give perfectly satisfactory single determinations, even at the best, and the use of ordinary commercial instruments, carelessly standardized, or not at all, cannot be too strongly depre-

cated. Where it is unavoidable, the use of coarsely graduated thermometers and roughly constructed scales may be permitted, but only then when a very large number of observations are taken, and an average thus obtained which may be fairly expected to fall within reasonable limits of error—say within one per cent.

9. The precautions to be taken before beginning a trial are prescribed in some detail, since your committee consider them of great importance, and have known of serious embarrassment arising by their neglect.

The method of starting and of stopping the trial is prescribed in a form which seems to your committee best as a whole. This is a very important matter, and yet is one upon which engineers of experience and acknowledged authority are not in complete accord. Your committee, for this, and also for the other reason, that the plan here proposed may not be always practicable, prescribe a second or alternative method, which may be adopted for such cases, or, where the engineer conducting the test is confident of being able to do better work than by the first of the two methods. The principles to be adhered to in this matter, as in every other detail of the operation of testing a boiler, are easily specified, but they are not always as easy of practice. All conditions should be as exactly the same at the beginning and at the end of the test as they can possibly be made. The period of the trial, and the times of stopping and of starting, should be capable of being exactly fixed, and the method of test should be such as should permit of the commencement and the end occurring at these exactly defined times, or, as an alternative, they should be such that the work done by the boiler during the less precisely determinable time of beginning and ending of the trial should be as nearly as possible *nil*, so that a slight error as to time may not appreciably affect the results. The "Standard Method" proposed by your committee is considered to meet these requirements as fully as any method in use. The alternative method is regarded as the next best.

10. During the trial, the essential provision should be the preservation of the utmost possible uniformity of working conditions throughout the whole period of the trial. Every irregularity gives rise to more or less loss of efficiency and to uncertainty in regard to the correctness of the reported figures. The nearer the working of the boiler is kept to the final average for the trial, the better.

11. Your committee consider the method of keeping the record of the test as no less important than the method of test itself.

Perfect uniformity of operation within the boiler-room, and maximum efficiency of boiler, are best attainable where a system of record is adopted which allows of that regularity being shown at all times; and records in proper form are the best possible security against error of observation. The committee are unanimous in recommending that graphical methods be adopted wherever it is found practicable to employ them. Such methods of record also exhibit most satisfactorily the accordance with or the deviation from the uniformity of operation considered so desirable on the score of efficiency and accuracy. Your committee present a form of record blank which they consider as concise as is ever desirable in any important trial; and would prefer, in special cases, a more, rather than a less, complete record.

12. It is proposed by your committee as desirable that, when practicable, analyses of the escaping gases should be made. This is an operation of great simplicity, and can easily be made familiar to any engineer who chooses to take the trouble of learning it. If, for any reason, it is not found convenient to make the analysis in the office of the engineer, he can readily have the work done, at little expense, by intrusting his samples to a chemist of known skill and reliability. This provision is made as a part of the code, on the ground that it is only by a knowledge of the proportion of constituents of the flue-gases that it can be determined whether the combustion is complete, whether the products of combustion are diluted with excess of air, and whether the fuel used has been so burned as to give its best effect. Such analyses also enable the engineer to ascertain the best method of burning the fuel. The code prescribes the precautions to be taken when this detail is carried into effect.

13. The establishment of the value of the "Unit of Evaporation," and that of the "Commercial Horse-power" of the boiler, are matters which have been considered by your committee to be of essential importance to the settlement of a thoroughly complete standard method of trial, and of a perfectly satisfactory system of reporting results.

It has been evident to every observer that the sentiment above alluded to, as having arisen among engineers during the present generation, in favor of reducing the whole matter of testing boilers to an acknowledged standard system, has led to the endeavor, on the part of the most able among practitioners, to determine standards with which to compare results obtained in such trials. The

two most essential standards are those just referred to. The trials of boilers are made under a wide range of actual conditions, the steam pressure, the temperature of feed-water, the rate of combustion and of evaporation, and, in fact, every other variable condition, differing in any two trials to such an extent that direct comparison of the totals obtained, as a matter of information relating to the relative value of the boilers, or of the fuel used, becomes out of the question. It has thus gradually come to be the custom to reduce all results to the common standard of weight of water evaporated by the unit weight of fuel, the evaporation being considered to have taken place at mean atmospheric pressure, and at the temperature due that pressure, the feed-water being also assumed to have been supplied at that temperature. This is, in technical language, said to be the "equivalent evaporation from and at the boiling point" (212° Fahr.). This standard has now become so generally and so indisputably incorporated into the science and the practice of steam engineering that your committee, even were they acquainted with any other equally satisfactory unit, would hesitate to recommend anything else. They would simply express their approval of the adoption, and recommend the permanent retention of this, which, as has been previously proposed, they would denominate the "*Unit of Evaporation*," *i. e.*, one pound of water at 212° F. evaporated into steam of the same temperature. This is equivalent to the utilization of 965.7 British thermal units per pound of water so evaporated. The relative economy of the boiler would then, as is customary, be expressed by the number of units of evaporation obtained per pound of combustible.

14. The character and magnitude of the unit to be chosen to express the "power" of the steam-boiler is not as well settled; and your committee find themselves compelled to take up, in this matter, a subject which has attracted much attention among engineers, and which remains, nevertheless, unsettled. It is evident that, since the boiler is simply an apparatus for the generation of steam, and since the province of the steam-engine is to develop power from that steam, by the conversion of heat into mechanical energy; and since, furthermore, the engine develops power with a degree of efficiency which may vary enormously with differences in construction and operation of that machine, it cannot be properly said that we have any natural unit of power for rating steam-boilers. The most nearly scientific system of power rating yet proposed is, perhaps, that which considers the power of a boiler to be that ex-

pended by it in driving all the steam which it makes out against the pressure of the atmosphere, a system which does not, however, meet the wants of engineers. What is needed is a standard unit of boiler-power which may be used commercially in rating boilers, and in specifications prescribing the power to be demanded by the purchaser and guaranteed by the vender. It is evident that such a unit would not, if established, serve as a gauge of the power to be actually obtained from any given combination of engine and boiler, since the power so obtained must be measured by the indicator at the engine, and not at the boiler, and since in so measuring power, the economy and efficiency of the boiler would be elements left entirely out of the account. The best that can be done is obviously to assume a set of practically attainable conditions under which it would be fair to assume that the boiler may be properly expected to be operated in average good practice, and to take the power so obtainable as the measure of its power to be used in commercial and engineering transactions. The unit which has been most generally assumed, up to the present time, is the weight of steam demanded per horse-power per hour by a fairly good steam-engine. The magnitude of this quantity has been gradually and constantly decreasing from the earliest period of the history of the steam-engine. In the time of Watt, one cubic foot of water per hour per horse-power was thought a fair allowance; at the middle of the present century, ten pounds of coal was still not an unusual figure for the consumption per hour per horse-power, and five pounds, equivalent to about forty pounds of feed-water, was a good allowance for the best engines. After the introduction of the modern forms of expansively working engines, this last figure was reduced twenty-five per cent., and the most recent improvements have still further lessened the consumption of fuel and of steam. By general consent, it seems likely that the unit which will meet with final acceptance for general purposes, in the estimation of boiler-power, is not far from thirty pounds of dry steam per horse-power per hour. This represents the performance of good mill engines of the non-condensing type. Large engines, with condensers, or compounded cylinders, will do better by from twenty to thirty per cent. Your committee have concluded to recommend thirty pounds as the unit of boiler-power.

15. But it remains to be determined under what circumstances this figure shall be taken as standard. It is on this subject that practitioners, and the members of your committee as well, are not

fully agreed. Nevertheless it is, in their opinion, advisable that some definite set of conditions be prescribed to be taken as standard without waiting for complete accordance of opinion throughout the profession.

The Committee of Judges of the Centennial Exhibition, to whom the trials of competing boilers at that exhibition were intrusted, met with this same problem, and finally agreed to solve it, at least so far as the work of that committee was concerned, by the adoption of the unit, 30 *pounds of water evaporated into dry steam per hour from feed-water at* 100° *Fahrenheit, and under a pressure of seventy pounds per square inch above the atmosphere*, these conditions being considered by them to represent fairly average practice. The quantity of heat demanded to evaporate a pound of water under these conditions is 1110.2 British thermal units, or 1.1496 units of evaporation (such as are here adopted and proposed for general use). The unit of power proposed is thus equivalent to the development of 33,305 heat-units per hour, or 34.488 units of evaporation. The arguments in favor of the retention of this unit of power without modification are: (1) It is, to a certain extent, established, being the only unit proposed by authority, up to the present time, which has been accepted to any important extent by practitioners; (2) It is considered by its proposers, and probably by engineers generally, fairly to represent good average practice in the application of steam-power, as exhibited in the operation of engines and boilers under ordinary actual working conditions. Both of these arguments are deemed by your committee to be valid and deserving of careful consideration. The abandonment of an already established standard is always confusing, and should not be permitted without the most cogent of reasons.

Another standard unit, which has been proposed to your committee, and strongly urged as preferable to the above, is that represented by the evaporation of thirty pounds of feed-water into dry steam "*from and at the boiling point*," at mean atmospheric pressure (212° F.) The arguments in favor of this unit are the following: (1) In the determination of the unit of evaporation to be used in steam-boiler practice, it has been generally, and probably unanimously, decided by engineers that the evaporation shall be reckoned as having been effected at the boiling point from water assumed also to be supplied at that temperature, and that one pound thus evaporated shall be the unit. This being the established unit of evaporation, consistency and convenience both dictate that the power

of the boiler should be expressed in the same unit, or some handy multiple thereof; (2) It is submitted that the reduction of this unit to an exact multiple of the unit of evaporation will greatly facilitate calculations, inasmuch as the work done by the boiler is to be reduced to the same standard of feed-temperature and temperature of evaporation; (3) By the adoption of this unit, the trouble and risk of error coming from the attempt to use a factor as proposed above, differing from the multiple of the already accepted factor by 14.96 per cent., may be entirely avoided; (4) The unit last proposed is equivalent to 26.09 pounds of water evaporated from 100° Fahr. into steam at 70 pounds pressure, and is claimed to be itself more nearly representative of good average practice than the centennial unit.

Your committee has carefully weighed the arguments relating to these standards, as they were presented in writing by their respective advocates, and, after due consideration, has determined to accept the Centennial Standard, the first above mentioned, and to recommend that in all standard trials the commercial horse-power be taken as *an evaporation of* 30 *pounds of water per hour from a feed-water temperature of* 100° *Fahr. into steam at* 70 *pounds gauge pressure*, which shall be considered to be equal to 34½ units of evaporation, that is, to 34½ pounds of water evaporated from a feed-water temperature of 212° Fahr. into steam at the same temperature. This standard is equal to 33,305 thermal units per hour.*

It is the opinion of this committee that a boiler rated at any stated number of horse-powers should be capable of developing that power with easy firing, moderate draught and ordinary fuel, while exhibiting good economy; and further, that the boiler should be capable of developing at least one-third more than its rated power to meet emergencies at times when maximum economy is not the most important object to be attained.

Any increase of temperature derived from a feed-water heater acted upon by the products of combustion escaping from a boiler should not be credited to the evaporative efficiency of the boiler

* According to the tables in Porter's Treatise on the Richards Steam Engine Indicator, which tables the committee would recommend for general acceptance by engineers, an evaporation of 30 pounds of water from 100° F., into steam at 70 pounds pressure is equal to an evaporation of 34.488 pounds from and at 212°; and an evaporation of 34½ pounds from and at 212° F., is equal to 30.010 pounds from 100° F., into steam at 70 pounds pressure.

The "unit of evaporation" being equal to 965.7 thermal units, the commercial horse-power $= 34.488 \times 965.7 = 33{,}305$ thermal units.

except by agreement; and in the latter case accurate tests can be made only with feed-water of the average temperature used during the regular operation of the boiler.

The code presented by your committee is necessarily, as has been already indicated, condensed to the utmost possible extent consistent with exactness, and essential completeness. In matters of detail, it must be left to the engineer to carry out the evident spirit and intent of the code by devising his own methods; and it may be expected that every engineer will be competent to supplement the directions here given, as far as is necessary.

In order, however, to exhibit the extent to which he may work up such details, and to present the views of the members of the committee more fully, both in matters in which they agree and in those in which differences of views exist, an appendix is added to the report, in which memoranda written out by them are given describing details of work more fully than they are given in the code, and expressing individual opinions in regard to such matters as have seemed to each of such importance as to demand special notice. Each of these notes is signed with the initials of the writer.

Respectfully submitted.

WM. KENT,
J. C. HOADLEY,
R. H. THURSTON,* *Committee.*
CHAS. E. EMERY,
CHAS. T. PORTER,

CODE OF RULES FOR BOILER TESTS.

PRELIMINARIES TO A TEST.

I. *In preparing for* and conducting trials of steam-boilers, the specific object of the proposed trial should be clearly defined and steadily kept in view. (Appendix I.)

II. *Measure and record the dimensions*, position, etc., of grate and heating surfaces, flues and chimneys, proportion of air space in the grate surface, kind of draught, natural or forced.

III. *Put the Boiler in good condition.*—Have heating surface clean inside and out, grate bars and sides of furnace free from clinkers, dust and ashes removed from back connections, leaks in masonry stopped, and all obstructions to draught removed. See that the damper will open to full extent, and that it may be closed

when desired. Test for leaks in masonry by firing a little smoky fuel and immediately closing damper. The smoke will then escape through the leaks.

IV. *Have an understanding with the parties* in whose interest the test is to be made as to the character of the coal to be used. The coal must be dry, or, if wet, a sample must be dried carefully and a determination of the amount of moisture in the coal made, and the calculation of the results of the test corrected accordingly.

Wherever possible, the test should be made with standard coal of a known quality. For that portion of the country east of the Alleghany Mountains good anthracite egg coal or Cumberland semi-bituminous coal may be taken as the standard for making tests. West of the Alleghany Mountains and east of the Missouri River, Pittsburgh lump coal may be used.*

V. *In all important tests* a sample of coal should be selected for chemical analysis.

VI. *Establish the correctness of all apparatus* used in the test for weighing and measuring. These are:

1. Scales for weighing coal, ashes, and water.
2. Tanks, or water meters for measuring water. Water meters, as a rule should only be used as a check on other measurements. For accurate work, the water should be weighed or measured in a tank. (Appendix VI. and VII.)
3. Thermometers and pyrometers for taking temperatures of air, steam, feed-water, waste gases, etc. (Appendix X. to XIII.)
4. Pressure gauges, draught gauges, etc. (Appendix IX., XIV., and XV.)

VII. *Before beginning a test*, the boiler and chimney should be thoroughly heated to their usual working temperature. If the boiler is new, it should be in continuous use at least a week before testing, so as to dry the mortar thoroughly and heat the walls.

VIII. Before beginning a test, the boiler and connections should be free from leaks, and all water connections, including blow and extra feed pipes, should be disconnected or stopped with blank flanges, except the particular pipe through which water is to be fed

* These coals are selected because they are about the only coals which contain the essentials of excellence of quality, adaptability to various kinds of furnaces, grates, boilers, and methods of firing, and wide distribution and general accessibility in the markets.

to the boiler during the trial. In locations where the reliability of the power is so important that an extra feed pipe must be kept in position, and in general when for any other reason water pipes other than the feed pipes cannot be disconnected, such pipes may be drilled so as to leave openings in their lower sides, which should be kept open throughout the test as a means of detecting leaks, or accidental or unauthorized opening of valves. During the test the blow-off pipe should remain exposed.

If an injector is used it must receive steam directly from the boiler being tested, and not from a steam pipe, or from any other boiler.

See that the steam pipe is so arranged that water of condensation cannot run back into the boiler. If the steam pipe has such an inclination that the water of condensation from any portion of the steam-pipe system may run back into the boiler, it must be trapped so as to prevent this water getting into the boiler without being measured.

STARTING AND STOPPING A TEST.

A test should last at least ten hours of continuous running, and twenty-four hours whenever practicable. The conditions of the boiler and furnace in all respects should be, as nearly as possible, the same at the end as at the beginning of the test. The steam pressure should be the same, the water level the same, the fire upon the grates should be the same in quantity and condition, and the walls, flues, etc., should be of the same temperature. To secure as near an approximation to exact uniformity as possible in conditions of the fire and in temperatures of the walls and flues, the following method of starting and stopping a test should be adopted:

X. *Standard Method.*—Steam being raised to the working pressure, remove rapidly all the fire from the grate, close the damper, clean the ash pit, and as quickly as possible start a new fire with weighed wood and coal, noting the time of starting the test and the height of the water level while the water is in a quiescent state, just before lighting the fire.

At the end of the test, remove the whole fire, clean the grates and ash pit, and note the water level when the water is in a quiescent state; record the time of hauling the fire as the end of the test. The water level should be as nearly as possible the same as at the beginning of the test. If it is not the same, a correction should be made by computation, and not by operating

pump after test is completed. It will generally be necessary to regulate the discharge of steam from the boiler tested by means of the stop valve for a time while fires are being hauled at the beginning and at the end of the test, in order to keep the steam pressure in the boiler at those times up to the average during the test.

XI. *Alternate Method.*—Instead of the Standard Method above described, the following may be employed where local conditions render it necessary:

At the regular time for slicing and cleaning fires have them burned rather low, as is usual before cleaning, and then thoroughly cleaned; note the amount of coal left on the grate as nearly as it can be estimated; note the pressure of steam and the height of the water level—which should be at the medium height to be carried throughout the test—at the same time; and note this time as the time of starting the test. Fresh coal, which has been weighed, should now be fired. The ash pits should be thoroughly cleaned at once after starting. Before the end of the test the fires should be burned low, just as before the start, and the fires cleaned in such a manner as to leave the same amount of fire, and in the same condition, on the grates as at the start. The water level and steam pressure should be brought to the same point as at the start, and the time of the ending of the test should be noted just before fresh coal is fired.

DURING THE TEST.

XII. *Keep the Conditions Uniform.*—The boiler should be run continuously, without stopping for meal-times or for rise or fall of pressure of steam due to change of demand for steam. The draught being adjusted to the rate of evaporation or combustion desired before the test is begun, it should be retained constant during the test by means of the damper.

If the boiler is not connected to the same steam pipe with other boilers, an extra outlet for steam with valve in same should be provided, so that in case the pressure should rise to that at which the safety valve is set, it may be reduced to the desired point by opening the extra outlet, without checking the fires.

If the boiler is connected to a main steam pipe with other boilers, the safety valve on the boiler being tested should be set a few pounds higher than those of the other boilers, so that in case of a

rise in pressure the other boilers may blow off, and the pressure be reduced by closing their dampers, allowing the damper of the boiler being tested to remain open, and firing as usual.

All the conditions should be kept as nearly uniform as possible, such as force of draught, pressure of steam, and height of water. The time of cleaning the fires will depend upon the character of the fuel, the rapidity of combustion, and the kind of grates. When very good coal is used, and the combustion not too rapid, a ten-hour test may be run without any cleaning of the grates, other than just before the beginning and just before the end of the test. But in case the grates have to be cleaned during the test, the intervals between one cleaning and another should be uniform.

XIII. *Keeping the Records.*—The coal should be weighed and delivered to the firemen in equal portions, each sufficient for about one hour's run, and a fresh portion should not be delivered until the previous one has all been fired. The time required to consume each portion should be noted, the time being recorded at the instant of firing the first of each new portion. It is desirable that at the same time the amount of water fed into the boiler should be accurately noted and recorded, including the height of the water in the boiler, and the average pressure of steam and temperature of feed during the time. By thus recording the amount of water evaporated by successive portions of coal, the record of the test may be divided into several divisions, if desired, at the end of the test, to discover the degree of uniformity of combustion, evaporation and economy at different stages of the test. (Appendix II. and III.)

XIV. *Priming Tests.*—In all tests in which accuracy of results is important, calorimeter tests should be made of the percentage of moisture in the steam, or of the degree of superheating. At least ten such tests should be made during the trial of the boiler, or so many as to reduce the probable average error to less than one per cent., and the final records of the boiler test corrected according to the average results of the calorimeter tests.

On account of the difficulty of securing accuracy in these tests, the greatest care should be taken in the measurements of weights and temperatures. The thermometers should be accurate to within a tenth of a degree, and the scales on which the water is weighed to within one-hundredth of a pound. (Appendix XVII. to XXI.)

ANALYSES OF GASES.—MEASUREMENT OF AIR SUPPLY, ETC.

XV. In tests for purposes of scientific research, in which the determination of all the variables entering into the test is desired, certain observations should be made which are in general not necessary in tests for commercial purposes. These are the measurement of the air supply, the determination of its contained moisture, the measurement and analysis of the flue gases, the determination of the amount of heat lost by radiation, of the amount of infiltration of air through the setting, the direct determination by calorimeter experiments of the absolute heating value of the fuel, and (by condensation of all the steam made by the boiler,) of the total heat imparted to the water.

The analysis of the flue gases is an especially valuable method of determining the relative value of different methods of firing, or of different kinds of furnaces. In making these analyses great care should be taken to procure average samples—since the composition is apt to vary at different points of the flue—(Appendix XVI.)—and the analyses should be intrusted only to a thoroughly competent chemist, who is provided with complete and accurate apparatus.

As the determinations of the other variables mentioned above are not likely to be undertaken except by engineers of high scientific attainments, and as apparatus for making them is likely to be improved in the course of scientific research, it is not deemed advisable to include in this code any specific directions for making them.

RECORD OF THE TEST.

XVI. A "log" of the test should be kept on properly prepared blanks, containing headings as follows:

Time.	Pressures.			Temperatures.					Fuel.		Feed Water.	
	Barometer.	Steam Gauge.	Draught Gauge.	External Air.	Boiler Room.	Flue.	Feed Water.	Steam.	Time.	lbs.	Time.	lbs. or c. ft.

REPORTING THE TRIAL.

XVII. The final results should be recorded upon a properly prepred blank, and should include as many of the following items as are adapted for the specific object for which the trial is made. The items marked with a * may be omitted for ordinary trials, but are desirable for comparison with similar data from other sources.

Results of the trials of a..
Boiler at..
To determine..

1. Date of trial..			
2. Duration of trial..	hours.		
DIMENSIONS AND PROPORTIONS.			
Leave space for complete description. See Appendix XXIII.			
3. Grate surface....wide....long....Area........	sq. ft.		
4. Water-heating surface........................	sq. ft.		
5. Superheating surface........................	sq. ft.		
6. Ratio of water-heating surface to grate surface.			
AVERAGE PRESSURES.			
7. Steam pressure in boiler, by gauge............	lbs.		
*8. Absolute steam pressure........................	lbs.		
*9. Atmospheric pressure, per barometer..........	in.		
10. Force of draught in inches of water............	in.		
AVERAGE TEMPERATURES.			
*11. Of external air........................	deg.		
*12. Of fire room........................	deg.		
*13. Of steam........................	deg.		
14. Of escaping gases........................	deg.		
15. Of feed-water........................	deg.		
FUEL.			
16. Total amount of coal consumed †..............	lbs.		
17. Moisture in coal........................	per cent.		
18. Dry coal consumed........................	lbs.		
19. Total refuse, dry........pounds =............	per cent.		
20. Total combustible (dry weight of coal, Item 18, less refuse, Item 19)........................	lbs.		
*21. Dry coal consumed per hour..................	lbs.		
*22. Combustible consumed per hour..............	lbs.		
RESULTS OF CALORIMETRIC TESTS.			
23. Quality of steam, dry steam being taken as unity.			
24. Percentage of moisture in steam..............	per cent.		
25. No. of degrees superheated..................	deg.		

* See reference in paragraph preceding table.

† Including equivalent of wood used in lighting fire. 1 pound of wood equals 0.4 pound coal. Not including unburnt coal withdrawn from fire at end of test.

WATER.			
26. Total weight of water pumped into boiler and apparently evaporated *	lbs.		
27. Water actually evaporated, corrected for quality of steam ††	lbs.		
28. Equivalent water evaporated into dry steam from and at 212° F. ††	lbs.		
*29. Equivalent total heat derived from fuel in British thermal units ††	B. T. U.		
30. Equivalent water evaporated into dry steam from and at 212° F. per hour	lbs.		
ECONOMIC EVAPORATION.			
31. Water actually evaporated per pound of dry coal, from actual pressure and temperature ††	lbs.		
32. Equivalent water evaporated per pound of dry coal from and at 212° F. ††	lbs.		
33. Equivalent water evaporated per pound of combustible from and at 212° F. ††	lbs.		
COMMERCIAL EVAPORATION.			
34. Equivalent water evaporated per pound of dry coal with one-sixth refuse, at 70 pounds gauge pressure, from temperature of 100° F. = Item 33 multiplied by 0.7249	lbs.		
RATE OF COMBUSTION.			
35. Dry coal actually burned per square foot of grate surface per hour	lbs.		
*36. Consumption of dry coal per hour. Coal assumed with one-sixth refuse. †† — Per sq. ft. of grate surface	lbs.		
*37. — Per sq. ft. of water heating surface	lbs.		
*38. — Per sq. ft. of least area for draught	lbs.		
RATE OF EVAPORATION.			
39. Water evaporated from and at 212° F. per sq. ft. of heating surface per hour	lbs.		
*40. Water evaporated per hour from temperature of 100° F. into steam of 70 pounds gauge pressure. †† — Per sq. ft. of grate surface	lbs.		
*41. — Per sq. ft. of water heating surface	lbs.		
*42. — Per sq. ft. of least area for draught	lbs.		

* Corrected for inequality of water level and of steam pressure at beginning and end of test.

†† The following shows how some of the items in the above table are derived from others:

Item 27 = Item 26 × Item 23.

Item 28 = Item 27 × Factor of evaporation.

Factor of evaporation $= \frac{H - h}{965.7}$, H and h being respectively the total heat

COMMERCIAL HORSE-POWER.			
43. On basis of thirty pounds of water per hour evaporated from temperature of 100° F. into steam of 70 pounds gauge pressure, (= 34½ lbs. from and at 212°) ††....................	H. P.		
44. Horse-power, builders' rating, at.....square feet per horse-power..............................	H. P.		
45. Per cent. developed above, or below, rating ††..	per cent		

APPENDIX TO CODE.

I. OBJECT OF THE TEST.

In preparing for and conducting trials of steam boilers, the specific object of the proposed trial should be clearly defined and steadily kept in view.

1. If it be to determine the efficiency of a given style of boiler or of boiler-setting under normal conditions, the boiler, brick-work, grates, dampers, flues, pipes, in short, the whole apparatus, should be carefully examined and accurately described, and any variation from a normal condition should be remedied if possible, and if irremediable, clearly described and pointed out.

2. If it be to ascertain the condition of a given boiler or set of boilers with a view to the improvement of whatever may be faulty, the conditions actually existing should be accurately observed and clearly described.

3. If the object be to determine the relative value of two or more kinds of coal, or the actual value of any kind, exact equality

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units in steam of the average observed pressure and in water of the average observed temperature of feed, as obtained from tables of the properties of steam and water.

Item 29 = Item 27 × $(H - h)$.

Item 31 = Item 27 ÷ Item 18.

Item 32 = Item 28 ÷ Item 18 or = Item 31 × Factor of evaporation.

Item 33 = Item 28 ÷ Item 20 or = Item 32 ÷ (per cent. 100 − Item 19).

Items 36 to 38. First term = Item 20 × $\frac{6}{5}$

Items 40 to 42. First term = Item 39 × 0.8698.

Item 43 = Item 29 × 0.00003 or = $\frac{\text{Item 30}}{34\frac{1}{2}}$.

Item 45 = $\frac{\text{Difference of Items 43 and 44}}{\text{Item 44}}$.

of conditions should be maintained if possible, or where that is not practicable, all variations should be duly allowed for.

4. Only one variable should be allowed to enter into the problem; or, since the entire exclusion of disturbing variations cannot usually be effected, they should be kept as closely as possible within narrow limits, and allowed for with all possible accuracy.

J. C. H.

II. GENERAL OBSERVATIONS.

All observations are to be made by the expert, either personally or by his assistants. No statement of any kind is to be received from the owner or persons in charge of the boiler. All possibility of anything that would falsify the results must be closely guarded against; all pipes not used must be taken away or blank flanges inserted.

The two great points that are to be determined in every test of a steam-boiler, whatever the special and precise purpose of such test may be, are, the pounds of fuel burned, and the pounds of water evaporated.

To arrive at these we need to know, first, the pounds of fuel put into the furnace, and second, the pounds of water fed into the boiler.

To ascertain these facts with certainty is the fundamental requisite in all cases. The possibility of an error in either of these respects throws doubt upon all the results or indications of the test. The coal supplied to the furnace and the water fed to the boiler should, therefore, each be ascertained in a manner that proves its own correctness and excludes doubt.

All tests of this nature are properly regarded with suspicion. I often myself read of tests and results that I put no faith in, and the same must be true of every one who is experienced in this matter. I am therefore strenuous on this point that a system of firing and a system of measuring the feed water should be employed that will prove the correctness of the record, and if errors are made, will clearly expose them.

If possible the steam generated should be condensed by passing it through a surface condenser, where it is cooled by a strong current of watér in a closed chamber. By this means the number of thermal units added may be ascertained with precision.

A boiler test cannot be conducted properly when it is complicated by being combined with an engine test. C. T. P.

III. PRECAUTIONS TO BE OBSERVED IN MAKING A BOILER TEST.

It should be steadily kept in mind that the principal observations to be made are the quantities of coal consumed and of water evaporated. If these quantities are ascertained accurately, and the conditions made the same at the beginning and end of the test, the most important requisites of a boiler trial will be secured. Other observations have their value both for scientific and practical purposes, but are in most cases subsidiary.

Boiler tests are often undertaken with insufficient apparatus and assistance. It is possible for a single person to test one boiler or even several in a battery, but it requires a great deal of labor to do so, and in many cases such person would be so fatigued as to be liable to make a simple error, vitiating the results. He would moreover at no time be able to give proper oversight to the test, so as to prevent accidental or unauthorized interferences. It is very desirable, in fact almost indispensable, that an assistant be detailed to weigh the coal, and another to weigh or measure the water; if calorimeter tests are to be undertaken, still another assistant should be provided. The engineer in charge is then left free to oversee the work of all, and relieve either temporarily when necessary. Engineers are frequently called upon to make boiler trials in connection with parties whose interests are antagonistic to a fair test, and frequently the voluntary assistance of busybodies is likely to produce errors in the results. It is therefore essential to have trustworthy assistants, and those of sufficient calibre not to be confused by interested parties, who will frequently endeavor in the most plausible manner to make out that a certain measure of coal has been already tallied, or that a certain tank of water has not been tallied.

In the first engine trials at the American Institute Exhibition (1869), in the Centennial boiler trials (1876), and since in private trials respecting performance of boilers as between the contractor and purchaser, the writer has arranged for both interests to take the data at the same moment, with instructions, if agreement could not be had, that the difference be at once referred to him.

In weighing the coal, the barrow or vessel used should be balanced on a scale and then filled to a certain definite weight. The laborer will soon learn to fill a vessel to the same weight within a few pounds by counting the number of shovels thrown in, when the change of a lump or two, to or from a small box alongside the scale will balance it.

The water may be measured in one tank by filling it to one mark and pumping down to another, but this involves stopping the pump when filling the tank, thereby failing to maintain uniformity of conditions. Two tanks arranged so that each can be filled and emptied alternately are much better. A still better plan is to have a settling tank to pump from and a measuring tank which is emptied into it, and this plan is improved by setting the measuring tank on a scale, and actually weighing the water. For large operations three tanks are necessary: a lower tank to pump from and two measuring tanks, one of which is filling while the other is being emptied. The writer has made several double measuring tanks with a horizontal section like the figure "8," there being a partition between the two tanks lower than the rim of the tanks. Water is conducted at will in either of the two tanks by a pipe swinging over the partition. One tank is allowed to fill until the water in it overflows into the other (which has been emptied and the cock shut), when the filling pipe is shifted into the empty tank, and as soon as the water level subsides in the full one, the water in that tank is allowed to flow out, the cock shut before the other tank is filled, and the operation repeated.

A simple tally should never be trusted. Nothing seems more reliable to an inexperienced observer than to mark 1, 2, 3, 4, with a diagonal cross mark for 5; but when there are waits of several minutes between the marks, and several operations performed after a tally is made, there will be confusion in the mind whether or not the tally has been actually made. The tallies both of weights of coal and of tanks of water should be written on separate lines, the time noted opposite each, and the records always made at the beginning or termination of some particular operation; for instance, in weighing coal at the time only when the barrel or bucket is dumped on the fire-room floor. It is desirable to have a number of coincident records of coal and water throughout the trial, so that in case of accident it may be held to have ended at one of such times. The uniformity of the operations may also be tested in this way from time to time. For this reason it will be found convenient to fire from a wheelbarrow set on a scale and to have a float or water-guage connected with the tank from which the water is pumped; by which means the coal and water used may, in an evident way, be ascertained for any desired interval.

As to calorimeter tests, note from the special article on that

subject (Appendix XVII.), that the results are liable to be untrustworthy simply from an improper connection to the boiler. Scales and thermometers very finely graduated are desirable, but if they cannot be procured, good instruments with medium graduations carefully standardized may be employed, when if the observer will take the precaution mentioned in the appended article of the writer on calorimeter experiments, and simply make each record according to his best judgment at the time, the average of the results will be substantially accurate, although the several experiments may disagree somewhat with each other.

C. E. E.

IV. WEIGHING THE COAL.

Where practicable, a box consisting of sides, back and bottom, capable of holding 500 pounds of coal for each boiler having twenty-five square feet fire-grate area, and in proportion for larger grates, should be placed on scales conveniently located for shoveling from it upon the fire grate.

The exact time of weighing each charge of say 500 pounds, should be noted and the net weight, whatever it be, set down. The box should be balanced by a fixed counterpoise, so that the readings of the scale beam may be net pounds of coal.

On the instant of closing the fire door after each firing, the weight should be taken and the exact time noted as well as the weight. The box should be completely emptied each time, and the accuracy of the counterpoise observed, and, if necessary, adjusted. The differences of weight at each firing will give the several quantities fired ; the differences of time will give the intervals in minutes and seconds between successive firings; and the differences of time between the successive charges—500 pounds, more or less—on the scales, will afford a check on the record of the firing. A chart or diagram should be plotted from the figures, which will clearly show the degree of regularity with which firing has been carried on, and reveal any omission or error. J. C. H.

V. WEIGHING THE COAL.

I would recommend that on a test no coal be brought into the furnace room except as follows :—

A barrow to be employed, and be loaded each time at the coal pile with an equal amount, say 600 lbs. of coal, weighed on platform scales at the pile. The time when it is thus wheeled into

the furnace-room to be noted. The barrow to be wheeled upon another platform scale before the furnace for the following purpose:—

In separate columns, the times of charging the furnace to be noted, and the reading of the scales after each charging. The coal to be shoveled from the barrow directly into the furnace.

Now here the log would show at once, by the great inequality of the intervals, if a barrow-load of coal had been added or omitted, and the weights charged on the fire would check the barrow-loads, and should also show the rate of firing.

No other coal being convenient to the furnace, reasonable watching will give assurance that none is surreptitiously added to the fire. C. T. P.

VI. WEIGHING THE WATER.

The best way is to have two tanks capable of holding 1,200 to 1,800 pounds—say 20 to 30 cubic feet, or two weighing tanks and one feeding tank, 144 to 216 gallons, each placed on a pair of scales, to be filled and emptied alternately. To avoid suspicion of leakage of stop-cocks, it is better to draw out the water by a flexible pipe or suction hose put alternately into the two tanks. The time of each weighing of each tank, to be designated as tank No. 1 and tank No. 2, should be accurately noted, and a method of checking the weighings by a diagram or chart as in respect to the coal, should be adopted. J. C. H.

VII. MEASURING THE FEED WATER.

I would recommend that on all tests of any magnitude the water be fed to the boilers from a single tank of known capacity. That the tank be always filled so as to overflow, while the feed pump is stopped, and also the communication to it is closed.

That the inlet pipe shall terminate above the tank so that its orifice is always visible. That after the supply has been shut off, and the overflow has ceased, the communication to the feed pump be opened and the pump be started. That the water be drawn down to a point that is determined by a line on a graduated rod attached to a float that has been well painted so as not to absorb the water; and that then the pump be stopped, communication with it be closed, and the tank be refilled.

The time of starting the pump each time to be carefully noted. The regularity of the intervals would leave no room for doubt

as to the number of tanks that had been emptied. The watch of opposite interests would insure the accuracy of the line at which the pump is stopped each time, and at which the test was closed.

C. T. P.

VIII. KEEPING TIME OF OBSERVATIONS.

All time-keepers should be set at the start, and compared at the close; a gong should be used to give a signal for all observations designed to be synchronous and isochronous, in order that such observations may be conveniently arranged. J. C. H.

IX. RECORDING STEAM GAUGE.

A good recording steam gauge, Edson's or other, carefully adjusted, should be used and accurately compared with the steam gauge at stated intervals. Such an automatic record, nicely integrated, is a good check on the record of the steam gauges.

J. C. H.

X. AIR THERMOMETER.

The air thermometer is the best instrument for taking the temperature of flues, smoke boxes, etc., from 300° to 700° or 750° F. These instruments cost but a trifle, \$3 to \$5, and can be made anywhere, by any competent expert, or by any one of his assistants under his direction, and can be relied on from ordinary temperatures, say 60° to 90°, up to any temperature which glass will bear without deformation. Ordinary machine-divided paper scales can be used with them. The great point is to deprive the interior of the bulb and tube of all moisture, and to fill the bulb and the upper half of the leg of the inverted syphon connected with the bulb, with dry air. (Appendix XI.). The expansion of dry air is practically uniform for all useful ranges of temperature, and its volume is directly proportioned to its temperature from absolute zero, say 461.2° F. below zero F., equal to 493.2° F. below the temperature of melting ice, to which the conventional zero of the air thermometer, at the accurately observed and noted temperature of the air when the mercury in the two legs of the inverted syphon is exactly level—the tubes being exactly vertical—can be conveniently referred. For instance, if the temperature of the air when the mercury in the two legs is level, be 73.8° F., add to this 461.2°, and we have 541.0° F. absolute, as the true absolute temperature corresponding to our zero.

To double this temperature—1082° F. absolute (equal to 1082° — 461.2° = 620.8° F. above zero F.), would double the volume of the air; but the volume being nearly constant—since the capacity of the tube may generally be disregarded, a difference of level will be produced in the height of the mercury in the two legs of the inverted syphon exactly equal to the height of the mercury column in a mercurial barometer at the time. No correction for capillarity is required, since the negative capillarity is equal in the two legs. No correction for temperature is required, unless the temperature of the *mercury* in the air thermometer is higher than that of the mercury column of the barometer. If there is an observable difference, it must be corrected for, at the rate of 0.0001 per degree F.

There should be at least two of these air thermometers, three would be safer, in readiness for each test, to avoid disappointment by accident. The legs of the inverted syphon must be vertical, but the tube from the upper end of the leg to the bulb may be straight, or bent to any angle.

For the determination of the heat of flue gases, this instrument is indispensable, up to the limit of the softening of glass; but since no flue will always, or even usually, contain volumes of gas of equal temperature throughout at the same instant, at least two tubes of gas-pipe, welded up at the lower end, and filled with mercury, should be placed in opposite sides of the flue, near the air thermometer, for observing the differences with chemical thermometers graduated on the glass. Sir Wm. Thompson highly commends thermometers incased in hermetically sealed glass tubes, with scales graduated on paper for use up to a point below the temperature required to scorch the paper. Dampness being excluded by the glass case, the paper scales are of unchangeable length, and the graduations and figures are very distinct and legible.

J. C. H.

XI. DESCRIPTION OF AN AIR THERMOMETER OF CONSTANT VOLUME (AFTER REGNAULT), AND OF THE MODE OF CONSTRUCTING AND USING THE SAME.

This instrument may be made in many forms, and of materials of several kinds—metals, or glass, or metal and glass. A simple, inexpensive, and convenient form * consists of a U tube of about

* Constructed and brought to my notice by Mr. Fred W. Prentiss. Originally devised by Regnault.—J. C. H.

three-eighths of an inch external diameter (Fig. 87) and about one-sixteenth of an inch calibre, or a little less; having a short leg about 39 inches long, and the other leg longer by 12 inches or more; the latter surmounted by a bulb blown out of the tube, $1\frac{5}{8}$ inches in diameter, $6\frac{5}{8}$ inches in extreme length, and 5 inches long in its straight, cylindrical portion.

The two legs, or branches, of the U, are 2 inches apart between centers.

They are separate tubes, each one bent to a right angle, by a curve of short radius, ground square and true at the ends which are to meet, and hermetically united by a short coupling of rubber tubing, firmly bound on each with wire.

In blowing the bulb, a small, short tube, about $\frac{1}{16}$ inch in calibre and 2 or three inches long, is formed on top for use in making the instrument—to be sealed by fusion when it is done.

Having formed the U tube by uniting its branches, the next thing to be done is to dessicate its interior perfectly and to fill it with dry air. For this purpose it is put in any covenient position—reclining, probably—a piece of rubber tubing is secured to the small tube on top of the bulb and connected with a U tube about 6 inches long in its branches, and $\frac{5}{8}$ inch or $\frac{3}{4}$ inch in diameter, filled with dry lumps of chloride of calcium and surrounded by crushed ice, to lower its temperature, and the temperature of the air passing through it to about 32° F., at which point air parts with a larger portion of its moisture than at any higher temperature.

An aspirator is now connected by a piece of rubber tube with the open end at the short branch of the instrument, and a stream of air is drawn in through the chloride of calcium tube and discharged by the aspirator.

A simple and efficient form of aspirator is merely a piece of $\frac{1}{4}$-inch gas-pipe, bent, when hot, into three or four sharp zigzags, with an inlet at its upper extremity for water, and at its side for air.

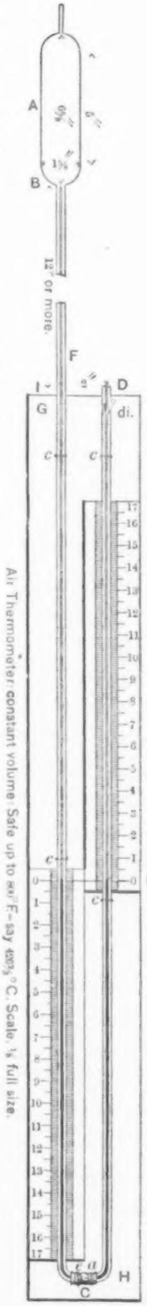


FIG. 87.

A stream of water flowing through the zigzag tube draws air in at the side orifice, and the air becoming entangled with the water, flows off with it, its place being supplied through the chloride of calcium tube and the U tube of the thermometer. This operation can be carried out conveniently in any office or other room supplied with a flow of water and a set wash-basin; and once arranged, requires only so much attention as to see that it remains undisturbed.

When the tube is completely dessicated (in so far as air at 32° F. will give up its moisture to chloride of calcium), which will be in about four or five hours, shut off the water, bend over the rubber tube connecting the calcium-chloride tube with the bulb, or, another rubber tube connected with the outer branch of the calcium-chloride tube, so as to prevent any mixture of moist air with the dry air in the bulb and U tube, and lay the instrument on its side in such a manner that the longer branch shall slope from the bulb down to the rubber coupling, and that the shorter branch shall also slope from the rubber coupling down to the extremity of this branch, which should be kept closed by the finger until it is immersed in mercury, to prevent the admixture of moist air. If the mercury is in a wide, shallow dish, like a plate or a saucer, the sloping end of the branch may be immersed in it sufficiently; or a short piece of glass tube coupled on *in advance*, when preparations were made for the dessicating process, may be held down in the mercury. Then apply the lips to the calcium-chloride tube or to the rubber tube connected therewith, and, by inspiration of breath, draw out air from the bulb until the mercury, forced into the shorter branch, fills it, and shows just beyond the rubber coupling in the lower end of the long branch. Then pinch the rubber tube, set the instrument upright (keeping the open end of the shorter branch closed with the finger until it is upright). See that the branch tubes are exactly vertical; carefully relieve the pinched rubber tube, so that air may escape, until the surface of the mercury in the two branches is exactly level; then pinch the rubber tube and fuse and seal the small glass tube into a little button on top of the bulb. Now hang up an accurate chemical thermometer, graduated on the tube, close beside the bulb, until this thermometer and the bulb and the dry air inside of it are certain to have come to a common temperature, and read and note this temperature; and make a distinct and permanent mark on the back-board of the instrument, at the level of the mercury in the two branches.

This back-board may be 4 or 5 inches wide, $\frac{1}{2}$ to $\frac{5}{8}$ inch thick, and about as long as the shorter branch, and the tube may be secured to it by little staples of annealed iron wires, going around (*i. e.* over) the tube, and through holes in the back-board; and twisted together at the back. A bit of soft leather at the staple, between the board and the tube, will form a secure bed for the tube, and obviate danger of breaking. Such staples are indicated on the drawing, at *c*, *c*, *c*. There may be two such staples at the bottom, passing over the rubber coupling, to further aid in keeping the two parts (branches) of the U tube in proper position. At the same time that the temperature is noted, note also the height of the mercury column of a barometer. On the air thermometer, hanging in my office, the notes are:

"Temperature 81.5° F.

"Barometer, Hg, 31.03 in."

Scales, as indicated on drawing, complete the instrument. For these, engine-divided paper scales will answer; and they may be graduated to inches and tenths of inches, as I have indicated, or to millimeters.

Since the instrument is to be used chiefly or wholly for temperatures above any atmospheric temperature at which it may be set, the scale on the long leg need not exceed much above, nor that on the short leg much below, the level line; but a $\frac{1}{2}$ inch, or an inch may be worth while, if the instrument is set as high as 80° F., for convenience of comparison with ordinary thermometers.

FOR USING THE INSTRUMENT:

Let t_1 = temperature at which thermometer is set.

t_2 = temperature sought from observation.

$\pm h$ = difference of level of mercury, when the thermometer was set; + when mercury is highest in short leg; − when mercury is highest in long leg; $h = o$, when mercury is level at the noted temperature when the thermometer is set.

Let b_1 = mercury column of barometer when the thermometer was set.

b_2 = mercury column of barometer at the time of observation.

$\pm h_2$ = difference of level of mercury in the two branches when observed:

Then:

$$t_2 = \left[(461.2 + t_1)\frac{(b_2 \pm h_2)}{(b_1 \pm h_1)}\right] - 461.2$$

If made of good hard glass, this instrument is safe at 800° F., say $426\frac{2}{3}$° C., and will not be very likely to fail at under 850° F., say $454\frac{2}{3}$° C.

The part of the tube below the bulb *BI*, may be of any convenient length, and may be bent as at *F*, to any angle to suit requirements of location. J. C. H.

XII. PYROMETER.

So far as known to me the only way to measure temperatures between 600° or 700° F., or above the range of the air thermometer, and 2500° or 2700° F., or up to the melting point of commercial platinum, is by the platinum water pyrometer.

One form of this pyrometer is described in the journal of the Franklin Institute, Vol. 84, pp. 169 and 252, September and October, 1882. J. C. H.

XIII. PYROMETER.

The temperature of the escaping gases should be ascertained, not by pyrometers, but by means of certified mercury thermometers introduced at a number of different points in the same plane transverse to the flue. The velocity of the current should be ascertained at each of these points. The distance of the transverse plane of observation from the boiler should be noted.

C. T. P.

XIV. DRAUGHT GAUGE.

Some instruments for indicating the force of chimney draught:

a. A bent glass tube filled with water.

b. A bent tube with two fluids.

c. An incased aneroid.

d. A differential pressure gauge.

The incased aneroid, having inches of mercury indicated by spaces of about two inches, divided to $\frac{1}{500}$, answers well. The case is air tight, and by means of a three-way cock the interior of the case may be put alternately in communication with the external air and with any flue into which a suitable pipe is inserted.

The differential pressure gauge was devised and put to use at the Massachusetts Institute of Technology, and similar instruments should be manufactured for sale. I will not attempt to describe it further than to say that a column of water in a glass tube, acting on a small diaphragm, balances the weight of the movable

parts when a large diaphragm is in equilibrium of pressure. Now if this large diaphragm have chimney pressure on the inner side, and atmospheric pressure on the outside, the difference of pressure will be shown by a rise of water in the glass tube to a height proportioned to the ratio of the areas of the small and large diaphragm.

Draught should be measured in different parts of the flue, in order to detect infiltration of air through cracks in the brick-work and through the brick-work itself. J. C. H.

XV. DRAUGHT GAUGE.

Mr. C. P. Higgins, of Philadelphia, has recently made the draught gauge shown in the sketch (Fig. 88). The gauge is filled with water above the level of the horizontal tube, so as to leave a bubble of air about half an inch long near one end of the horizontal tube when the water is level in the side tubes. The inside diameter of the two vertical tubes being the same,

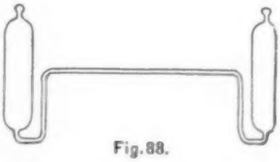


Fig. 88.

say half an inch, and the diameter of the horizontal tube one-eighth of an inch, a draft equal to one inch of water, or which will cause the difference in the level of the two tubes to be one inch, will cause the bubble to move eight inches in the horizontal tube.

The readings of the ordinary U tube draught gauge are thus multiplied by 8, with the additional advantage that the position of the air bubble can be read more accurately than the difference of level in the ordinary gauge. The scale applied to the horizontal tube requires to be standardized for the ratio of areas of the small and large tubes and for irregularities in the calibre of tubes.

W. K.

XVI. SAMPLING FLUE GASES.

Very great diversities in the composition of flue gases often exist in the same flue at the same time. To obtain a fair sample, it has been found sufficient to have one orifice to draw off gases through for each 25 sq. inches of cross section of flue. The pipes must be of equal diameter and of equal length. $\frac{1}{4}$ in. gas-pipes, all alike at the ends, and of equal lengths, answer well. Similar steel tubes will be still better.* These should be secured in a box or block of galvanized sheet iron, equal in thickness to one course

* Because smoother and more uniform.

of brick, in such a manner that the open ends may be evenly distributed over the area of the flue *A* (Fig. 57*a*), and their other open ends inclosed in the receiver *B*. If the flue gases be drawn off from the receiver *B* by four tubes, *C C*, into a mixing box, *D*, beneath, about 3 inch cube, a good mixture can be obtained.

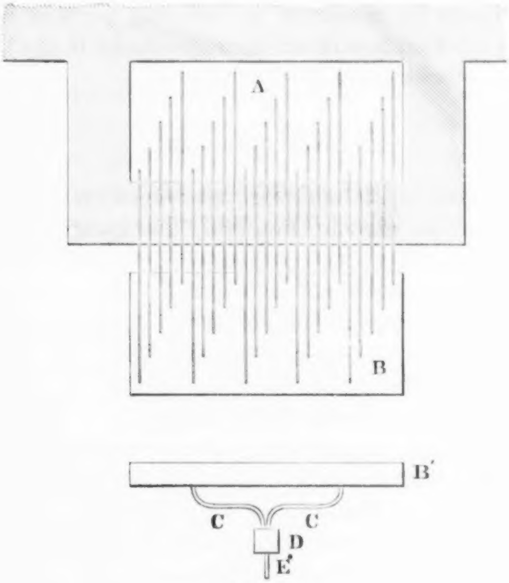


FIG. 57*a*.

Two such "samplers," one above the other a foot apart, in the same flue, will furnish samples of gases which show by analysis the same composition. J. C. H.

XVII. CALORIMETER EXPERIMENTS.

In all boiler experiments it is important to ascertain the quality of the steam, *i. e.*, 1st, whether the steam is "saturated" or contains the quantity of heat due to the pressure according to standard experiments; 2d, whether the quantity of heat is deficient, so that the steam is wet; and 3d, whether the heat is in excess and the steam superheated. The best method of ascertaining the quality of the steam is undoubtedly that employed by a committee which tested the boilers at the American Institute Exhibition of 1871-2, of which Professor Thurston was chairman; but this plan cannot always be adopted. When all the steam generated is not

condensed, the method of making the connection for the purpose of taking out a sample is of the utmost importance. Unless great care be exercised, the results will frequently show that the steam is superheated when the boiler has no superheating surface. The cause of this is pointed out at p. 82 of the writer's general report on the exhibits referred to the Judges of Group XX., Centennial Exhibition. It is not fair to take the steam direct from the boiler, for if there be no steam circulation at that point the steam will of course show dry. The samples should be taken from the main steam pipe, but not from the bottom, as this would take all the water draining to that point. The method of taking it through a perforated pipe crossing the main steam pipe is sure to cause difficulty whenever the velocity of steam flowing to the calorimeter is sufficient to reduce the pressure in the supply pipe, for in such case the temperature of the steam in that pipe falls at the inlet, and the steam of full pressure and higher temperature flowing through the main pipe adds heat to that flowing into the calorimeter pipe, so that the latter, when referred to the pressure from which it is derived, shows superheating. The same effect takes place in a less degree when the steam for the calorimeter is taken through a lateral opening of small diameter, the metal surrounding the opening being kept warm by the current passing through the main pipe, and imparting its heat to the steam flowing in the lateral pipe to the calorimeter. To avoid this difficulty, the writer recommends making the lateral opening leading to the calorimeter $1\frac{1}{2}$ to 2 inches in diameter, and then at a little distance from the main pipe, say 1 foot, reducing the supply pipe to calorimeter to $\frac{3}{4}$ or $\frac{1}{2}$ inch diameter.

For general use the writer prefers the ordinary barrel calorimeter, which has the advantage over a continuous calorimeter operating at a slow rate of flow, that with the latter the condensation in the connecting pipes may cause the small quantity of steam flowing to the calorimeter to be moist, and thereby vitiate the results. With the barrel calorimeter it is desirable to heat the water promptly, so that the question of condensation in connecting pipes is of minor importance. At the same time the quantity of steam drawn off should not be so great, in connection with that passing to other points, as to cause the boiler to foam, or to reduce the pressure.

The practice of the writer is to use a barrel, holding preferably 400 lbs. of water, which is set upon a platform scale, and pro-

vided with a cock or valve for allowing the water to flow to waste. I have always provided a small propeller made with blades simply cut out of a disc of sheet iron, twisted to give the pitch and bolted on to the bottom of a vertical rod supported in a wooden step in the bottom of the barrel, and passing through a cross piece on the top of the barrel. The rod terminates at the top in a crank, and a collar is placed on the vertical shaft under the upper support. A fixed thermometer is run through a cork in the bunghole of the barrel. The pipe conducting the steam from the main steam pipe is made of graduated sizes, as previously referred to, and the smaller pipe provided near the calorimeter with a valve connected by means of a coupling with a rubber hose. In the coupling is to be placed a disc of metal, provided with a regulating hole of from $\frac{3}{16}$ to $\frac{1}{4}$ inch in diameter.

To operate the calorimeter the barrel is filled with water, the weight and temperature ascertained, steam blown through the hose outside the barrel until the pipe is thoroughly warmed, when the hose is suddenly thrust in the water, and the propeller operated until the temperature of the water is increased to the desired point, say about 110° usually. The hose is then withdrawn quickly, the temperature noted, and the weight again taken. The object of the particular details adopted will be readily understood. The simple propeller insures a uniform heating of the whole of the water. The little disc in the supply pipe enables the stop valve in pipe from boiler to be opened wide without drawing off so large a quantity of steam as to lower the pressure or produce priming. To avoid the jar when the steam hose is in the water, it is better to cut some lateral holes in the hose near its lower end. In this way a circulation is induced through the holes which prevents most of the jar and noise.

The weight of water in calorimeter should be increased proportionally to the weight and specific heat of all metal exposed to changes of temperature with the water. An addition of one-ninth of the weight of the propeller and submerged portion of shaft and fastenings will be substantially correct if the apparatus be made of iron.

The importance of errors of measurement or observation are inversely proportional to the magnitudes of the quantities. The weight of water added by condensation of steam being comparatively small, it must be weighed accurately, say within a quarter of one per cent. The writer has done this on an ordinary plat-

form-scale in good order by using a second movable poise, in addition to the customary one, and of one-tenth its weight. In weighing, the lighter poise is adjusted to bring the free end of the beam to a fixed mark. The same result may be obtained by loading the platform with small known weights to bring the lever to a fixed point each time, and deducting such weights from the reading of scale in regular notches.

The above must be considered a makeshift, but a valuable one. When possible, delicate scales should be employed, and, in the opinion of the writer, better satisfaction can be obtained in this direction than by the use of the more complicated apparatus required to weigh the water of condensation separately.

In making the calculations the following notation and formula prepared by the writer for the report of the Committee having in charge the testing of the boilers of the Centennial Exhibition will be found convenient:

Let W = original weight of water in calorimeter.
Let w = weight of water added by heating with steam.
Let T = total heat in water due to the temperature of steam at observed pressure.
Let H = total heat of steam at observed pressure.
Let l = latent heat of steam at observed pressure = $(H - T)$.
Let t = total heat of water corresponding to initial temperature of water in calorimeter.
Let t' = total heat in water corresponding to final temperature of water in calorimeter.
Let Q = quality of steam.

Then

$$Q = \frac{1}{l}\left(\frac{W}{w}(t' - t) - (T - t')\right). \tag{1}$$

Then when $Q < 1$, percentage of moisture in steam = 100 $(1 - Q)$.

When $Q > 1$, number of degrees steam is superheated = 2.0833 $l\,(Q - 1)$.

The later practice of the writer when there are a large number of calculations to be made is as follows:

Add to above notation the following:
Let m = percentage of moisture in steam.
Let s = number of degrees steam is superheated.

Let A = number of heat units lacking per pound of steam condensed. Equals quantity in parenthesis, Equation (2).

Let Σ = sign of summation. To be read: Sum of values of—

Let n = number of experiments to be averaged.

Then

$$(2) \qquad m = \frac{1}{l}\Big(\overbrace{(H - t') - \frac{W}{w}(t' - t)}^{A}\Big).$$

$$(3) \qquad Q = 1 - m.$$

When A or m is minus.

$$(4) \qquad s = -\ 2.0833\,A.$$

Averaging several experiments

$$(5) \qquad m = \frac{\Sigma A}{n\,l}.$$

$$(6) \qquad s = -\ 2.0833\,\frac{\Sigma A}{n}.$$

C. E. E.

XVIII. NOTE ON USE OF THE BARREL CALORIMETER.

In the use of the barrel calorimeter not less than 300 lbs. of water should be used, and it is an advantage, when practicable, to cool the water by means of pulverized ice. By vigorous agitation the water may thus be cooled to 36° F., or even 34° F., in a few minutes, when the remaining ice is to be completely removed. As the ice floats on the surface, this can be readily done. The weight of steam condensed can thus be often doubled, and still the temperature of the water not be raised above 100° F., at which point no sensible loss of heat will be suffered through evaporation. The greater the weight of steam condensed, the less will be the unavoidable percentage of error.

If the barrel be covered with a non-conductor, it will be found that no sensible change in the temperature of the water will take place in a long time.

C. T. P.

XIX. EFFECT OF SMALL ERRORS OF OBSERVATION IN CALORIMETER TESTS.

Suppose a case in which errors of observation occur, as in the following table:

	OBSERVED READING.	TRUE READING.	AMOUNT OF ERROR.
Weight of condensing water, corrected for equivalent of apparatus, W	200.5 lbs.	200 lbs.	$\frac{1}{2}$ pound.
Weight of condensed steam, w	9.9 "	10.0 "	$\frac{1}{10}$ "
Pressure of steam by gauge, P	78. "	80 "	2 pounds.
Original temperature of condensing water, t	44.5°	45°	$\frac{1}{2}$ degree.
Final " " " t'	100.5°	100°	$\frac{1}{2}$ "

The formula for calculation is

$$Q = \frac{1}{H - T}\left(\frac{W}{w}(h - h_1) - (T - h_1)\right)$$

in which Q = quality of the steam, dry saturated steam being unity.
H = total heat of steam at observed pressure.
T = " " water " "
h = " " condensing water, original.
h_1 = " " " " final.

		Moisture per cent.	Error per cent.
Substituting in the formula the "true readings" in the table, we have for the value of	$Q = 0.9874$	= 1.26	= 0.
All readings true except $W = 200.5$,	$Q = .9906$	= 0.94	= 0.32
" " " $w = 9.9$,	$Q = 1.0000$	= 0.00	= 1.26
" " " $P = 78.$,	$Q = .9880$	= 1.20	= 0.06
" " " $t = 44.5$,	$Q = .9989$	= 0.11	= 1.15
" " " $t' = 100.5$,	$Q = .9994$	= 0.06	= 1.20
" " incorrect	$Q = 1.0272$	= (minus)	= 3.98

The last case, $Q = 1.0272$, is equivalent to 50.2 degrees superheating.

The errors above noted are all such as may easily occur even with good apparatus. The condensing water being usually weighed in a barrel on an ordinary platform scale, an error of $\frac{1}{2}$ a pound could easily be made if the scale were not carefully tested and standardized. To make as small an error as $\frac{1}{10}$ of a pound in the weight of the condensed steam, when it is weighed in the

bulk with the condensing water, taking the difference of readings before and after the test, is almost more than can be expected. The probable error of such a method of weighing the condensed steam is usually more than a quarter of a pound. The error in this weight is the most important of all those given in the table, showing dry steam, $Q = 1.00$, instead of 1.26 per cent. moisture, the true result. If the error of the weight of the condensed steam were $\frac{1}{4}$ lb., it would be equivalent to an error of 3 per cent. in the calculated moisture in the steam, and consequently of 3 per cent. in the total result of the boiler test. The error of steam pressure, 2 lbs., is well within the limit of error of many steam gauges, but as seen in the result, it is the least important of all the errors, giving 1.20 per cent. moisture instead of 1.26 per cent. The errors of $\frac{1}{2}$ a degree in temperature of condensing water are also quite important, and show the necessity of having thermometers carefully standardized. The effect of an error of weighing the condensed steam is so serious, and it is so likely to occur, that in the writer's opinion the method of making tests with a barrel on a platform scale, without any special weighing of the condensed steam, is so inaccurate that it should be discouraged, or at least that the results obtained by it should be considering as having a probable error of 3 per cent. It is questionable whether averaging a large number of results so obtained will give any greater approach to truth, for the errors of weighing in a barrel on a coarse platform scale, of the condensed steam together with the condensing water, due to personal equation, to absorption and evaporation of water, to error of sliding or stationary poise, and to friction of scale are apt to be, comparatively, constant, and may by no means be expected to balance each other. W. K.

XX. COIL CALORIMETER.

The following is a description of a calorimeter, which the writer has found to give fairly good results, but sufficient experiments have not yet been made with it to determine its limit of error.

A surface condenser is made of light weight copper tubing $\frac{3}{4}''$ in diameter and about 50′ in length, coiled into two coils, one inside of the other, the outer coil 14″ and the inner 10″ in diameter, both coils being 15″ high. The lower ends of the coils are connected by means of a brazed T-coupling to a shorter coil, about 5′ long, of 2″ copper tubing, which is placed at the bottom of the smaller coil and acts as a receiver to contain the condensed water.

The larger coil is brazed to a $\frac{3}{4}''$ pipe, which passes upward alongside of the outer coil to just above the level of the top of the coil and ends in a globe valve, and a short elbow pipe which points outward from the coil. The upper ends of the two $\frac{3}{4}''$ coils are brazed together into a T, and connected thereby to a $\frac{3}{4}''$ vertical pipe provided with a globe valve, immediately above which is placed a three-way cock, and above that a brass union ground steam tight. The upper portion of the union is connected to the steam hose, which latter is thoroughly felted down to the union. The three-way cock has a piece of pipe a few inches long, attached to its middle outlet and pointing outward from the coil.

A water barrel, large enough to receive the coil and with some space to spare, is lined with a cylindrical vessel of galvanized iron. The space between the iron and the wood of the barrel is filled with hair felt. The iron lining is made to return over the edge of the barrel, and is nailed down to the outer edge so as to keep the felt always dry. The barrel is furnished also with a small propeller, the shaft of which runs inside of the inner coil when the latter is placed in the barrel. The barrel is hung on trunnions by a bail by which it may be raised for weighing on a steelyard supported on a tripod and lifting lever. The steelyard for weighing the barrel is graduated to tenths of a pound, and a smaller steelyard is used for weighing the coil, which is graduated to hundredths of a pound.

In operation the coil, thoroughly dry inside and out, is carefully weighed on the small steelyard. It is then placed in the barrel, which is filled with cold water up to the level of the top of the globe valves of the coil and just below the level of the three-way cock, the propeller being inserted and its handle connected. The barrel and its contents are carefully weighed on the large steelyard; the steam hose is connected by means of its union to the coil, and the three-way cock turned so as to let the steam flow through it into the outer air, by which means the hose is thoroughly heated; but no steam is allowed to go into the coil. The water in the barrel is now rapidly stirred in reverse directions by the propeller and its temperature taken. The three-way cock is then quickly turned, so as to stop the steam escaping into the air and to turn it into the coil; the thermometer is held in the barrel, and the water stirred until the thermometer indicates from five to ten degrees less than the maximum temperature desired. The globe valve leading to the coil is then rapidly and tightly closed, the

three-way cock turned to let the steam in the hose escape into the air, and the steam entering the hose shut off. During this time the water is being stirred, and the observer carefully notes the thermometer until the maximum temperature is reached, which is recorded as the final temperature of the condensing water. The union is then disconnected and the barrel and coil weighed together on the large steelyard; the coil is then withdrawn from the barrel and hung up to dry thoroughly on the outside. When dry it is weighed on the small scales. If the temperature of the water in the barrel is raised to 110° or 120° the coil will dry to constant weight in a few minutes. After the weight is taken, both globe valves to the coil are opened, the steam hose connected, and all of the condensed water blown out of the coil, and steam allowed to blow through the coil freely for a few seconds at full pressure. When the coil cools it may be weighed again, and is then ready for another test.

If both steelyards were perfectly accurate, and there were no losses by leakage or evaporation, the difference between the original and final weights of the barrel and contents should be exactly the same as the difference between the original and final weights of the coil. In practice this is rarely found to be the case, since there is a slight possible error in each weighing, which is larger in the weighing on the large steelyard. In making calculations the weights of the coil on the small steelyard should be used, the weights on the large steelyard being used merely as a check against large errors.

It is evident that this calorimeter may be used continuously, if desired, instead of intermittently. In this case a continuous flow of condensing water into and out of the barrel must be established, and the temperature of inflow and outflow and of the condensed steam read at short intervals of time, as in Mr. Geo. H. Barrus's calorimeter described below. W. K.

XXI. THE BARRUS CALORIMETER.

The Barrus Calorimeter (Fig. 57) is of the continuous type, and consists essentially of a small surface condenser. The accompanying cut shows its general features. The steam enters by the pipe *j*, which is a common half-inch iron steam pipe. The condensing surface, *a*, is a continuation and enlargement of the supply pipe, and is an ordinary one-inch iron pipe with a length of 12 inches of exposed surface. This pipe is under the full pressure of steam.

The condensed water which amounts to about 50 pounds per hour under 80 pounds per square inch of steam pressure, collects in the lower part of the apparatus, where its level is shown in the glass, *e*, and is drawn off by means of the drip valve, *d*. The injection water, previously cooled to a temperature of 40° Fahr., or less, enters the wooden vessel, *o*, through the valve, *b*. Here it circulates around the condensing pipe, being carried downward to

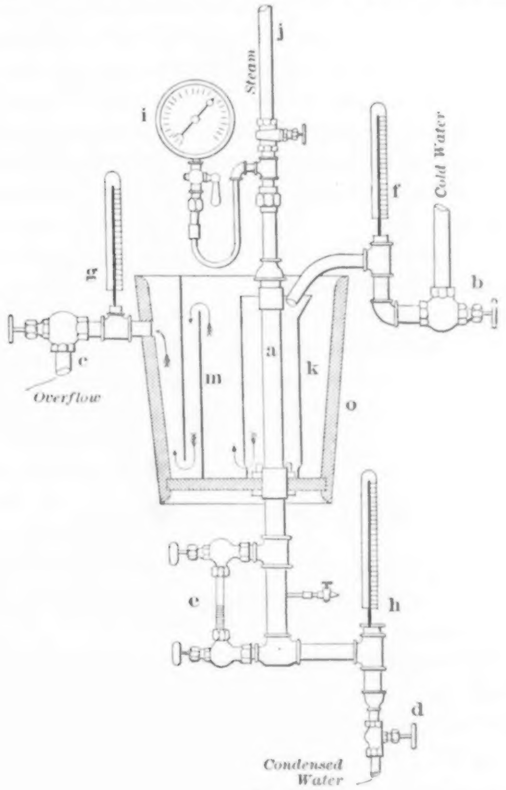


Fig.57.

the bottom by means of the tube, *k*, and it overflows at the pipe, *c*, after passing through the mixing chambers, *m*. The amount of water admitted is regulated so as to secure a temperature at the overflow of 75° or 80°, or the approximate temperature of the surrounding atmosphere. The thermometers, *f* and *g*, which are read to tenths of a degree, show the temperature of injection and overflow water, and the thermometer, *h*, shows that of the condensed water.

The overflow water and the condensed water is in each case collected in a system of weighing tubs. The steam pipe down to the surface of the water, and the pipes embraced in the lower part of the apparatus, are covered with hair felt.

When once in operation, this calorimeter can be worked any number of hours desired. By making observations with sufficient frequency, accurate mean readings can be obtained for either a long or a short period. There is no wire-drawing of the steam, and no allowance to be made for specific heat of the apparatus. The only correction to be made that is of material amount is that for radiation from the pipes covered with hair felt, and this can be accurately determined in each particular case by an independent radiation experiment, made when the condenser vessel is empty.

Below are the approximate data and results of an experiment under 80 lbs.:

Condensed water, 50 lbs. per hour.

Injection water, 1,100 lbs. per hour.

Injection water heated from 35° to 75° = 40°.

Temperature condensed water, 300°.

Condensed by radiation from steam pipe, 1 lb. per hour.

$$\text{Total Heat} = \frac{1100 \times 40}{50 - 1} + 300^\circ = 898^\circ + 300^\circ = 1198^\circ.$$

W. K.

XXII. REPORTING THE RESULTS.

As to reporting the results of boiler tests—two things are necessary, in order to make the reports, (*a*,) generally intelligible, and, (*b*,) strictly comparable.

1. The number of pounds of water actually evaporated under stated (actual) conditions of feed-water temperature and steam-gauge pressure, into steam containing not over three per cent. of entrained water, by each pound of coal burned—coal of good mercantile quality, dry; water dried out of a sample and allowed for, or, containing not over one-half per cent. of surface moisture, by actual experiment of drying samples. In this latter case, the one-half per cent. of water in the coal, like the three per cent. of entrained water in the steam, and the stated quantity of ashes and refuse in the coal, are taken in for the sake of representing usual conditions. So much for *general intelligibility*.

2. The equivalent evaporation in pounds of water of $t = 212^\circ$ F. converted into dry saturated steam of one atmosphere pressure,

=760 mm= 29.92 in. mercury,—with one pound of *dry* combustible consumed. This for *strict comparability*.

It is obvious, if attention be given to the subject—too often neglected—that all the surface water in the coal, if not ascertained and allowed for, will appear as combustible and disappear as water evaporated.

For example, two per cent. of water in the coal, passing over the bridge wall and going up chimney, leaving no weight to represent it in the "ashes and residue," will increase the item of "combustible" by two per cent. of the gross weight of the coal; and if ashes and residue $= \frac{1}{6}$ of the gross weight, the addition will be $2\% \times \frac{6}{5} = 2.4\%$. At the same time, about two-ninths of one per cent. of the water evaporated will escape observation, going up chimney unnoticed.

There should also be introduced into general practice an equivalent statement. of

3. The equivalent evaporation in pounds of water from feed-water temperature = 100° F. into usual steam containing not over three per cent. entrained water of seventy pounds per square inch pressure by steam gauge above 1 atmosphere = 760 mm. = 29.92 inches mercury—for each pound of commercial coal containing not over one-sixth ashes and residue including surface water; one pound of such commercial coal being capable of imparting to the water, in a boiler of good proportions, about 10,000 British thermal units.

J. C. H.

XXIII. REPORTING THE TEST.

The report should include a complete description of the boiler, which, for special boilers, should be written out at length, but generally can conveniently be presented in tabular form substantially as follows:

Type of boiler.
Diameter of shell.
Length of shell.
Number of tubes.
Diameter " "
Length " "
Diameter of steam drum.
Width of furnace.
Length of furnace.

Kind of grate bars.
Width of air spaces.
Ratio of area of grate to area of air spaces.
Area of chimney.
Height of chimney.
Length of flues connecting to chimney.
Area " " " " "

GOVERNING PROPORTIONS.

Grate surface.

Heating surface { Water. Steam. Total.

Area of draught through or between tubes.
Ratio grate to heating surface.
" draught area to grate
" " " " total heating surface.
Water space.
Steam space.
Ratio grate to water space.
" " " steam space.

C. E. E.

XXIV. OBSERVATION BLANKS.

The observations taken during a test should be recorded on a series of blanks prepared in advance so as to be adapted to the purposes of the trial. The number of sheets and the particular items on each may be varied to suit the number of observers and the work designated for each. The following are copies of observation blanks used in the Centennial trials with a few lines of figures inserted, without reference to each other, for the purposes of illustration. The columns should of course be of sufficent length to contain the number of observations expected.

C. E. E.

LOG OF TRIAL OF BOILER.

NO. 1.—RECORD OF FEED-WATER.

1	2	3	4	5	6	7	8
Time.	Tank A.			Tank B.			Height of Water in Glass.
	Initial Weight.	Final Weight.	Temperature.	Initial Weight.	Final Weight.	Temperature.	
Hrs. Min. *a.m.*	Lbs.	Lbs.	Deg Fah.	Lbs.	Lbs.	Deg. Fah.	Ins.
5.22	......	....		......	.....	....	7
6.19	1442.5	136	63	......	.....	....	0
6.40	......	.....	..	1421 5	169.5	63.5	6
7.05	1447	131.5	68	......	.....	....	6
7.28	......	.....	..	1431.5	193.5	67	7
8.00	1445.5	316	67	......	.....	....	5

Deduct 56.25 pounds of feed-water for difference of level in boiler.

LOG OF TRIAL OF BOILER

NO. 2.—GENERAL OBSERVATIONS—COAL AND ASHES.

9	10	11	12	13	14	15	16	17	18	19	20	21
Time.	Steam Pressure.	Temperatures. (Fahrenheit.)					Coal and Ashes.				Barometer.	Height of Water in Glass.
		Air.	Fire-Room.	Steam.	Uptake.		Coal Weighed out on Floor.	Coal Consumed.	Coal found in Ashes.	Weight of Ashes. *Net.*		
					Ther.	Pyr.						
Hrs. Min. *a.m.*	Lbs.	Deg.	Deg.	Deg.	Deg.	Deg.	Lbs.	Lbs.	Lbs.	Lbs.	Ins.	Ins.
9.22	70	49	88	310	....	375	230.5	170	578	304.5	30.20	10
10.00	70	51	82	299	....	401	229.5	Wood.	....	....	....	7
10.30	70	57	83	308	....	446	229.5	235.5	....	...	....	3
11.00	70	..	80	309	....	446	229.5	229	...	....	....	0.5
11.30	70	55	80	299	....	432	223.5	221.5	....	....	....	10
12.00	70	..	80	298	....	444	231	227	....	....	...	8

LOG OF TRIAL OF BOILER.

NO. 3.—RECORD OF CALORIMETER EXPERIMENTS.

22	23	24	25	26	27	28
TIME.	WATER.			TEMPERATURES. (Fahrenheit.)		STEAM PRESSURE.
	Weight of Barrel.	Initial Gross Weight.	Final Gross Weight.	Initial	Final.	
Hrs. Min. *a.m.*	Lbs.	Lbs.	Lbs.	Deg.	Deg.	Lbs.
5.35	80.5	400	412.5	73.5	106.125	70 —66
5.55	80.5	400	413.375	68.25	110.50	70 —67
6.15	80.5	400	411.375	72.50	111	70 —68.5
6.35	80.5	400	417.25	66	122	70 —68
6.55	80.5	400	415.125	67.5	114.25	70 —64
7.15	80.5	400	416	74.5	122.25	70 —66
7.35	80.5	400	411.75	74.5	113.75	69 —70
7.55	80.5	400	413.25	72.5	115.25	70 —65
8.15	80.5	400	413.25	71	112.75	70 —62

XXV. HORSE POWER.

The writer's preference for rating boilers in horse power is:

Capacity to evaporate into dry steam, *i.e.*, not containing over three per cent. of entrained water, and the water actually entrained allowed for and deducted:—

1. $34\frac{1}{2}$ pounds of water from and at 212°, equal to

2. 30 pounds of water of $t = 100°$ under $p = 70$ pounds per square inch above one atmosphere; with easy firing, moderate draught, and ordinary fuel, implying good economy, and capability of fifty per cent. increase to meet emergencies.

As to the last condition, "capability of fifty per cent. increase to meet emergencies":

It must be obvious that a boiler which, under most favorable conditions of fuel, draught, firing, and everything else, just capable of evaporating into dry steam 3,450 pounds of water from 212° into the atmosphere, with open safety valve—or, what comes to the same thing, 3,000 pounds from $t = 100°$ to $p = 70 +$ atm. could not be

called a 100 horse-power boiler with any propriety. Good ordinary practical conditions should do that, with satisfactory economy; and then fifty per cent. more should be obtainable to meet a sudden call, or to supply a brief deficiency. J. C. H.

XXVI. STEAM UNITS.

All measurements of the quantity of heat are based on the *thermal unit*, which, for British measures, equals the quantity of heat required to raise the temperature of one pound of pure water at or near its freezing-point one degree Fahr.*

The unit commonly used to express the evaporative power of the fuel is the quantity of heat required to evaporate one pound of water at a temperature of 212° under the ordinary pressure of the atmosphere corresponding to that temperature. This was called by Rankine a "peculiar thermal unit," and its value given at 966.1 British thermal units, but has since been called the "*unit of evaporation*," which term is adopted in the foregoing general report of the committee. Its value, however, in the prominent American tables is given at 965.7 thermal units.

The *mechanical equivalent* of a thermal unit equals very nearly 772 foot-pounds of work, but the power that can be utilized practically per unit of heat depends on so many conditions that a universal standard of work or power (the rate of work) based on heat units, is impossible. Compound engines operated with high steam slightly superheated, require a little over 14 pounds of feed-water evaporated per honr, while there are still in use poor engines, ill-proportioned steam pumps, and the like, that require over 60 pounds, or say one cubic foot of water per hour, which was considered as about equivalent to a horse-power of steam in the days of Watt. It has of late years, however, been well accepted that 30 pounds of feed-water per hour is a fair standard of horse-power for average good high-pressure engines, such as are used for manufacturing purposes. Bearing in mind that this quantity of steam must be furnished by the boiler under actual conditions, the writer, in preparing the report of the committee of the judges of Group XX. appointed to test the boilers at the Centennial Exhibition, suggested to his associates, Messrs. Chas. T. Porter and Joseph Belknap, that the value of the "commercial horse-power of a boiler be fixed at 30 pounds of water evaporated at 70 pounds

* Compare Rankine on Steam Engine, Art. 208; Porter on the Richards Indica'or, page 43.

gauge-pressure from a temperature of 100°."* This standard having been adopted in the foregoing report of the committee of the American Society of Mechanical Engineers, may be considered as established both by precedent and authority. It is fixed as equal to 34½ units of evaporation per hour, and is, for all practical purposes, equal to 33,333 thermal units per hour, making it convenient to obtain the horse-power by multiplying the total number of thermal units derived from the fuel per hour by 0.00003. It is of interest also to note that a cubic foot of steam at 70 pounds gauge-pressure weighs 1-5 of a pound avoirdupois, so that a Commercial Horse-power on the above basis is also represented by 150 cubic feet of steam per hour at 70 pounds pressure.

In administering the steam supply of the New York Steam Company, the writer provided for selling steam at a fixed rate per thousand "*Kals*," explaining that a "*Kal*" meant a pound of water evaporated into steam. This term has been in use in that business since February, 1883, and has proved so convenient that the writer has suggested that it can possibly be utilized to express the unit of the Commercial Horse-power above referred to. On this basis a boiler horse-power would equal simply 30 "Kals" per hour.†

In preparing the general report of the judges of Group XX., Centennial Exposition, it was observed that if a boiler supplying any kind of pumping machinery be proportioned to utilize 10,000 heat units per pound of coal consumed (corresponding to an evaporation of about 9 pounds of water at 70 pounds gauge-pressure from a temperature of 100°), the number of foot-pounds of work obtained in the engine for each thermal unit would also represent the duty, in millions of foot-pounds per 100 pounds of coal.‡ From this it will be seen that the Commercial Horse-power above referred to corresponds to a duty of 59.4 millions of pounds lifted one foot high with 100 pounds of coal, which is about the average duty of the simpler class of pumping engines, but not of first-class engines. Evidently, for the better class of steam machinery of all kinds, the steam producing capacity of the boiler must be made to conform to the actual amount of steam to be used by the engines. Any standard of the horse-power of a boiler necessarily relates

* See report of Committee at page 131 of the report of the Judges of Group XX. International Exh., 1876. J. B. Lippincott & Co., Philadelphia.

† See "Estimates for Steam Users," Vol. V. Transactions Am. Soc. Mech. Engineers, page 284.

‡ See Report of Judges of Group XX., Cent. Exh., pp. 21 and 115.

simply to its steam-producing capacity, referred to the arbitrary standard of a horse-power above mentioned.

C. E. E.

XXVII. MEMORANDUM RELATIVE TO A STANDARD METHOD OF TRIAL-TEST FOR STEAM-BOILERS.

The method customarily pursued in the course of the work of the Mechanical Laboratory of the Stevens Institute of Technology, as instituted by the writer, and that practiced in his own professional work, has usually been such as to secure data sufficient to enable the observer to fill out all the columns of the Log-blank and Table of Performance, copies of which are appended.

In starting the trial, which is usually of at least ten hours' duration, it is customary, where it can be conveniently done, to get up steam with a fire of wood, which is raked out after steam has begun to form freely, and the trial commences with the introduction of a new fire, in which wood is used to ignite the coal, and is charged as a certain percentage of its weight of coal—forty per cent. is probably as accurate as need be. The damper should be carefully closed during the few minutes required to perform this operation. Toward the end of the time fixed for the trial, the steam-pressure and height of water are made as nearly identical with the same conditions at the beginning as is possible, the fire is burned as low as the skill of the fireman and supervising engineer will permit, and when the end of the trial is recorded, the fire is hauled, the coal and ashes weighed dry, no more water being used in cooling them as they are drawn than is absolutely necessary for the comfort of the fireman, and never sufficient to leave any portion of the mass in the slightest degree damp.

Where it is impracticable to start with a new fire, and to remove the fire at the end of the trial, it is preferred to begin and end the trial with the cleaning hour, the quantity of coal being then most easily estimated and identical conditions being thus most readily approximated.

The steam-pressure is read from a gauge which it is intended shall always be carefully standardized both before and after the trial. The same precaution should be taken with all instruments used whenever possible.

During the trial, all the conditions are kept as nearly uniform as is possible, and as exactly those for which the boiler is designed as

is practicable. The supply of feed-water and of fuel, the pressure of steam, the frequency of firing or "stoking" are to be made definitely constant. A continuous feed rather than an intermittent supply of water is much preferred, and the injector is preferred where choice of instrument is permitted. The customary mode of feeding is, however, often best, whatever that may be. Determinations of the character of the steam made are considered essential in every case. The open, or "barrel," calorimeter of Hirn has been generally employed, making the number of observations sufficient to give a small margin of probable error. It is hoped that the relative value of the different forms may after a time be so well determined as to permit the acceptance of some one as a standard. The intermittent instrument consisting of a coil of pipe in a vessel of water and the continuous calorimeter, such as was proposed by Van Buren some years ago, and used by Skeel and the writer, and modifications since made by many others, are capable of doing good work; but engineers greatly differ in their estimates of their relative value, and the simplest form is at present in most general use, probably because of its portability, or the ease with which it can be improvised. Could it be done, the method of condensing all the steam made, as practiced at the American Institute trials of 1871, would be always adopted, in preference to the system of "sampling."

In the analysis of gases, the apparatus made by Greiner and Linke is found convenient when it is considered necessary to make such analyses. In fuel analysis, Monroe's system of carbon determination of by the use of lead oxide is probably as easy and as satisfactory as any for general use. More complete analyses are intrusted to a professional chemist, and are made in the chemical laboratory. The draught-gauge used is that designed by Mr. Allen for the Hartford Steam Boiler Insurance Co.

General Principles.—In the operation of conducting a trial of a steam-boiler, we have, usually, a single, well-understood object in view, and the engineer should accustom himself to carefully define that object in his own mind, and to as carefully describe that object in his instructions and regulations for the proposed trial. The whole operation can then be carried on with that point distinctly in view, and the proposed end can then be accomplished with maximum economy of time and labor, as well as with greatest exactness. The observations must be made by the engineer conducting the trial, or by his assistants, with this object

distinctly in mind, and each should have a well-defined part of the work assigned him, and should assume responsibility for that part, having a distinct understanding in regard to the extent of his responsibility, and a good idea of the extent and nature of the work done by his colleagues, and the relations of each part to his own. No observations should be permitted to be made by unauthorized persons for entrance upon the log; and no duties should be permitted to be delegated by one assistant to another, without consultation and distinct understanding with the engineer in charge. The aim of the observers is, in all cases, to obtain an exact determination of the weight of fuel used, its proportion of combustible matter effective in developing heat, the exact weight of water evaporated under the known conditions of the trial, into steam, the determination of the character of that steam, and often the nature of the combustion and the composition of the furnace-gases. Each of these distinct objects requires the determination of certain well-defined quantities, and the observer to whom each set of observations is intrusted should, whenever possible, be made sufficiently well acquainted with the object to be attained, and the method to be pursued in reaching it, to be able to make his own readings with accuracy, and to work up the results correctly. It is only after he has acquired this knowledge that he can be expected to do his work without direct supervision, and with satisfactory precision. The trial should, wherever possible, be so conducted than any error that may occur in the record may be detected, checked, or, if advisable, removed, by some process of mutual verification of related observations. It is in this direction that the use of graphical methods of record and automatic instruments have greatest value. We should lose no opportunity to introduce both.

Weighing Fuel.—Several methods of weighing fuel have been found very satisfactory, in the writer's experience; but he is inclined to make it an essential feature of either that the weights shall be made by one observer, and checked by another, at as distant a point as is convenient. The weighing of the fuel by one observer, at the point of storage, and the record at that point of times of delivery, as well as of weights of each lot, and the tallying of the number and record of the time of receipt, at the furnace-door, will be usually found a safe system. The introduction of unweighed coal, whether by accident, or by design on the part of some interested person, can never be too carefully guarded

against. The failure to record any one weight, which is a not unknown accident, leads to similar error, and can only be certainly prevented by an effective method of double observation and check.

Weighing Feed-Water.—The same remarks apply, to a considerable extent, to the weighing of the water fed to the boiler. A careful arrangement of weighing apparatus, a double set of observations, where possible, and thus safe checks on the figures obtained, are essential to certainty of results. With good men at the tank, and with small demand for water, a single tank can be used; but two are preferable, in all cases, and three should be used if the work demands very large amounts of feed-water, as at trials of very large boilers, or of "batteries." The more uniform the water supply, as well as the more steady the firing, the less the liability to mistake in making the record.

Character of Steam.—It has been the endeavor of every engineer conducting trials of boilers, of late, to secure correct determinations of the quantity of water entrained with the steam, or of the degree of superheating, where superheating occurs. This is, however, a matter of considerable difficulty. It was, so far as the writer is aware, first proposed and attempted by Hirn, the distinguished French engineer and physicist, who, many years ago, used what is now known as the tank, or barrel, calorimeter for this purpose. A jet of steam from the boiler was led into a tank containing a considerable mass of water, and condensation was allowed to go on until the water had acquired as high a temperature as was convenient. The amount of "priming" was then calculated by a comparison of the amount of heat transferred to the barrel, by the weight of steam taken from the boiler, with the amount that would be transferred by the same weight of perfectly dry steam.

This method was in use some years when the continuous calorimeter was proposed by Van Buren. This form was adopted by a committee, of which the writer was chairman, in the year 1875, in making tests of "sectional" boilers, the instrument used being designed by the late Mr. Theron Skeel, a member of the committee. The results of its work were satisfactory to the committee.

The method of testing the character of steam made in boilers by this system of sampling seemed somewhat open to doubt in respect to its accuracy, when used by Hirn, and, for a long time the writer looked for an opportunity to determine with certainty

what is the real amount of priming, under ordinary conditions of operation, in the common forms of boiler. This was finally offered in the year 1871, when as chairman of a committee on boiler trials for the American Institute, it became necessary to arrange for a comparison of several competing boilers of, fortunately, widely different types and forms. Through the liberality of Mr. J. B. Root, and with the earnest co-operation of Mr. Chas. T. Porter and others of the then exhibitors, it was rendered possible to construct a large surface-condenser, in which to condense *all* the steam made by each boiler during its trial. The arrangements were made with great care, and conducted under the writer's own personal direction and supervision, by carefully selected observers, and with the most cheerful and gratifying co-operation on the part of all the competing exhibitors. The result was the determination, with the most satisfactory certainty, of the real amount of total priming, as ascertained by observation of the total amount of water passing off as steam, and of the total amount of water carried out of the boiler unevaporated. Two of the boilers superheated their steam slightly; the others primed from three to seven per cent. The main object of the investigation, the determination of the question whether sampling steam can give fairly correct measures of the character of the mass, was in the writer's opinion well settled affirmatively by these experiments.

As to the best form of calorimeter, the writer is not yet fully satisfied, and hopes to find a way of making one that shall be at once simple, easily transported, and accurate. He has a strong impression that it will be a continuous calorimeter, but has very little doubt that improvements in accessory apparatus now in progress may make the Hirn form of instrument, sooner or later, a satisfactory one. The best work thus far has been done probably by the intermittent form of coil condenser, although experience with the continuous instrument has been very encouraging. Mr. Hoadley has done some beautiful work, and the apparatus described by Mr. Kent gives a means of checking weights which is a very useful and almost essential improvement upon that type of instrument.

A steam-boiler trial in which the quality of the steam is not, at least approximately determined, cannot be accepted to-day as giving any reliable measure of the efficiency of a boiler.

Near the end of the series of data recorded in the blanks appended, are columns intended to include the constants, as derived

from the trial, for introduction into the formulas of Rankine for efficiency of boiler and of the writer for that of chimney. It was the writer's expectation to be able, in course of time, to accumulate such an extensive set of data in this form as would enable Rankine's formula to be adjusted for use in all trials of the usual forms of boiler, and with our native fuels. The American fuels, and our common boilers, cannot be estimated, in respect to efficiency, by the use of that formula, with the degree of exactness that is desirable. The writer has been accustomed, in making such estimates, as a rule, to adopt a value of the constant multiplier less by about ten per cent. than that given by the author of the formula. It is hoped that an opportunity, ere long, will be afforded to make the comparison here alluded to.

R. H. T.

TABLE I.

LOG OF TRIAL

....

OF

MECHANICAL LABORATORY, DEPARTMENT OF ENGINEERING, STEVENS INSTITUTE OF TECHNOLOGY.

TEST MADE

At..

....

on..

$$x = \frac{U - wh}{H - h}$$

$$\frac{\frac{U}{w} + t - T}{0.48} = \text{Degrees of Superheating.}$$

TIME.	PRESSURES.			TEMPERATURES.					WEIGHTS.			REMARKS.
	Barometer.	Steam Gauge.	Draught Gauge.	External Air.	Boiler Room.	Fuel.	Feed Water.	Steam.	Fuel.	FEED-WATER.		
										Per Meter.	Per Tank.	

PRIMING TESTS.

NO.	TIME.	STEAM PRESSURES.	CALORIMETER.					HEAT UNITS PER POUND FROM BOILER.		Heat transferred to Calorimeter. $W \times R = U$	Heat from Steam. $H = T - t'$	Heat from Water. $h = t - t'$	Steam run into Calorimeter. x	Percentage of Priming. y	SUPERHEATING.		REMARKS.
			WEIGHTS.		TEMPERATURE.												
			Condensing Water. W	Wet Steam. w	Initial. t''	Final. t'	Range. $R = t' - t''$	Water. t	Steam. T						Degrees.	Heat Units.	

TABLE II.
AVERAGE AND TOTAL RESULTS OF TRIAL
OF
MECHANICAL LABORATORY, DEPARTMENT OF ENGINEERING, STEVENS INSTITUTE OF TECHNOLOGY.

TRIAL MADE *FUEL.*

at..............................

COMPOSITION.

on..............................

Number of Trial.	Date of Trial.	Length of Trial.	Areas.				Ratio of Grate to Heating Surface.	Average Temperatures.				Average Pressures.			Consumption of Fuel.			Ashes.		Total Combustible.	Remarks.
			Grate.	Heating Surface.	Super-heating Surface.	Least Cross-Section of Flues.		Boiler Room.	External Air.	Entrance to Chimney.	Feed-Water.	Barometer.	Steam Gauge.	Draught Gauge.	Total.	Per square foot of Grate per hour.	Per sq. ft. of Heating Surface per hour.	Total.	Proportion of Total Fuel.		
		Hours.	sq. ft.	sq. ft.	sq. ft.	sq. ft.		Fahr.	Fahr.	Fahr	Fahr	ins.	lbs.	ins.	lbs.	lbs.	lbs.	lbs.	per ct.	lbs.	

Apparent Evaporation.									Real Evaporation.											
Per Pound of Fuel.			Per Pound of Combustible.			Per square foot of Heating Surface, per Hour.			Per Pound of Fuel.			Per Pound of Combustible.			Per square foot of Heating Surface, per Hour.			Square feet of Heating Surface Required to Evaporate one Cubic foot of Water.		
From actual temperature of Feed-Water and at actual steam pressure.	Equivalent from and at 212° F.	Equivalent from 212° F. and at actual steam pressure.	From actual temperature of Feed-Water and at actual steam pressure.	Equivalent from and at 212° F.	Equivalent from 212° F. and at actual steam pressure.	From actual temperature of Feed-Water and at actual steam pressure.	Equivalent from and at 212° F.	Equivalent from 212° F. and at actual steam pressure.	From actual temperature of Feed-Water and at actual steam pressure.	Equivalent from and at 212° F.	Equivalent from 212° F. and at actual steam pressure.	From actual temperature of Feed-Water and at actual steam pressure.	Equivalent from and at 212° F.	Equivalent from 212° F. and at actual steam pressure.	From actual temperature of Feed-Water and at actual steam pressure.	Equivalent from and at 212° F.	Equivalent from 212° F. and at actual steam pressure.	From actual temperature of Feed-Water and at actual steam pressure.	Equivalent from and at 212° F.	Equivalent from 212° F. and at actual steam pressure.
lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	sq. ft.	sq. ft.	sq. ft.

TABLE II.—*Continued.*

TOTAL WATER FED TO BOILER.			AVERAGE PRIMING.	TOTAL WATER PRIMED.	WATER EVAPORATED INTO DRY STEAM.			REMARKS.
From actual temperature of Feed-Water and at actual steam pressure.	Equivalent from and at 212° F.	Equivalent from 212° F. and at actual steam pressure.			From actual temperature of Feed-Water and at actual steam pressure.	Equivalent from and at 212° F.	Equivalent from 212° F. and at actual steam pressure.	
lbs.	lbs.	lbs.	per cent.	lbs.	lbs.	lbs.	lbs.	

Average Amount of Superheating.	EVAPORATION FROM AND AT 212° F., EQUIVALENT TO TOTAL HEAT UNITS DERIVED FROM FUEL.			EFFICIENCY.			VALUES OF A AND B IN $F = A\sqrt{H} \pm B$.		HORSE-POWER.		REMARKS.
	Per pound of Fuel.	Per pound of Combustible.	Per sq. foot of Heating Surface, per hour.	Experimental.	Estimated.	$R = \frac{\text{Experimental.}}{\text{Estimated.}}$	A	B	Rated.	Actual.	
Fahr.	lbs.	lbs.	lbs.	per cent.	per cent.						

CLXVIII.—*A.*

DISCUSSION OF THE REPORT ON A STANDARD METHOD OF CONDUCTING STEAM BOILER TRIALS.

Presented to the American Society of Mechanical Engineers.

NOTE.—The Report of the Committee was formally presented at the New York Meeting of the Society, November, 1884. On account of its thoroughness and magnitude it was not discussed at that meeting, but it was ordered that it should be printed and sent to all the members before the spring meeting, that it might receive their careful examination. The discussion of that Report being made a special order for the second session of the meeting of May, 1885, the following suggestions were presented. They are printed in direct connection with the Report to which they relate, for convenience of reference, although constituting a part of the Proceedings of Part II., and belonging to the XIth Meeting.

The Society accepted the Report, but for reasons of policy clearly brought out in that part of the Discussion which is printed in a special Appendix, decided *not* to adopt the Report officially, although the trend of debate was decidedly favorable to it, but voted simply that the discussion be printed in full in the Transactions. The discussion was as follows :

Prof. W. P. Trowbridge.—The committee state in their Report very clearly and definitely the objects of a trial or test of a steam boiler, and present a code of rules for such tests which will doubtless meet with general approval. Their recommendations looking to the establishment of a unit of boiler-power appear to me, however, to be wanting in the exactness and precision which should characterize the definition of so important a unit; and I beg to offer my views on that part of the Report which treats of the subject, actuated only by the feeling that before final action by the Society is taken, this unit should be considered from all points of view, and especially in its commercial bearings and aspects.

There seems to have been a diversity of opinion in the committee as to whether the "unit of boiler power" should be expressed in terms of the "unit of evaporation," or in terms of another and different unit of evaporation first introduced and employed by the judges at the Centennial Exposition: the arguments in favor of the latter being that it has already been established by *authority*, that it conforms to general practice, and that it is now a generally accepted unit.

None of these arguments appear to me to be strictly tenable.

The unit adopted by the distinguished judges at the Centennial has not been sanctioned, nor enjoined by any statute, State or National, as far as I am aware, and has no binding force anywhere in commercial transactions.

The action of this Society, embracing as it does mechanical engineers from all parts of the country, will go further in establishing any particular unit *authoritatively*, if this term can be applied to such action, than an announcement from any other source. It is moreover doubtful, I think, whether the centennial unit, so called, has become an accepted unit in all parts of the country. In commercial transactions between buyers and sellers of boilers I think it may be positively asserted that this unit has not yet been even extensively adopted.

I would strongly urge, with great deference, however, to the majority of the committee, and to members who may agree with them, that the most rational mode of expressing the unit of boiler-power is to express it in terms of the universally accepted unit of evaporation. This conforms to custom in establishing systems of weights and measures, and also to general practice, which has tacitly recognized the old unit of evaporation as that which is to be employed in estimating the performance of a boiler. I say old unit of evaporation, meaning the "evaporation of one pound of water from and at 212° F." which the committee recommend to be retained; the retention of this unit and the establishment of another and a like unit in connection with the unit of boiler-power seems a duplication of units of a similar kind, which is apt to lead to confusion; and which at least seems unnecessary.

If the unit of boiler-power recommended by the committee be adopted, viz., "30 pounds of water evaporated from feed water of 100° F. temperature and at 70 pounds pressure," which is merely 30 of the new units of evaporation, there will still be needed a reduction to find from the actual performance of a boiler its power estimated by the standard unit. The claim that this new unit of evaporation conforms "more nearly to practice" than the old does not make this reduction any more simple, and has little importance, it seems to me, when we consider what an indefinite expression "average practice" is in this connection.

Whichever unit of evaporation be adopted, whether the old or the new, or both, there is still a want of precision in the recommendations of the committee in defining the "unit of boiler-power," especially if this unit is to be of commercial value in aiding the

buyers and sellers of boilers to a better understanding in their transactions. And this want of exactness is likely, I think, if not corrected, to render of little importance which unit shall be adopted, or whether it is worth while to define the unit of evaporation or unit of power with such extreme precision, while other factors on which the power of a boiler essentially and in a higher degree depends, are introduced under the very general and often embarrassing term "average practice."

In the language of the committee, "what is needed is a *standard unit of boiler-power* which may be used *commercially*, in *rating boilers*, and in *specifications* presenting the power to be *demanded by the purchaser* and *guaranteed by the vender*." (The italics in the quotation are mine.)

This is a distinct and clear statement; and such a unit, if it can be established, will serve a purpose of very great importance, as it will tend to prevent misunderstandings between buyers and sellers of boilers, and thus possess an element of real commercial value.

The principal commercial considerations in the sale and purchase of a boiler are its "capacity" or "power," and its economy. If the unit of boiler-power as defined by the committee is sufficiently exact for commercial transactions, then it must be admitted that capacity for evaporation and economy depend so little on the quantity of fuel burned in a given time, that variations in the performance of the same boiler, or of different boilers, under varying rates of combustion are so slight that these variations need not be taken definitely into account.

The committee define the unit of boiler-power, practically, in the following manner: "An evaporation of 30 pounds of water per hour from a feed water temperature of 100° F. into steam at 70 pounds pressure," "with *easy firing, moderate draught*," "*ordinary fuel, and good economy*."

As far as purchaser and vender are concerned this definition seems to me analogous to one which should define a mode of calculating the cubic contents of a *particular* building by stating that the ground area is to be measured with great care by a standard unit of measure, and the number of square feet in the ground plan should then be multiplied by — the height of an average ordinary building.

To specify more particularly the case under consideration, let us refer to the results of the experiments of Mr. Isherwood, which give the capacity or amount of evaporation, and the economy of the

same boiler under different rates of combustion or draught. Representing the number of pounds of anthracite coal burned per hour on each square foot of grate surface of a marine tubular boiler (having 25 square feet of heating surface to 1 of grate surface), by the numbers in the first horizontal line below,

6.	8.	10.	12.	14.	16.	18.	20.	22.	24.
10.5	10.4	10.1	9.5	8.9	8.2	7.7	7.3	7.	6.8

the numbers in the second horizontal line will represent the number of pounds of water evaporated from and at 212° F. by each pound of coal burned. The figures would apply also very well to all horizontal tubular boilers having the same proportions as the marine tubular boilers of the experiments; and they represent the law for all boilers, in a general way, in regard to the diminution of efficiency or economy with increased draught or rate of combustion.

It is easy to see what a large range of performances might possibly fall under the designation "easy firing," "moderate draught," and "good economy." It is doubtful whether the members of the committee themselves, if asked, would all select the same numbers as the proper interpretation of these terms, if they should decide separately and without consultation with each other. Ordinary buyers and sellers would certainly differ widely, the buyer being more at a disadvantage because it would rest with him to prove, in case of disagreement, that he did *not* get what he bargained for.

A horizontal tubular boiler employed for steam-heating purposes might, for instance, be considered as working under a "moderate draught" if it burned 8 pounds of coal per hour on each square foot of grate surface; and again a boiler absolutely the same in every respect might be demanded for manufacturing purposes where the rate of combustion was to be 14 pounds of coal per hour for each square foot of grate surface. Both might be claimed to answer the definition of "average practice" under the special conditions of use.

The difference in the power and economy of the boiler under these separate conditions may be easily calculated on the basis of the above experiments.

In the first case the boiler evaporates 83.2 pounds of water per hour for each square foot of grate surface, and in the second case 124.6 pounds per hour for each square foot of grate. The power in the second case is just 50 per cent. greater than in the first, while the economy of evaporation is less in the second case by the ratio $\frac{8.9}{10.4}$: or a difference of evaporation per pound of coal of 1.5 pounds

of water, and for each square foot of grate surface a difference of 41.4 pounds of water—a difference which is not measured by a small fraction of the "unit of power" recommended, but which absorbs one and one-third of these units for each square foot of grate surface.

If the boiler has 12 square feet of grate surface it will develop, employing 30 pounds of water as the basis for 1 H.P., 33.28 horse-power, and in the second 49.84 horse-power, a difference of 16.5 horse-power.

This example shows, I think, how important it is in establishing a "unit of boiler-power," which may be "prescribed" in specifications by the purchaser, and "guaranteed by the vender" that some definite rate of combustion or draught should be taken into account, since this is the *principal element* which governs both power and economy.

It might be well also to recommend certain standard proportions of heating to grate surface, etc., for boilers of various classes, inasmuch as these proportions have also an important bearing.

The Novelty Iron Works published, some years ago, an advertising pamphlet, in which were given the proportions of boilers of various classes, and their capacities for evaporation.

The calculations were made, I believe, by Mr. Emery, a member of the committee.

The proportions of heating to grate surface were for

Plain Cylinder	**Boiler**	$\frac{1}{11}$
Cylinder Flue	"	$\frac{1}{17}$
Cylinder Tubular	"	$\frac{1}{27.6}$
Stationary Locomotive	"	$\frac{1}{28}$

The rates of evaporation of these boilers were respectively (the water being supposed fed at a temperature of 60°, and evaporated at 80 pounds pressure) for each square foot of grate surface of the

Plain Cylinder	**Boiler**	**53.**	**pounds.**
Cylinder Flue	"	**60.6**	"
Cylinder Tubular	"	**64.4**	"
Locomotive	"	**66.1**	"

Another shop, in another part of the country, might adopt very different proportions of heating to grate surface, etc., and assume very different rates of combustion; and there would be, and probably is, such diversity of practice in this respect in different parts of the country, that unless some more explicit rule can be recommended than "easy firing, moderate draught, and good economy,"

it would be quite as well to say that the unit of boiler-power shall be an average quantity of water (30 to 40 pounds) evaporated from an average temperature of feed water, at an average pressure, with an average draught and average economy.

I would suggest, however, that the manufacturer or seller of a boiler should be held responsible for the proportions of his boilers, or in other words their economy; this would include the proportion of heating to grate surface, draught areas, etc., and the buyer should be responsible for the draught, over which he has generally exclusive control, and that under these circumstances the "unit of boiler-power" might be (adopting the old unit of evaporation), *an evaporation of* 35 * *pounds of water from and at* 212° *F., when the rate of combustion is not less than* 10 *pounds of ordinary coal on each square foot of grate surface.*

This would not only furnish a fair standard of comparison for different classes of boilers, but it would, with the exception of locomotives, cover the greatest range of commercial transactions.

I am aware that in a trial test it might be difficult to burn exactly 10 pounds of coal on each square foot of grate surface; but some specified minimum rate of combustion seems better than the very general designations of "average" or "moderate"—terms which if used in specifications would be certain to *invite*, rather than to *prevent* and ward off, misunderstandings and litigations. In the example I have given such a misunderstanding might possibly occur involving 16 horse-power in a boiler of very common size.

With the definition of the unit of boiler-power which I have proposed above, such a misunderstanding could not well happen. If ten pounds of coal (or any other proper number of pounds) be adopted as the *minimum* rate for which the boiler-power is guaranteed, the buyer would know that if he burned more than this amount he would get more power than he bargained for with less economy, and if he burned less he would get less power than he bargained for with perhaps greater economy; but the seller would be in no way responsible for the variations in the rates of combustion, as specified in the contract between them, this specification being an essential part of the definition of the "unit of boiler-power."

Since preparing the above I have conferred with Professor C. B. Richards, of the Sheffield Scientific School of Yale College, and

* In the discussion as presented at the meeting this figure read 30 pounds, and the subsequent discussion had reference to that figure.—*Ed.*

am very glad to concur in the following suggestion made by him. In the unit which I have suggested above the question of economy is still left indefinite.

Professor Richards suggests that the rate of combustion be referred to the heating surface instead of to the grate surface. The "Unit of Power" will then include both the rate of combustion and the economy. We concur in the belief, also, that 30 pounds of water evaporated is too low a number even with the committee's unit of evaporation.

It can hardly be said that the *average* non-condensing steam engine supplies 1 horse-power for each 30 pounds of water evaporated, but taking into consideration the recommendation of the committee that when a boiler is ordered it is well to order one which has one-third more capacity than the application of their unit would call for, the following unit of boiler power is presented, amended according to Professor Richards' suggestions: 40 *pounds of water evaporated from and at* 212° *F., with a rate of combustion of not more than* $\frac{4}{10}$ *pounds of good ordinary coal per square foot of water-heating surface.*

For ordinary horizontal tubular boilers this will give an evaporating power of about 3.25 pounds of water per square foot of heating surface, or about eight pounds of water from feed water of 160° F., and at 80 pounds pressure per pound of coal, and about 12 square feet of heating surface will give a horse power.

The adoption by the public of the *Mechanical Engineers' Standard* of boiler-power would then render it unnecessary for purchaser and vender to make any reference to rate of combustion, as it would be implied in the standard unit.

The President.—I think there is perhaps a difference of opinion among engineers, in the first place, as to the pressure under which trials of boilers should be made, and it would be well to discuss that point whether it is advisable to accept the recommendation of the committee as to carrying on a test under a pressure of 70 pounds or atmospheric pressure.

Mr. Kent.—I do not understand that the committee report in favor of any pressure whatever, and Prof. Trowbridge makes no reference to any pressure whatever. The question under discussion is the question of the unit of horse-power.

Mr. Root.—The principal thing it seems to me is to have a unit. It doesn't make much difference what it is. In 1867 I published a rating for horse-power. At that time the question came

up what was a horse-power in steam boilers. Almost every one claimed that a cubic foot per horse-power according to the old English idea was the right thing. Studying up the subject I found that any good engine ought to produce a horse-power on 23 or 24 pounds of water, and it would be a very poor engine that would not produce a horse-power on 30 pounds of water. So I adopted and published that as the rate of the boilers I sold—30 pounds of water evaporated from a temperature of 212°. That, I believe, was the start to this whole thing as to the rating of horse-power of steam boilers. Now I do not see that it makes very much difference so long as it is a settled thing. The question of what a horse-power is becomes very important sometimes, especially where there is a disagreement, and the matter comes before the Courts. It is very important then to have some unit, though as I said I don't know that it makes much difference what that unit is, and while I think that a unit of 30 pounds evaporated from a temperature of 212° is a fair unit, still the Centennial unit is all right, I should think, from the indorsement that it has had by eminent engineers. Now, when it comes to the matter of testing boilers to ascertain their rate of economy, a great deal of time, a great deal of talent has been expended on that, and when you go into a labored test of the steam boiler, and make a calorimeter test, I think I know some engineers that can appreciate the amount of work there is in it.

Then there is another point in regard to it. That is, that people do not generally understand it. You may figure the thing up, and go through with all your different equations, and then put it before a boiler user, "Why," he says, "I don't understand this. For me to put any confidence in this I would have to hire an expensive engineer to interpret it for me." At the time of the Centennial the committee had different ideas as to taking that test. Previous to that time I had gone through with one or two of those tests. I made up my mind that there ought to be a simple way of getting at the evaporative capacity of a boiler. Now the whole point lies in the amount of expansion that is put into the water that is pumped into the boiler, and if you can have something in the shape of a steam meter that you can put on a boiler and take an account of the number of cubic feet of steam that is delivered from that boiler per minute, you have the whole thing. If the boiler primes and carries over water, you don't get so many cubic feet of steam to the pound of coal burned. There is the fact, and it gives

you the whole result; but I think that this matter of steam boiler tests is simply expending too much time in making figures. It would be better to put on the boiler a steam meter that would give you the number of cubic feet of steam that the boiler turns out per pound of coal.

Mr. Kent.—Can you give me such a meter?

Mr. Root.—I can give you a drawing.

Prof. Webb.—Mr. Root seems to suppose that the object of this portion of the report is that the public may be enabled better to understand tests and calculations in which it may be used; it seems to me, however, to be exactly the opposite. A distinguished engineer, now present, said at the Pittsburgh meeting in support of a proposed new name for the unit of evaporation, that the customer would not know how much it was. Now it seems to me evident that after the tests and calculations are completed, and we know that a certain boiler will supply to the water within it so many thermal units per hour, the result of the tests is in a shape to be understood by any business man, who would need to know in addition simply the number of thermal units needed per hour for his particular engine. In order, however, to prevent any such understanding, and for reasons unintelligible to a business man, unscientific, and upon which the committee differed among themselves, we are to divide the number of thermal units by 33,305—no simpler number would do—and call the result some kind of a horse-power. I can appreciate differences in horses, but supposed a horse-power to be a fixed amount of mechanical energy. To be a commercial unit it should be fixed by law. If it is to be called a *commercial* horse-power, Prof. Trowbridge is right in claiming that in any attempt to establish such a unit the exact value chosen for it is of little consequence unless the normal conditions under which it is to be used be laid down with equal definiteness and authority. But it has been said in answer to Prof Trowbridge that it is intended to be *scientific* rather than commercial; I do not see anything scientific in the quantities chosen, or their relations to each other or to fundamental units, nor are the numbers proposed easy to remember or use. These quantities –30 pounds of water, 70 pounds pressure, 34½ evaporation units—how long have they been and how long may we expect them to remain a fair average of every day practice, or have they indeed been shown to *be* such an average in reality. To my mind it would be simpler and better to express results in *thermal units per hour*, and at all events not

to express them in horse-powers which are very far from being horse-powers. After the adoption of such a unit as that proposed, it would seem to me appropriate to fix upon some one notch of the Birmingham, or other, gauge as a unit for the thickness of boiler plates, and to name it a *commercial inch*, so that a customer may have as little knowledge of the actual thickness of his boiler as he will have of its power. Such a standard notch might be made and placed with the Secretary for safe-keeping, with a statement that it was equal to so many thousandths of an inch, and although in actually measuring the thickness of plates a micrometer caliper reading to thousandths of an inch would be used, the result of such measurement would never be used in business until it had been divided by the proper number to reduce it to the "commercial inch." Suppose, for instance, that the No. 1 notch of the Birmingham gauge were adopted; then the number of thousandths would need to be divided by 300 (ten times the number of pounds of water chosen for the "commercial horse-power") to get the number in "commercial inches," and then a customer would be just enough confused as to the thickness of his boiler to give it up with the explanation—that's the way boilers are sold. I begin to understand what this new horse-power is for.

Mr. Kent.—The remarks of Prof. Trowbridge appear to be based upon a misconception of the meaning of the terms used in the report. I do not think it is open to the charge which he makes of want of exactness and precision in the definition of a boiler horse-power. The discussion repeatedly speaks as if there were "two units of evaporation" considered in the report, as in the words "another and different unit of evaporation first introduced by the judges at the Centennial Exhibition."

The report uses the words "unit of evaporation" in one sense only. Only one such unit was considered, and the report is consistent throughout in giving it one definition, namely: one pound of water at 212° F. evaporated into steam of the same temperature. This definition is given on page 8 of the report and is substantially repeated on page 11, where the unit of boiler horse-power is said to be considered to be equal to 34½ units of evaporation; that is, 34½ pounds of water evaporated from a feed-water temperature of 212° F. into steam at the same temperature. The report of the committee cannot possibly be misunderstood upon this subject. The unit of evaporation is a constant of nature of the utmost scientific precision of definition, being the exact amount

of heat needed to evaporate a pound of pure water into steam at the mean atmospheric pressure at the sea level.

The unit of boiler horse-power is an entirely different unit, and is considered in paragraphs 14 and 15 of the report. There was no possible difference of opinion among the members of the committee concerning the definition of a unit of evaporation, but there were numerous differences at first concerning the unit of boiler horse-power to be recommended. So carefully was this matter considered that each member presented his individual views in writing and, after a great deal of argument in which numerous proposed units were considered, a unanimous vote was at last reached in favor of the unit which is defined on page 11:—30 pounds of water from a feed-water temperature of 100° F. into steam at 70 lbs. pressure, or 34½ units of evaporation. The figure of 30 lbs. of steam per horse-power had become so generally considered by engineers as a fair average consumption of steam in engines that it was advisable to adopt that figure if at all possible; but it was also advisable, for the purpose of securing ease in calculation, to make the unit of boiler horse-power a multiple of the unit of evaporation. It was found that 34½ units of evaporation were the equivalent of the evaporation of 30 lbs. of water from 100° F. to 70 lbs. steam pressure—within $\frac{1}{30}$ of one per cent., according to the best steam tables procurable. As this error is far within the limit of error of instrumental observation in boiler testing, and is probably within the limit of error of the steam tables, it was considered right to neglect it and, therefore, after the definition of a boiler horse-power as "an evaporation of 30 lbs. of water per hour from a feed-water temperature of 100° F. into steam at 70 lbs. pressure," there were inserted the words "which shall be considered to be equal to 34½ units of evaporation. This standard is certainly not open to the charge of want of exactness and precision. Prof. Trowbridge says, page 3:

"The committee define the unit of boiler power practically in the following manner:

"An evaporation of 30 lbs. of water per hour from a feed-water temperature of 100° F. into steam at 70 lbs. pressure with easy firing, moderate draught, ordinary fuel and good economy."

It is only by a mixing of two separate paragraphs of the report that any such meaning can be derived from it. The definition on page 11 says: "In all standard trials the commercial horse-power be taken as an evaporation of 30 lbs.," an absolute

and unconditional statement to which the words expressing the condition of "easy firing, moderate draught, ordinary fuel and good economy," have no relation whatever. These quoted words are in a separate paragraph from that in which the standard horse-power is defined and relate to an entirely different subject, namely: the *rating* of boilers, or the horse-power they should be called in selling or advertising them. Is it possible that Prof. Trowbridge has misunderstood what the unit of boiler horse-power is for, and that he thinks it is to be used not as a measure of work actually done; that is, water actually evaporated in a boiler trial, but as a standard for measuring boilers for sale—for giving them a rating in the market? If so, he has misapprehended the object of a boiler test, which is to determine how many horse-power the boiler actually develops at the time, not how many horse-power it should be rated at or sold for. Just as in a test of an engine, we apply an indicator or a brake, not to determine what horse-power the engine should be called in selling it, or in advertising it in a catalogue, but what horse-power it actually develops at the time of trial, and with what economy of steam it develops the same; so in a boiler trial we weigh the water evaporated and the coal used, not to determine the question what the horse-power of the boiler should be called in selling it, but what horse-power it develops under the conditions existing when the test is made, and with what economy it develops such horse-power. The horse-power in an engine test is the measure of the work done during the test, the unit of horse-power being defined as 33,000 foot lbs. of work per minute. The horse-power of a boiler in a boiler test is the work it does during the test, but as the work done by a boiler cannot be expressed in foot-pounds of work as it can in an engine test, we give the term horse-power as applied to boilers a different definition, viz.: 30 pounds of water evaporated. The words "commercial horse-power" or "boiler horse-power," have been used by the committee as a technical term to signify the horse-power of a boiler, as above defined, and to distinguish it from engine horse-power, defined as 33,000 foot-pounds of work per minute. We would have used the word "nominal" instead of "commercial," but it might have been misunderstood as having some relation to the term "nominal horse-power," as it is still used in England in measuring engines, but which is obsolete in this country. The word commercial is perhaps not a good word and may

possibly have led some to suppose it applied to the rating for sale, but no other word was suggested which did not seem more objectionable.

I think the paragraph on page 11 relating to the rating of a boiler is clear enough to the comprehension of most engineers, but it might be paraphrased as follows to make it still plainer: While the commercial horse-power is, as above stated, an evaporation of 30 lbs. of water, etc., which standard is to be used as a measure of work actually done in a boiler test, it is the opinion of the committee that a boiler should not be called 100-horse-power (or rated at 100 horse-power) merely because it evaporated 30 x 100 = 3,000 lbs. of water per hour in a boiler test, unless it is capable of that evaporation with easy firing, moderate draught and ordinary fuel, while exhibiting good economy; and, further, that it should be capable of developing 130 commercial horse-power (such commercial horse-power being defined as above 30 x 130 = 3,900 lbs.) of water per hour.

Prof. Trowbridge, introducing a standard of his own (viz.: an evaporation of 30* pounds of water from and at 212° F. *when the rate of combustion is not less than* 10 *pounds of ordinary coal per square foot of grate surface*), says "in establishing a unit of boiler power which may be prescribed in specifications by the purchaser and guaranteed by the vender some definite rate of combustion or draught should be taken into account, since this is the *principal element* (italicized by Prof. Trowbridge) which governs both power and economy."

As an example of the practical working of such a standard, suppose a boiler rated at 100 horse-power should be guaranteed by the vender to develop 120 horse-power on trial. It is tested and it evaporates 4,500 lbs. of water per hour, and the rate of combustion is only 9 lbs. of coal per square foot of grate. How is it possible to determine the horse-power developed by Prof. Trowbridge's standard? Suppose the same evaporation is obtained when 11 lbs. of coal is reached, what then is the horse-power?

The rate of combustion or draught is not only not the principal element which governs both power and economy, but it is frequently not an element at all, since both power and economy may be made independent of the rate of combustion per square foot of heating-surface or of the force of the draught. A boiler which under certain conditions of draught and rate of combustion de-

* See note on page 319.

velops 100 horse-power with an evaporation of 9 pounds of water per pound of coal, may be made to give the same results with a very different draught and rate of combustion, by simply shortening or lengthening the grate surface to correspond with the increase or diminution of the force of the draught.

The same criticisms apply of course to the other proposed standard of 40 pounds of water evaporated from and at 212° F., with a rate of combustion of not more than $\frac{4}{10}$ pounds of good ordinary coal per square foot of water-heating surface.

Let us apply the new standard to the above special case and assume that the boiler had 1,500 square feet of heating surface, evaporating 4,500 lbs. and burned 500 lbs. of coal per hour. The grate surface now disappears from the problem; the rate of combustion of $\frac{500}{1500} = 0.333$ lbs. per square feet of water heating surface—what is the horse-power obtained in the test? But suppose the heating surface is only 1,000 square feet and the same evaporation is obtained from the same coal but it is burned at $\frac{500}{1000} = 0.5$ lbs. per square foot of water-heating surface, what is the horse-power obtained? Surely, this example is sufficient to show the absurdity of attempting to make an absolute unit of work done dependent upon such a variable condition as the rate of combustion.

Prof. Trowbridge makes another suggestion, that it might be well to recommend certain standard proportions of heating to grate furnace, etc., for boilers of various classes. The committee intentionally refrained from doing anything of the kind. Their duty was to recommend a set of rules for testing boilers, and not a set of rules for building them.

Mr. Babcock.—I agree with Prof. Trowbridge in part. I think that he is sound when he says we do not want two units; one unit is sufficient. Now a unit of evaporation, as the committee says, is a standard thing: it is scientific; it is not dependent on thermometers or on measures of length or breadth; it is definite, and may be easily determined by means of an ordinary balance, and is therefore a proper thing to use. Let us accept that as a unit and measure our horse-power by it. Our committee have done this, but only as an alternative. For regular use they have assumed an entirely arbitrary unit which they call a unit of horse-power. They seem to have been led to that unit because of a desire to retain the number 30, which Mr. Root first published as a measure of horse-power. I see no particular advantage in retaining the 30; and therefore I see no need of making a conventional

unit to fit it. 33 units of evaporation would have given a number which is familiar to engineers in connection with horse-power, being the same as the number of thousand foot pounds in the engine horse-power. 33 units of evaporation and 33,000 foot pounds form something nearly akin, are easily remembered, and seem to be a part of the same general notation.

Another unit is suggested by them, the heat-unit. That is a scientific term and there would seem to be good reasons for adopting it. 33,000 heat units, a measure nearly equivalent to the committee's, would have similar advantages to 33 units of evaporation. Insomuch I agree with Prof. Trowbridge. I do not agree with him in respect to what is a boiler horse-power. It should have nothing whatever to do with the rates of combustion either on a square foot or grate, or a square foot of heating surface. In fact, Prof. Trowbridge's argument overthrows his unit; he shows us that on plain cylinder boilers there should be one square foot of grate to every eleven square feet of heating surface, while on return tubular boilers the proportions should be 1 to 28. Now, then, if you apply his unit of horse-power to those two different boilers it will not fit at all. The conditions destroy each other. There is no question but that a boiler will give a better economy at one rate of evaporation than at another; that is, at one particular rate of evaporation it will give its best economy. There is a maximum. I could not give it in definite figures, they would vary with different boilers, but it would approximate a curve of this form (Fig. 225). If we take for vertical values the quantity of water evaporated, and for horizontal the quantity of coal burned per unit of surface, we will find that the curve of capacity will rise nearly regularly at first, gradually falling off in ratio as the combustion increases. Now if we wish to represent the economy in pounds of water evaporated per pound of coal, it would be by a curve starting from nothing, running up very quickly to a maximum, which will be nearly maintained for a while, then falling away in an increasing ratio. The best work of the boiler would be done within the vertical lines A and B—and its commercial horse-power should be somewhere within that range, which will vary, however, with the kind of boiler. This diagram will answer for any boiler by varying the scales of values.

Mr. Root.—In regard to this matter of economy I think there is just as much in adjusting the rate of combustion and the temperature of the furnace, compared with the terminal temperature

of the flue, as there is between the initial and the terminal pressures in the steam-engine. If you reduce your grate surface and burn a large amount of coal per square foot, the result is, as I have found in a great many cases, to reduce the temperature of the gases passing off through the flue, and the reverse is the case when the furnace temperature is reduced. The result is, when you put up your temperature by a higher rate of combustion in the furnace, that the part of the boiler adjacent to the furnace,

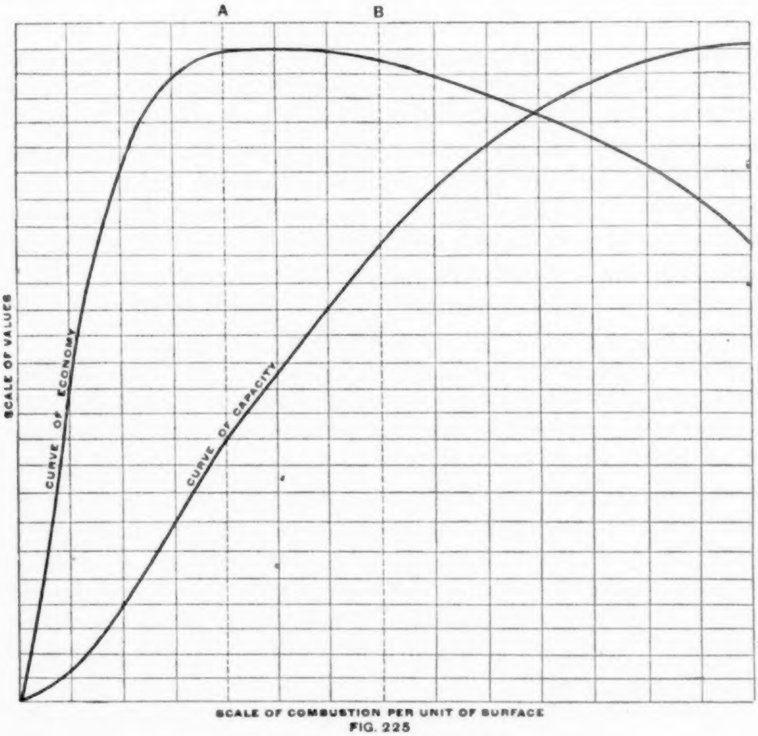


FIG. 225

being subjected to a higher temperature, takes up a great amount of the heat generated by the coal at that point. That leaves a less amount of the heat to be absorbed in the run through the outlet of the flues to the chimney. I found that by reducing the area of the grate and putting up the temperature of the furnace to a very high point I actually reduced the temperature of the gases in the flue. I think that is the point that varies all these equations and curves that you may make, and that results will

be varied by the proper proportion of grate surface and flue areas.

J. F. Holloway.—As I understand, Mr. Kent proposes to eliminate from this discussion a great many of these points which were referred to, and I understand him to say that he simply proposes to give to you the water and the coal and you are to get the best results out of that. He might use as an illustration the fact that the horse-power of a horse going over the country is not dependent on a variety of conditions. In the case of fast horses in a race, where the intent is that the horse shall have a fair start and the one that gets under the wire first is to be considered the best horse, what is known as jockeying often comes in. I do not know that the same thing comes in boiler tests; but if it does, it is in such ways as are suggested by differences in the way the firing is done, and the way the damper is handled, and other such manipulations. The point that Mr. Kent means, I suppose, is that all we have to do with is the result in the end. Having those two elements to begin with, we get the best results out of them —what should be the starting and what should be the end; the intervals between the two being left to the best jockey.

Mr. C. E. Emery.—I feel assured that the pressure of other duties has prevented Prof. Trowbridge from making the thorough examination necessary to appreciate the full value of all the conditions which are to be considered in discussing the unit of boiler-power as suggested in the Report. The discussion states, on its third page: "The committee define the unit of boiler-power practically in the following manner: "An evaporation of 30 pounds of water per hour, from a feed-water temperature of 100° F. into steam at 70 pounds pressure," "with easy firing, moderate draught," "ordinary fuel and good economy." The Report of the committee states:

"Your committee has carefully weighed the arguments relating to these standards, as they were presented in writing by their respective advocates, and, after due consideration, has determined to accept the Centennial Standard, the first above mentioned, and to recommend that in all standard trials the commercial horse-power be taken as *an evaporation of* 30 *pounds of water per hour from a feed-water temperature of* 100° *Fahr. into steam at* 70 *pounds gauge pressure*, which shall be considered to be equal to $34\frac{1}{2}$ units of evaporation, that is, to $34\frac{1}{2}$ pounds of water evaporated from a feed-water temperature of 212° Fahr. into steam at the

same temperature. This standard is equal to 33,305 thermal units per hour.

"It is the opinion of this committee that a boiler rated at any stated number of horse-powers should be capable of developing that power with easy firing, moderate draught and ordinary fuel, while exhibiting good economy; and further, that the boiler should be capable of developing at least one-third more than its rated power to meet emergencies at times when maximum economy is not the most important object to be attained."

With the above quotation before us, where is the want of "exactness and precision" in the unit of boiler power, which is referred to? Is it implied that the unit should be stated in units of evaporation? It is so stated in the Report but not in the quotation from it. It is also stated in thermal units.

It would not be unnatural that criticism should be called out from an active engineer approaching the subject hastily and failing to find in the Report a complete treatise on all the conditions which affect the performance of boilers. It is stated in the discussion: "The principal commercial considerations in the sale and purchase of a boiler are its 'capacity' or 'power' and its 'economy.'" This statement is unobjectionable, but simple as it is, it includes a range of problems entirely outside the scope of the subject referred to the committee, and involves conditions which it would be impossible to embody in formulating rules for general use. The committee attempted only to write a report on "a standard method of conducting steam-boiler trials." The question is, whether the report submitted furnishes rules for conducting such trials with sufficient elaboration to make the results obtained practically accurate and comparable one with another. In order to show the power of a boiler it was necessary to fix a standard of boiler-power, and the power developed by a boiler at any given time can be compared with this standard and accurately stated. The committee cannot dictate to manufacturers and users of boilers all over the country what rate of combustion they shall employ, either per square foot of grate as proposed in the first modification of Prof. Trowbridge, or per square foot of heating surface as proposed in the second modification by Profs. Trowbridge and Richards. One of Prof. Trowbridge's own illustrations is well calculated to prove this. He refers to some of my work for the Novelty Iron Works some 16 years ago, where I gave the proportions of grate to heating surface of a plain cylinder boiler,

1 to 11; of a cylinder flue boiler, 1 to 17; and of a cylinder tubular boiler, 1 to 28. It is well to add, however, that in the circular I gave the relative evaporations as 1 for the plain cylinder, 1.14 for the cylinder flue, and 1.32 for the cylinder tubular, to show purchasers what kind of boilers it was for their interest to buy. The proportions fixed were not notions of my own; they were founded on the average proportions of similar boilers then in use. Such boilers are still in use. For utilizing waste heat where fuel is cheap the plain cylinders will probably continue to be used for years, and on account of bad water the cylinder flue boilers will probably continue to be used in the West, indefinitely. The proposition to conduct boiler trials of these different kinds of boilers at the same rate of combustion per square foot of heating surface is preposterous, as the cylinder flue and tubular boilers would not nearly give the power usually obtained from them in regular practice.* Moreover, the proportions of many other boilers sold in the market vary nearly as much as those referred to, so the proposed modification would be rarely applicable. The purpose of a boiler trial is not to dictate what the power and economy shall be, but to ascertain what they are.

Of course the same boiler can be operated at different powers, and of course a very large increase in the power of a boiler may be obtained without very largely reducing its economy, but the power and economy during each particular trial can be ascertained definitely by the rules presented.

The particular quantity of water evaporated selected as one horse-power should not be confounded with the question of what shall be the rated horse-power of a boiler. The manufacturer of a boiler may rate his boiler at any power he chooses. If a test be made, the actual power developed under the particular conditions will be ascertained under the rules given. If the question, as to whether or not a boiler is of a given power, be submitted to an engineer, he may by measurement of its size and proportions, in connection with its chimney and setting, determine approximately how much water it will evaporate, and thereby determine its power by the rule, or he may obtain the results more accurately by actual trial. If the builder has guaranteed a certain economy, that can be determined beyond peradventure. There will be no dispute about it, simply because there is only one way of expressing economy.

* Only eleven twenty-eighths of regular power in one case and seventeen twenty-eighths in the other.

There have been difficulties however when only a certain power was guaranteed, for the reason that there were great differences of opinion as to the value of a horse-power, which it is now proposed to settle. The two questions are entirely independent. An engineer in determining whether or not a boiler is of a certain horse-power under the rules, should be guided by the suggestion of the committee that the boiler should produce "that power with easy firing, moderate draught, and ordinary fuel," and further that the boiler "should be capable of developing at least one-third more than its rated power to meet emergencies at times when maximum economy is not the most important object to be attained."

Evidently a boiler must have some surplus power in order to enable the fireman to keep the steam pressure steady when manipulating his fires, when the day is dull or the fuel poor. Frequently, too, the regular fireman is absent, and there are many other contingencies which will at once suggest themselves to the practical engineer. Personally I should like to have seen the provision of one-third surplus power made more prominent. The committee after a full discussion thought it should come in simply as a suggestion, since the actual power developed in a particular trial could always be stated in terms of the standard. The members of the committee differed somewhat in opinion at first, more particularly however as to details, and it required considerable discussion, but I am happy to say little compromise, to bring about a full agreement. When the committee had nearly finished their work and were settling the details of the Report, it was informally suggested to make the standard exactly 33,000 British Thermal Units per hour, so that it would be numerically the same as the number of foot-pounds per minute constituting an actual horse-power, and again 33,333 B. T. U. were suggested to facilitate the calculations, but the general feeling of the committee was against any change whatever. The members had come to think alike. They believed that the standard fixed by the Judges of the Centennial Exhibition so nearly expressed their convictions as to what the value of a horse-power should be that it should be adopted without question. The members who were not on the Centennial Committee were apparently most earnest in opposing any change whatever.

Mr. Babcock thinks that the value of a horse-power should be stated only in "units of evaporation," and other speakers feel that confusion will arise by adopting any other standard. It will

be observed, however, that the committee fixed the standard in three alternative forms, in one of which it is stated in "units of evaporation." The object of first stating the standard in the form used by the Centennial Committee, to wit: 30 pounds of water evaporated per horse-power per hour at a gauge pressure of 70 pounds from a temperature of 100°, and afterwards stating that this is equivalent to 34½ units of evaporation or pounds of water evaporated from and at 212°, is simply that the former more nearly expresses the result under actual practical conditions. Engines are not operated at atmospheric pressure, and the feed water is rarely 212°; but very many engines are operated at or about 70 pounds pressure, and the feed-water temperature of 100° is easily obtained in average practice. If a manufacturer compares the amount of coal and water used for a given time, as shown by his coal bills and a properly connected water meter, he should be able to ascertain an average number of horse-powers used, approximating that shown by an elaborate trial. If an engine requires 30 pounds of feed water per horse-power per hour, the boiler must furnish that amount of power for each horse-power under actual conditions, and any owner or practical superintendent who had watched a trial as closely as some do, on inspecting a report stated in units of evaporation would say that his engine did not require 34½ pounds of water per horse-power per hour, but only 30 pounds, as he had checked the figures. The practical man would be right. Water evaporated from and at 212° certainly has nothing to do with an *engine* trial. *The engine and boiler units should correspond*, so that the boiler will furnish and the engine use not only the same actual quantity but numerically the same number of units of water. Hence *the horse-power of a boiler should be expressed primarily in such manner that scientific corrections to a standard basis will not greatly vary the apparent result.*

Prof. Webb, in his remarks, goes still further and claims that the British Thermal Unit should always be used, and that the other methods of stating the value of a horse-power only confuse the matter. What would the practical boiler owners and superintendents say about confusion if the results were only stated in British Thermal Units? The answer is evident. Now which should govern, language comprehensible to men who own and operate the thousands of horse-power of boilers and engines now in use, or to those who have little or nothing, practically, to do with them? It is the duty of the schools to teach principles, but to

learn to apply them in practicable ways. We should all keep in mind Rankine's definition of Science, which is, substantially, that it is a combination of *Theory* and *Practice*. In this matter it seemed better to the committee to announce a practical standard that all could understand, and then fix its value definitely on a scientific basis. *The value of the unit of horse-power announced is* 33,305 *British Thermal Units per hour*, which being stated in the Report definitely fixes the standard. It also equals $34\frac{1}{2}$ units of evaporation, within one-thirtieth of one per cent., and this value is also stated so that parties who desire to use it in that form can do so.

It may be interesting also to put on record here some reasons why 30 pounds of water per horse-power per hour, evaporated under actual conditions, is considered equal to one horse-power, rather than to any greater or less amount. Very small engines use 40, 50, and sometimes even 60 pounds of water per horse-power per hour, while large engines working expansively, in factories, require 22 to 24 pounds only, some results being reported still lower. Compound engines well adapted for their work require still less. Small engines are, however, generally furnished in connection with the boilers which are to supply them with steam. In many cases the engine and boilers are practically a unit, as in the case of portable engines. So also for factory purposes boilers are furnished to supply particular engines with steam, and generally by the parties furnishing the engines—in any case by those who understand most of the conditions of the problem. The unit of boiler horse-power should therefore be most applicable to intermediate sizes. It is a fact easily proven that from 50 to 75 horse-power can readily be obtained in a modern engine, using steam expansively but non-condensing, for less than 30 pounds of water per horse-power per hour, which indicates that 30 pounds is ample to allow for the increased consumption due to such ordinary derangements as occur in average practice. It was thought, however, that 30 pounds of water evaporated per hour from and at 212°, which is the equivalent of only 26.1 pounds of water, evaporated under average actual conditions, would be insufficient to produce a horse-power in average engines of moderate size. The standard selected, 30 pounds of water, evaporated at 70 pounds gauge pressure from a temperature of 100°, was considered to be the better compromise, one which more nearly corresponded with the views expressed by others previously, and which moreover

possessed the advantage of having been previously promulgated by the judges who conducted the test at the Centennial Exhibition.

Mr. Root has suggested that boilers should be tested with a steam meter in order to avoid the necessity of many of the elaborate details provided for in the code. I applied a steam meter to a boiler lately, and was very much surprised in observing it. The boiler was one of twenty or more supplying steam to our mains. The discharge from the boiler was through a check valve, and the meter showed that the boiler took a rest every time it was fed with fuel or water. No matter how carefully these operations were performed, no steam passed out of the boiler for a little time afterwards. This could not have taken place if the boiler had been operated singly to furnish a regular supply of steam. In such case the water in the boiler would have kept up the supply temporarily after firing or supplying feed to the boiler, and the pressure would have varied slightly. In the case referred to, the other boilers kept up the pressure, and the boiler simply stopped generating steam momentarily until there was a slight excess of pressure within it to raise the check valve. The quantity of steam passing through the meter was shown by the position of a pencil moving over a ribbon traversed by clock-work, so that the records were a series of hills and hollows varying with the management of the boiler, condition of the fire, etc. The depressions which showed that no steam was passing were very short generally.

This statement will be deficient without a brief explanation of the construction of the meter. The velocity of steam in our pipes, for the losses of pressure desired, is about 80 feet per second, so that it was practically impossible to construct a displacement meter. Such a meter would necessarily have as large a displacement per unit of time as the engine it was supplying. Several velocimeters were tried, but, as is common with this class of mechanism, they varied their rate not only with the quantity flowing through, which could have been allowed for, but with the friction of the apparatus, and the latter, in steam machinery, is liable to variation on account of wear and the difficulty of maintaining lubrication. We had to go back to the principle of blowing steam through a graduated orifice with a constant head or difference of pressure. This method is as accurate as any other, depending as it does upon the well-known principle of the laws of gravitation. The meter I developed consists substantially of a piston valve regulating rectan-

gular openings of definite size and operated by a weighted piston, the weight being so adjusted to the diameter of the piston as to require 2 lbs. difference of pressure to lift it, and this difference of pressure is by means of the weighted piston automatically maintained between the inlet and discharge sides of the meter openings. The motion of the piston is transferred outside the cylinder by means of a rock shaft and levers, and its position is continuously recorded on a ribbon of paper set in motion by a clock, as previously explained. A fixed pencil also marks on the paper a line of reference. The heights of the diagrams at various points are in proportion to the quantities of steam flowing at the respective times, the average height is proportioned to the average quantity of steam used, while the area is proportioned to the total quantity of steam used for the time considered. In practice the areas are ascertained by means of special planimeters designed for the purpose. The necessity of using a clock and recording mechanism on every meter was at first thought to be a misfortune, but it proved a blessing in disguise. One great drawback of the steam business is that janitors and employees will waste it. They leave it on at night so that the buildings will be warm in the morning, and not require them to be on hand as early as they would if the steam had been shut off. In many places during the winter steam is not shut off from one week's end to another, and is consequently used 168 hours in the week instead of about 60, as expected. In well-ventilated buildings the quantity used during the night does not appear to vary materially from that used during the day, so that naturally great complaints arise as to the amount of the bills. Owners are disposed to believe the statements of their janitors or other employees whom they have trusted so many years, and not until they are confronted with the charts showing carelessness, and perhaps have tested the accuracy of the registers by privately seeing steam shut down at particular times and then asking for reports from us, will they believe that the amounts of the bills are due to a want of care on their own premises, and not to the greediness of the corporation, as they are too willing to claim.

Mr. Root.—I see that the gentleman has come to the meter idea after all, and I think it would be well if that would take the place, in the reports of the trials of steam boilers, of so much figuring and so many equations that the majority of people cannot understand. I will ask Mr. Emery how accurate this appears to be and

how much trouble there is in working it. Is it liable to get out of order? If the weight of the steam is increased by being loaded with water I should think the rate of flow should be changed.

Mr. C. E. Emery.—If it is in order I will be happy to say anything in regard to the matter. No experiments have been made to ascertain the variations in flow due to moist steam. Our large pipes act as steam drums, the water of condensation separates readily and flows along the bottom until it is removed by a trap, so that the quality of the steam is about the same at all places. In regard to the accuracy of the apparatus, I will say that each meter is accurate when it works freely. We have attempted to make the meters exact duplicates, so as to avoid rating every one by actual test. This involved all the difficulties in any manufacture due to making parts interchangeable, which have been successfully overcome. We are however still rating every meter by actual test, so that there will be no question as to the accuracy of the records. It is desirable to have the meters fit closely and yet move with absolute freedom. The meters are made reliable by using comparatively heavy weights. The piston of a $1\frac{1}{2}$ inch meter weighs 30 lbs., so if the resistance to motion be $\frac{1}{10}$ of a lb. the difference of pressure is affected $\frac{1}{300}$ of 2 lbs. or $\frac{1}{150}$ of 1 lb. We find that some of the meter cylinders slightly change shape after being heated up for a time and it is necessary to scrape the bearing points. We have however some meters in use two years which have not been touched. Fortunately small resistances do not affect the results. Variations in demand cause a movement of the piston, and even when supplying a constant quantity of steam for heat a slight variation of steam in the boiler-house will cause slight variations in the pencil record of the meter; the pencil rising as the steam pressure falls and falling as the steam pressure rises, within very small limits. The record is therefore evidently correct for the average steam pressure. The slight undulations also correct errors due to the minor resistances, for the pencil falls as much short of reaching the absolutely correct higher limit as it does of reaching the corresponding lower limit, so that the average is absolutely correct. Lastly, the slack motion in the connections is also eliminated in the same way, when it is less in amount than the movement of the pencil due to reasons named. We are thus able to study the diagrams the same as indicator diagrams, and to tell from their appearance whether or not the meter is in order. A straight line on the diagram is

always regarded with suspicion. A test of the meter is immediately made to see if it will respond to changes in the position of the delivery valve. We desire to see a slight undulation in the line even where the demand for steam is very regular.

Mr. Barrus.—I regret that the Report of the committee does not recommend a simple standard method of making calorimetric tests of the dryness of steam. But perhaps the memorandum describing Mr. Emery's barrel calorimeter was thought sufficient. If so, I beg to criticise the method he describes. In one respect, I believe it is inaccurate. My object in writing this, is, 1st, to state wherein the inaccuracy consists; 2d, to present for consideration the data and results of some experiments that support my views; and 3d, to offer a self-evident remedy.

Mr. Emery's memorandum on the thirty-fifth page of the Report and the 290th of the volume (next to the last paragraph) reads, "The weight of water in calorimeter should be increased proportionally to the weight and specific heat of all metal exposed to changes of temperature with the water. An addition of one-ninth of the weight of the propeller, and submerged portion of shaft and fastenings, will be substantially correct, if the apparatus be made of iron."

I have used the barrel calorimeter to a considerable extent, and have found that the wooden material of the barrel itself absorbs heat derived from the steam, in the same way as the metal referred to, but to less extent. I have been in the habit of making an allowance for the effect of the wood, and as it amounts to from 1 to 3 per cent. of the heat given up by the steam, it has appeared to me an important correction. Mr. Emory's memorandum makes no reference to the matter, and herein the method appears to me in error.

In support of this view, I submit the following data and results of some experiments with a barrel calorimeter, which I have undertaken for this special object.

The calorimeter was constructed essentially like the one Mr. Emery's memorandum recommends. Its principal difference was in the propeller, which was made of pine wood. There was no metal in the barrel excepting two or three iron plugs screwed in from the outside, used for stopping holes; and the $1\frac{1}{4}$-inch iron nipple with its elbow and outlet valve, which was attached to the bottom.

The wood of the barrel was oak, three-quarters of an inch thick. The barrel held, when filled, about 350 lbs. of water.

Although the object of the experiments was to find the amount of heat absorbed by the wood of the barrel, they were made to embrace complete calorimetric tests.

The method adopted was as follows: The barrel was filled to a certain mark and steam admitted till the temperature reached as near as might be 110° F. The propeller was set to work to equalize the temperature, and then the barrel was emptied. After observing the weight of the empty barrel, the cold water supply valve was opened and the barrel filled to the same mark as before. During the operation of filling, the temperature of the inflowing water was observed at intervals of half a minute, a thermometer being set in the supply pipe for the purpose. It required about three minutes' time for the complete filling. The temperature of the water in the barrel was again made uniform by the use of the propeller, and the temperature observed from a thermometer held in the hand and immersed in the open barrel. The weight of the filled barrel was taken and steam blown through the hose till all the drip disappeared. Then the hose was dropped into the barrel and steam admitted with full force. When the temperature of the water reached 110°, as near as might be, the steam was shut off, the hose carefully removed from the barrel, and the propeller operated till the temperature became uniform. The weight and temperature were again observed, and the experiment was finished. This course was followed in each experiment of the series, and the same length of time was occupied in similar operations.

Four consecutive trials gave respectively for total heat of the steam, 1197, 1191, 1198, 1199.

The full log of the third trial is given below, and is representative of all the others.

1. Steam of previous trial shut off at....................2.49—30.
2. Water of previous trial emptied at....................2.54—30.
   Temperature at 111.7° F.
3. At 2.58- 0 Weight of empty barrel................82 lbs. 4oz.

	Time.		Temperature.
4.	At 2 58-30	Open cold water supply......................	53.5°
	" 59- 0	" " " "	50.8
	" 59-30	" " " "	50.6
	" 3 00- 0	" " " "	50.6
	00-30	" " " "	50.5
	" 01- 0	" " " "	50.5
	01-30	Shut off cold water......................	50.5
		Average......................	50.8°

5. At 3 03-30	Weight of barrel	383 lbs. 10 oz.
6. " 3 05- 0	Temperature of water in barrel	53.3°
7. " 3 07- 0	Open steam valve.	
8. " 3 12- 0	Shut steam valve.	
9. " 3 14- 0	Weight of barrel	400 lbs. 6 oz.
10. " 3 15- 0	Temperature of water in barrel	111.6°
Steam pressure by gauge		71.5 lbs.

It will be seen from this log that the cold water gained in passing into the barrel 2.5° of temperature. Subsequent experiments showed that 0.5 of a degree was absorbed from the atmosphere, which had a temperature of 88° F. The remaining 2.0 degrees is that due to the heat that was stored in the wood on the previous experiment. This is $3\frac{4}{10}$ per cent. of the heat given out by the steam. In the other experiments of the series, the number of degrees received from the wood was, in the first 1.7°, in the second 1.9°, and in the fourth 1.8°. I hold that these quantities measure the allowance that should be made for the specific heat of the wood in barrel. In this particular calorimeter the allowance amounts to over 30 thermal units of total heat, which corresponds to what would be called over three per cent. of moisture in the steam.

The remedy for the error of Mr. Emery's method, if my reasoning is sound, is to take for the initial temperature, not that of the cold water in the barrel, but that of the cold water flowing into the barrel corrected for the heat derived from the atmosphere.

I would say that the scales used in weighing the water on the tests were sensitive to a change of half an ounce in weight, and the thermometers, graduated to fifths of a degree, could be easily read to tenths, and were found substantially correct when compared with each other and with standards.

I am indebted to Prof. Lanza of the Mass. Institute of Technology, where the tests were made, for permission to use some of the apparatus in the steam engineering laboratory of the Institute for the purpose.

I would call attention also to the fact that the report of the committee suggests no rules for the treatment of the unburned and unconsumed coal, which, during the progress of a test, falls through the grates. Tests that are made for commercial purposes call for the determination of the amount of *coal* consumed, without regard to the amount of *combustible.* In the case of anthracite coal an appreciable percentage of the coal that is fired finds its way through the grates into the ash pit, either in an unburned or in a partially burned condition. The amount depends somewhat upon the width

of air spaces in the grates, but however small the spaces may be, within the limits of practical use, small pieces of coal may always be found in the ashes.

I would suggest that in the case of anthracite coals which are larger in size than those termed "buckwheat," the pieces of coal, whether partially or wholly unburned, which will pass through a screen having meshes one-quarter inch square in the clear, be classed as *ashes*, and those that fail to pass through such a screen be classed as *refuse coal*. The weight of the refuse coal should be deducted from that of the coal which is fired, in order to determine the weight of coal actually consumed. My own custom has been to select and screen a sample of the ashes, refuse coal, and clinkers, and thus determine the proportion which the refuse coal bears to the whole weight, without going through the labor of screening the whole quantity.

Since the above was written, I have repeated the calorimeter experiments, using, instead of the wooden barrel, a metal tank.

The tank was $19\frac{1}{2}$ inches in diameter, $23\frac{1}{2}$ inches deep, and was made of No. 15 B. W. G. wrought iron, the inside of which was tinned. It had two handles, but no other attachments. Its weight was 34 pounds 14 ounces, and all of this, except a rim of metal at the top three inches wide, representing about 1.28 pounds weight, was exposed to the changes of temperature of the water. In using this calorimeter, according to the directions of Mr. Emery's memorandum, the weight of water should be increased by $\frac{33.6}{9} = 3\frac{7}{10}$ pounds, provided the vessel be of iron. It may be regarded as wholly iron, the tin lining being very thin.

Calorimeter tests were made with this apparatus in a similar manner to that described for the barrel calorimeter, except that only one initial temperature of the cold water was observed, and that *after* the water had entered the tank.

Three consecutive experiments, made under the same conditions of place and circumstances as those made with the barrel calorimeter, gave for total heat of the steam, 1196, 1200, and 1197 respectively. The data of the first of these tests, which is representative of all, are as follows:

1. At 12.28 Weight of empty tank including the hair felt underneath.... ....37 lbs.
2. At 12.31 Weight of tank filled with cold water....251 lbs. $8\frac{1}{2}$ oz.
3. At 12.33 Temperature of cold water in tank............52.9°
4. At 12.34 Let on steam.
5. At 12.37 Shut off steam.

6. At 12.40 Weight of tank filled with cold water....261 lbs. 2 oz.
7. At 12.41½ Temperature of warm water in tank..........101.1°
Pressure of steam by gauge..............................73 lbs.
Temperature of atmosphere...............................73°

The close agreement that exists between the results obtained by the two methods, is proof, to my mind, of the correctness of the method described for working the barrel calorimeter.

I would submit also the following formal suggestions, as covering these and some of the other points in which I think the Report of the committee could be improved.

1. Amendment to Section X.

Change the beginning of the second paragraph so as to read as follows:

At the end of the test, remove the whole fire *as soon as one-fourth part, as near as may be estimated, has become dead,* clean the grates, etc.

2. Amendment to Section XIV.

Insert the following words between the first and second paragraphs:

On all except tests for scientific purposes, the standard form of calorimeter shall be one of the barrel type, made of iron or other metal, suitable correction for the thermal equivalent of which shall be allowed. In conducting calorimetric tests the final temperature of the water shall be carried only so far above the temperature of the surrounding atmosphere as the initial temperature is below that point.

3. Amendment to the tabular record, Section XVII.

Insert between lines No. 19 and No. 20 the following new line:

Proportion of refuse that fails to pass through a screen having meshes one-quarter of an inch square in the clear.

Mr. C. E. Emery.—Mr. Barrus was kind enough to send me a copy of his intended discussion in relation to a proposed method of correcting the results when using a barrel calorimeter to allow for the specific heat of the wood in the barrel. It is true that the influence of any mass of material may be ascertained from its weight and specific heat, or, as Mr. Barrus proposes, by the change such mass makes in the temperature of another mass of known weight; for instance, the water in the calorimeter.

A correction for the specific heat of the wood in the barrel is desirable if its influence is of any considerable moment. Mr. Barrus does not fix the amount of the correction with sufficient definiteness to prove that it is important. If his experiments are worked out in detail, it will be found that they show superheating when the temperature of the entering water is taken before, instead of after, it enters the barrel. A portion of the increased temperature he ascribes to the absorption of heat from the air

but he does not state how he determines this amount. I communicated with him, asking distinctly in regard to this, but he does not answer the question. Are we to infer since the experiments by his method will show superheating, that all the heat in excess of what he considers ought to have been shown, should be credited as having been derived from the air? Unless he explains how his correction for the air was obtained we have a right to assume that both the air correction and the correction for the barrel are determined from the same experiments, or else that his data are deficient and therefore of no value. Of course if we explain the correction the first way, a wide field is opened at once for conducting calorimeter experiments according to any person's notion of how much heat is derived from the air. By reducing the quantity of heat supposed to be derived from the air it is possible to increase that derived from the steam, as the sum of the two is what is shown by the experiments. If this be considered unfair, then where is the fairness in assuming that the quantity of heat derived from the air would be constant under varying conditions? The question arises: What would be such difference when the hose is held six inches above the water's surface compared with that derived when it is held sixteen inches above it? What would be the difference in result when the stream is directed against the stirring apparatus or the side of the barrel so as to become dispersed and thereby expose a larger quantity of surface to the air compared with that when the stream enters unbroken?

The work Mr. Barrus has undertaken is desirable, as all additional investigation in relation to physical experiments is desirable, but certainly in the shape he has left it, no valuable information is conveyed. The wooden apparatus he used for stirring was undoubtedly well calculated to absorb water, which, as it was heated and cooled, would have something to do with the changes of temperature which he describes. It is evidently better to make the propeller or stirring apparatus of iron, which will not absorb water and which will change temperature rapidly so as to correspond to that of the water, and the weight of which can be accurately ascertained. Similar considerations apply to the barrel or vessel itself which holds the water. It is undoubtedly better to make it of metal, as thin as it will keep its shape, to protect it carefully with felt or even eider down, if available, as Mr. Hoadley did with his calorimeter, and then allow for the weight of metal heated and cooled with the water, according to the rule given in my memo-

randa. An apparatus of this kind is somewhat permanent in its nature and rather bulky to send to a distance. The results with barrel calorimeters, when the experiments are made in the manner suggested in my memoranda, have heretofore agreed fairly well with the results obtained with more delicate apparatus, and the barrel method has therefore been considered sufficiently accurate for practical purposes. The absorption of water by the wood of the barrel probably makes more difference with the results than anything else. In most cases I have used barrels previously employed for holding oil or spirituous liquor, and did not suppose that there could be any great error in the results, due to the specific heat of the wood. I think it would be well thoroughly to oil the interior surface of the barrel in any case, so as to prevent the absorption of water.

I trust Mr. Barrus will not be discouraged, but continue his experiments until he is willing to publish the entire details, and obtains results which will correspond among themselves. While on this subject, I wish particularly to call attention to the necessity of publishing all the details of experiments, even those which do not appear to conform to others. The ascertainment of general laws has been postponed simply from the lack of detail in the records of previous experiments. Full details will frequently aid others in tracing a law the underlying principles of which differ altogether from preconceived notions. Some engineers think it necessary to omit from their records any mention of deficiencies in the apparatus used, which might appear to militate against the accuracy of the results of the experiments, imagining that their own good intentions will excuse such omission. I have known a really capable engineer to furnish an engine-builder with elaborate tables of the performance of his engine under certain varying conditions which were to be tested, without any reference whatever in the report to the fact that the boiler from which the steam was derived leaked an amount equaling some fifteen to twenty-five per cent. of the amount of steam used by the engine. It may be that the method was not very inaccurate for merely comparative experiments, using about the same quantity of steam in each case; but to say that these particular experiments should be published to the world in comparison with others made with other engines, under different conditions, would be very questionable, as the absolute amount of water leaking from a boiler when not supplying steam freely, might vary considerably

when the circulation of the water was active and the fires burning briskly. In any case all conditions should be stated in the report sufficiently in detail to enable others to judge of the manner in which the experiments were conducted and their general reliability—it being necessary to assume the honesty and good intentions of the observer in all cases until the contrary is proven.

Mr. Kent.—Referring to the note of Mr. Barrus upon the treatment of unconsumed coal which falls through the grates during a boiler test, I am of the opinion that no allowance should be made for such coal, but that it should be counted in with the ashes. In a commercial test made to determine the relative commercial value of two kinds of coal, it is evident that, other things being equal, the coal which, through extreme friability or other cause, crumbles into dust and runs through the grates is less valuable in practice than one which holds itself up on the grates. If allowance should be made for the dust or small particles of such coal which fall through the grates, it might lead to an erroneous conclusion in regard to the commercial value of such coal.

If allowance should be made, as Mr. Barrus suggests, it might be claimed that allowance should also be made for such unconsumed coal as was blown into the chimney by the force of the draught; and also for the heat wasted by carrying too much air through the fire. The suggestion that "at the end of the test the pieces of coal, whether partially or wholly unburned, that pass through a screen having meshes $\frac{1}{4}$ inch square be classed as ashes, and those that fail to pass through such a screen be classed as refuse coal" is liable to several objections. Such a treatment of the refuse adds another complication to the boiler test, which is already sufficiently troublesome, and would not be likely to meet general approval by all who have to make boiler tests. Such an arbitrary classification of refuse coal and ashes is inaccurate and unscientific, and the extent of its inaccuracy will be different with different coals.

If a boiler test is made to determine the absolute economy of the boiler with reference to its filling a guarantee, it is not fair to the purchaser of the boiler to make an allowance for the coal passing through the grates, since such coal is actually lost in practice, and, if allowed for, the result of the test would be larger than could be obtained in practice. If the test is made to determine not the absolute economy, but the comparative economy between two furnaces, or two methods of firing, or two kinds of coal, one of

the elements of value in any particular furnace, method of firing, or kind of coal, is that of the quantity of coal which goes through the grates. In such case, therefore, it would be improper to allow for such coal.

Mr. Barrus's suggestion to select and screen a sample of the ashes, refuse coal and clinkers, instead of screening the whole quantity, is also open to the objection that, to select a sample which should be an absolute average of such a heterogeneous mass, which varies in its composition during every hour of the test, would be a matter of considerable difficulty, and there would always be doubt as to its accuracy.

Mr. Barrus.—In view of Mr. Kent's objections, so far as they apply to tests for determining the economy between different methods of firing, different kinds of coal, and, it might be added, different forms of grates, I would suggest that in tests of this kind the amount of *refuse coal* that passes through the grates be determined and the result recorded, but that no allowance be made for the same in determining the weight of coal consumed. In tests for all other purposes, however, and especially in those which are made to find the efficiency of the boiler itself, I would strenuously urge that the allowance be made for this refuse coal. There is such a variety of forms of grates in use on the same types of boilers that the matter appears to me an imperative one.

I must utter my protest against Mr. Kent's objection that the treatment named "adds another complication to the boiler test, which is already sufficiently troublesome, and would not be likely to meet general approval by all who have to make boiler tests." The person who finds or expects to make boiler testing *anything else but troublesome*, has had a different experience from mine in the matter. We cannot make or expect to make the work easy, but we can do the work right if it is worth while to do it at all. It is my wish that the code be one which can be referred to, and, as far as possible, followed without qualification.

The burden of Mr. Emery's criticism to my note regarding the allowance to be made for the specific heat of the wood of a barrel calorimeter, appears to be that in my experiments, the heat added to the water might be derived to a principal extent from the atmosphere.

I would say that this could not be the case. The 0.5° that is given for the heat derived from this source was obtained in the following manner: The barrel was filled to the mark with cold

water and emptied, and this was done a number of times so as to abstract all of the heat from the wood of the barrel. The water from the supply pipe was then admitted in the same manner and under the same circumstances as described for conducting a calorimetric test, the temperature of the inflowing water was observed, and, after having been made uniform by working the propeller, the temperature was observed of the water in the barrel. This test was made again with identical results, and I repeat that the heat derived from the atmosphere added 0.5° to the water while that derived from the atmosphere and from the hot wood of the calorimeter on the main tests added from $2\frac{2}{10}$ to $2\frac{5}{10}$ degrees.

I would be glad to hear from Mr. Emery as to how he can reconcile the close agreement of the two sets of tests, one made with a wooden tank with no other allowance than that of using for the initial temperature that of the *inflowing water* corrected for the heat derived from the atmosphere, and the other being made with a metal tank with an allowance according to his own method.

(ADDED SINCE THE MEETING.)

Mr. Barrus.—Prof. Lanza has called my attention to a reason for the varying results which often occur in a series of calorimetric tests, although they all appear to be made in precisely the same manner. He suggests that the variations may be caused by changes in the temperature of the steam pipe due to changes of pressure. If the pressure is rising when the test is made, the pipe absorbs heat from the steam and causes an increase in the amount of moisture. If, on the other hand, the pressure is falling, heat that has been stored in the metal goes back to the steam and causes a decrease in the amount of moisture. Experimental evidence, which I will refer to, leads me to believe that this effect may become a powerful one. I was once engaged in testing the quality of steam taken from the exhaust pipe of a 15 horse-power non-condensing engine working with saturated steam. A series of tests was made to determine the effect of various back pressures. A high back pressure was carried for the first test and subsequently lower pressures were tried. In one instance the test with a lower pressure was made directly after the new conditions for that test had been established. The result came out very differently from the preceding one. There was a large decrease in the amount of moisture shown. A little reflection made it plain that the metal of the cylinder and exhaust pipe, which had been overheated by

the high pressure of the previous trial, had not had time to assume their normal temperature before the second experiment was made. Hence the variation in the results of the calorimetric tests.

Mr. Babcock.—The admirable Report of the committee is to be commended. It is so complete, and discusses the several points so fully that there is little more can be said upon the subject. The question of the best method of determining the condition of the steam, as to dryness, is one of considerable importance. The appendices to the Report point out some of the difficulties in the way of such a determination, and suggest several points to be observed in order to secure accuracy. None too great stress is laid upon them. With the best apparatus, and the greatest care, it is doubtful if even approximately accurate results can be obtained. I have plotted a number of series of such tests, and have never yet found one in which there were not very erratic results. In several instances the average led to impossible conclusions, as, for instance, superheated steam with no superheating surface. Mr. Emery has pointed out the possibility of this being due to the imparting of heat from the steam, in consequence of a reduction of pressure in the pipe through which the quantity tested was taken. Though, theoretically, this is possible, the quantity of heat which might be imparted is quite insignificant, and could not materially affect the result.

To show this, let us take the construction used at the Centennial test, where Mr. Emery first pointed out this possible action. The calorimeter was supplied through a $\frac{3}{4}$ inch pipe inserted horizontally across the main pipe, and provided with perforations of greater area than the pipe, directed toward the current of steam. Outside the main pipe an inserted nipple reduced the bore to $\frac{1}{2}$ inch, to regulate the velocity of flow, and at the end of the $\frac{3}{4}$ inch pipe was a common $\frac{1}{2}$ inch globe valve, connecting with a 1 inch rubber hose which was inserted in the water. The pipe was well felted. We thus have two half-inch openings with a chamber between. As demonstrated by Prof. Blake, the pressure in the intermediate chamber under such circumstances is $\frac{4}{5}$ the initial pressure, which in this case was 85 pounds absolute; and by Rankine's ready formula ($\frac{p}{70}$ = the flow of steam in pounds per second per unit of area into a pressure less than one half of p), we find $\frac{85}{70} \times \frac{4}{5} \times 60 = 58.3$ lbs. per minute outflow for 1 inch area. The area of opening in the $\frac{1}{2}$ inch valve is 0.3 inch, and the coefficient of such an opening is 0.8; hence $58.3 \times 0.3 \times 0.8 = 13.99$, say 14 lbs. actual

discharge per minute. What would be the reduction in pressure, due to such a flow, within the portion of the pipe inclosed in the steam? We have no record of the openings into the pipe, but as they are stated to have been in excess of the area of the pipe, it is fair to treat it as if the end of the pipe was open. The head required to overcome the resistance of the opening, and produce the velocity is $h = 1.505 \frac{v^2}{2g}$, and is measured by $h = \frac{144\delta}{D}$; δ being the difference in pressure per square inch, and D the density, or weight per cubic foot, of the steam. But $v = \frac{W}{Da}$, W being weight delivered per second, and a = area of opening in square feet. Substituting and reducing, we get

$$\delta = .000162 \frac{W^2}{Da^2},$$

or, when W is weight per minute, and a is expressed in square inches,

$$\delta = .000933 \frac{W^2}{Da^2}.$$

The true area of a "$\frac{3}{4}$" inch pipe is 0.553 sq. inches, and the weight per cubic foot of steam at 85 lbs. is 0.198. W, we have seen, is 14. Substituting we find $\delta = 3.25$ lbs. loss of pressure, equal to a difference of temperature of 3° between the steam in main pipe and that in the calorimeter pipe.

If this latter were filled with water, it would transmit thereto 330 heat units per hour per square foot, internal surface per degree difference of temperature. It is not probable that it would do the same to steam, but we will assume that it may. If the pipe projects into the steam 6 inches, its internal surface would be $\frac{2.6 \times 6}{144}$ $= .108$ sq. ft. and $\frac{.108 \times 330 \times 3}{60} = 1.782$ heat units, transmitted to the steam per minute. But the 14 lbs. steam discharged per minute represent $14 \times 1178 = 16492$ heat units, and $\frac{1.782}{16492} = .000108$, or $\frac{1}{100}$ of 1 per cent. possible superheating.

In this calculation I have made no account of the effect of friction in the pipe, which would modify it somewhat, but only to reduce the quantity delivered, and therefore the proportional superheating; for the quantity varies directly as the velocity, while the head, and consequent difference of temperature, varies as the square of the velocity.

But this almost infinitesimal amount of superheating would be

more than lost before the steam reaches the barrel of the calorimeter, by radiation from the pipe, be it ever so carefully felted. A $\frac{3}{4}$ inch pipe, covered with 1 inch wool felt, and canvased, will lose 24 heat units per hour, per foot run. If we assume that this pipe was 10 feet long, which is probably a fair average, the loss would be $\frac{24 \times 10}{60} = 4$ heat units per minute, or $\frac{4}{1.782} = 2.24$ times as much as the possible superheating. It will be necessary, therefore, to look somewhere else for the cause of a calorimeter test showing superheating, where such a result is impossible.

PAPERS

OF THE

ATLANTIC CITY MEETING.

XIth.

PROCEEDINGS

OF THE

ATLANTIC CITY MEETING.

XIth.

Local Committee of Arrangements:—Wm. Kent, *Chairman;* H. C. Francis, S. A. Hand, F. R. Hutton, S. T. Williams.

First Day.

The meeting was called to order at eight o'clock, on Tuesday evening, May 26, by Mr. Hand, who introduced Mayor Maxwell of Atlantic City. He made a brief address of welcome, as follows:

Mr. President and Gentlemen:—The pleasant duty has fallen upon me to say a word of welcome to you on your first visit as an organization to Atlantic City. I feel free to say for our city that she feels proud that she has been selected as the spot in which you deliberate for the interests of your Association for the year 1885. We are glad that you are here, for the reason that we are anxious to promote the welfare and interests of our sea-side home, and we expect that your presence and deliberations in reference to the science of which you are the representatives will be of some political benefit in improving if possible our sanitary condition. The gods help those who help themselves, and while we appreciate the fact that we are surrounded by pure air and other necessary adjuncts to good health, we recognize the necessity of seizing upon every possible means for assisting the forces of nature in maintaining unimpaired the high estimation in the minds of the people we have possessed as a healthful resort. I might be pardoned, perhaps, were I to refer to the many excellences concentrated around Atlantic City. I might refer to our dry atmosphere, excellent bathing, fishing, gunning and sailing facilities,

our schools, churches and benevolent associations, our sea-side homes, memorial homes, our fair women and brave men; but I do not propose to inflict upon you a speech. I shall leave all these to be personally examined by yourselves and await with confidence your verdict, satisfying myself in extending to you all, collectively and individually, a hearty welcome to our city, and expressing the hope that your stay among us may be pleasant and profitable to you and of benefit to our goodly city.

The President responded as follows to the address of welcome:

President Holloway:—While it is my good fortune to reply to the very hearty and generous welcome with which your Honor has been pleased to greet us, on behalf of your citizens and yourself, it is the ill fortune of our Society, that this duty has not fallen upon one better fitted for its accomplishment. This gathering of our members at a noted sea-side watering-place, for the purpose of holding a meeting, is a new departure in our history. It has been our practice in the past, when we were able to devote a few hours to business and recreation, to seek some spot abounding in smoky chimneys and dusty thoroughfares, where, surrounded with rumbling wheels, roaring blast and hissing steam-pipes, we proceeded to enjoy ourselves by climbing up and down cinder-heaps, tumbling over scrap-heaps and piles of pig-iron or rails, half blinded with the glare of roaring furnaces, and nearly melted by their heat. We came to the close of the day, begrimed with our surroundings, wilted as to our attire, but full of the conviction that we had been having a delightful picnic.

From what you have been pleased to say to us, and from what we have been able to observe since our arrival, it is evident that no such picnic awaits us here. So for the time being the Mechanical Engineer will have to forego his usual experiences, and assume, for the present at least, the new and doubtless to many the untried character of a lounger by the seashore, a tide-waiter at the beach. But, sir, knowing as I do, the unbounded resources of the gentlemen you see before you, I have no hesitation in assuring you, that they will readily adjust themselves to their surroundings, and that with the hearty welcome which they have received, will in no great length of time, prove equal to any emergency that may arise.

I have heard with much interest and satisfaction of the merits of your city as a winter resort, and I may say that if the warmth of

your climate then, equals the warmth of your welcome now, Atlantic City must be a good place to come to at all times.

Thanking you in behalf of those present, for the kind words you have spoken at this, our opening meeting, I will only add, I sincerely hope that during our stay among you, many pleasant acquaintances may be made on both sides, and that at the close we shall leave with you, as I am sure we shall take away, pleasant memories of the Atlantic City Meeting of the American Society of Mechanical Engineers.

Instead of presenting one of the papers at this session, the evening was spent in discussing two questions presented by the Committee on Queries. Messrs. Bergner, Towne, Root, Kent, Green, Sweet, Babcock, Walker, Partridge, O. Smith, Stratton and the President took part in the remarks made in answer to the question: "Are welded boilers and flues stronger and stiffer than riveted work, and have they merits and defects not found in riveted work?" The second question was in regard to the relation existing, as determined by experience, between the surface speed of a bearing and the pressure which it will sustain. Messrs. Towne, Babcock, Durfee, Hamilton, Bancroft, Green, O. Smith, Kent, Walker, A. H. Emery, Lewis, Hawkins, Sweet, Stratton, and the President spoke upon it. These discussions are given in full under an appropriate heading among the papers of the meeting. After announcements by the President and Secretary, the session adjourned for a social reunion of the members present.

Second Day.

The meeting was called to order at ten o'clock A.M., Wednesday, May 27, in Bartlett Hall, by President Holloway. The Secretary's registers showed the following members in attendance at the sessions:

Babcock, Geo. H.	New York City.
Baldwin, Stephen W.	New York City.
Bailey, Jackson	New York City.
Bancroft, J. Sellers	Philadelphia, Pa.
Barrus, Geo. H.	Boston, Mass.
Beardsley, Arthur	Swarthmore, Pa.
Bergner, Theo	Philadelphia, Pa.
Bond, Geo. M.	Hartford, Conn.
Cheney, Walter L.	Hartford, Conn.
Colwell, A. W	New York City.
Copeland, Chas. W.	New York City.

Couch, Alfred B.	Philadelphia, Pa.
Creelman, W. J.	Rochester, N. Y.
Davis, E. F. C.	Pottsville, Pa.
Dodge, James M.	Philadelphia, Pa.
Douglas, E. V.	Philadelphia, Pa.
Durfee, W. F.	Bridgeport, Conn.
Emery, Albert H.	Stamford, Conn.
Emery, Chas. E.	New York City.
Geer, James H.	Johnstown, Pa.
Gill, John L., Jr.	Philadelphia, Pa.
Gold, Samuel F.	Englewood, N. J.
Good, Wm. E.	Reading, Pa.
Green, Howell	Jeanesville, Pa.
Hamilton, Homer	Youngstown, Ohio.
Hand, S. Ashton	Toughkenamon, Pa.
Hawkins, John T.	Taunton, Mass.
Hazard, Vincent G.	Philadelphia, Pa.
Hollingsworth, Sumner	Boston, Mass.
Holloway, J. F., *President*	Cleveland, Ohio.
Hutton, F. R., *Secretary*	New York City.
Jones, Henry C.	Wilmington, Del.
Jones, Wm. R.	Pittsburgh, Pa.
Kent, Wm.	New York City.
Lewis, Wilfred	Philadelphia, Pa.
Lipe, Chas. E.	Syracuse, N. Y.
Merrick, J. V.	Philadelphia, Pa.
Moore, Lycurgus B.	New York City.
Morse, Chas. M.	New York City.
Murray, S. W.	Milton, Pa.
Parker, Walter E.	Lawrence, Mass.
Partridge, Wm. E.	New York City.
Philips, Ferdinand	Philadelphia, Pa.
Pusey, Chas. W.	Wilmington, Del.
Randolph, L. S.	Susquehanna, Pa.
Robinson, A. Wells	Montreal, Can.
Robinson, J. M.	New York City.
Root, John B.	Greenpoint, N. Y.
Schuhmann, Geo.	Reading, Pa.
See, Horace	Philadelphia, Pa.
Smith, Geo. H.	Providence, R. I.
Smith, Oberlin	Bridgeton, N. J.
Snell, Henry J.	Philadelphia, Pa.
Spies, Albert	New York City.
Sperry, Chas.	Port Washington, N. Y.
Stiles, Norman C.	Middletown, Conn.
Stirling, Allan	New York City.
Stratton, E. Platt	College Point, N. Y.
Swasey, Ambrose	Cleveland, O.
Sweet, John E.	Syracuse, N. Y.
Thompson, Erwin W.	Thomasville, Ga.
Thorne, Wm. H.	Philadelphia, Pa.

Towne, Henry R. . . . . Stamford, Conn.
Walker, John . . . . Cleveland, O.
Webb, J. Burkitt . . . . Ithaca, N. Y.
Whiting, Chas. W . . . . Pottsville, Pa.
Whiting, S. B . . . . Pottsville, Pa.
Wilder, Moses G . . . . Philadelphia, Pa.
Williams, Samuel T . . . . Tacony, Phila., Pa.
Williamson, Wm. C . . . . Philadelphia, Pa.
Woodbury, C. J. H . . . . Boston, Mass.

The Secretary read the following report from the Council to the Society:

REPORT OF THE COUNCIL TO THE AMERICAN SOCIETY OF MECHANICAL ENGINEERS.

The membership has already been advised, by circular and otherwise, of many of the decisions of the Council since the last meeting. The notice as to the collection of dues, etc., through the Secretary's office, as to the discussion of topics without previous reading of a formal paper, as to the change in the address of the Society's headquarters, have all been the result of decisions of the Council. The instruction of the Society in reference to official and individual memorials as to reform in the Patent Office have also been duly carried out.

The Council has held six meetings for the transaction of business since last November. The standing committees have regularly reported and their action has been confirmed, and the duty of scrutinizing applications for membership has been carefully performed.

The committee on standards for method of test and of sizes of test specimens has been appointed as directed by the Society, and consists of Messrs. Egleston, Henning, C. H. Morgan, Thurston, and Towne. A committee has been appointed to confer with similar committees of other Engineering Societies in reference to a plan for a Joint Library for the use of the several Societies. This committee consists of Messrs. Stirling, Durfee, Trowbridge, Oberlin Smith, and Partridge.

The Council has passed the following resolutions:

Resolved, That no description of any engineering or mechanical device or methods shall be accepted to be read at any meeting of this Society unless the same shall have been approved by successful use. But this restriction may in any case be waived by a unanimous vote of the Publication Committee.

Resolved, That the time allotted to any member for the reading or presentation of a paper at any meeting of the Society be limited to a time not exceeding fifteen

minutes, and that when the full paper cannot be read in that time the author shall submit it by a condensed abstract or digest. Provided, however, that this rule may be suspended as to any paper by a majority vote of the members present at such meeting.

The Council would submit the following report of members elected at this meeting in pursuance of Article 13 of the Rules.

NEW YORK, *May* 22, 1885.

The undersigned who were appointed a committee of the Council to act as tellers to count the ballots cast for or against each of the persons proposed for membership in the Society of Mechanical Engineers, to be voted for previous to the spring meeting, 1885, hereby certify that we met this day at the office of the Society and proceeded to discharge our duties.

There were cast in all 314 ballots, but five ballots were thrown out because of informality, the name of the member voting not having been affixed, and all the persons whose names appear on the ensuing list were duly elected in accordance with the Rules to their respective grades:

MEMBERS.

AITKEN, ROBERT W.
ALLDERDICE, W. H.
ASHWORTH, DANIEL.
BARNES, PHINEHAS.
BARR, J. N.
BASSETT, NORMAN C.
BILGRAM, HUGO.
BULKLEY, HENRY W.
CREELMAN, W. J.
CROUTHERS, JAS. A.
DAGRON, JAS. G.
DAVIS, ISAAC H.
DEBES, J. C.
DE KINDER, J. J.
DOANE, WM. H.
FELTON, EDGAR C.
FRICK, A. O.
GRISWOLD, FRANK L.
HALSEY, JAS. T.
HAYES, GEO.
HENDERSON, ALEX.
HERMAN, LUDWIG.
HOLBROOK, ELLIOT.
HYDE, CHAS. E.
INSLEE, WM. H.
JACOBI, A. H.
JENKINS, JOHN.
KELLY, O. W.
LAWRANCE, J. P. S.
LIVERMORE, CHAS. W.
MARTENS, FERDINAND.
MUCKLÉ, M. R.
PERKINS, GEO. H.
PHILIPS, FERDINAND.
POTTER, CHAS., JR.
ROBERTS, EDWARD P.
ROBINSON, J. M.
RUMELY, W. N.
SCHLEICHER, ADOLPH.
SMITH, C. A.
SWEENEY, JOHN N.
WEBSTER, JOHN H.
WELLS, J. LELAND.

JUNIORS.

BEACH, CHAS. S.
FOSTER, ERNEST H.
MOORE, W. J. P.
PARSONS, H. DE B.
ROMMEL, C. E.
SCOTT, FRANK A.
TORRANCE, K.
VAN DUZER, H.
WHITING, CHAS. W.

PROMOTION TO MEMBERSHIP.

HIGGINS, SAM'L, JUNIOR, A. S. M. E.

ASSOCIATE.

EMERSON, B. F.

(*Signed*), WM. LEE CHURCH,
CHAS. C. WORTHINGTON,
Tellers.

Since the first of January the deaths of the following members have been reported:

J. H. Burnett	January 1, 1885.
Horace Lord	February 8, 1885.
D. H. Hotchkiss	April 29, 1885.

The addition of the above names makes the roll of membership of the various grades of the Society as follows:

Honorary Members	12
Life Members	5
Members	539
Associates	24
Juniors	30
Total	610

Respectfully submitted,

BY THE COUNCIL.

In the absence of the Treasurer in Europe, the following statement of the finances of the Society was presented:

REPORT FROM THE FINANCE COMMITTEE, *May* 25, 1885.

The following report of the finances of the Society is herewith presented:

Balance received from C. W. Copeland, retiring Treasurer, November 7th, 1884	$751 03	
Total receipts to date	7,236 80	
	$7,987 83	
Total disbursements to date		$6,066 25
Balance on hand and in Bank		$1,921 57

The above balance includes the sum of $559.40 which has been contributed towards the Library Fund, and has been deposited drawing interest in a savings bank. There is still due to the Society from the membership the sum of $773.49, and the initiation fees and dues of the members elect amount to $1,310, payable within the present fiscal year of the Society.

An earnest request is made that the gentlemen who have not yet made their payments to the Society which were due last November, will lose no time in remitting the sums due from them.

The expenses for the rest of the year will make a heavy drain upon the balance standing to the credit of the Society.

Presented by,

F. R. HUTTON, *Secretary.*

On behalf of the standing committee of the Society on the Library, the Chairman, Mr. Towne, made the following report:

Mr. Towne.—Mr. Chairman, on behalf of the Library Committee I can simply say that the project has opened very satisfactorily. There have been responses, from quite a large portion of the membership, to the circular that was sent out inviting subscriptions, so that at present there is to the credit of the Library fund, as has just been stated by the Secretary, $559.40. This has been contributed by 112 members, and if the whole membership will respond in a similar manner during the coming year, the fund will grow rapidly and satisfactorily. I would like to remind the members present that the plan provides for continuing subscriptions, to be paid annually, and while probably a majority of the members may not be disposed to make large contributions at any one time, certainly a great many can afford to give one or two or three dollars a year in addition to the present low dues to the Society. If this is done, the total amount we shall get in that manner in the course of a few years will give a very satisfactory foundation for our Library. At present the Society accommodations make no provision for the storage of any large library, but it is hoped that before our fund grows to a point which will make that a pressing matter some solution will have been found of the question of a Library site and building. In the mean time the books that are received and that may be purchased will be kept at the Society rooms in New York. The Library project contemplates, you will remember, the providing of books for circulating among members out of the city. In brief, the proposal is that there shall always be a full set of the Society's books in the Library in New York, and that valuable books or rare books shall not be taken from it, except by a vote of the council, I think; but that, in addition, if it seems the desire of the membership, there shall be provided duplicate or triplicate copies—any required number—of books of frequent reference, which shall be available for the

whole membership, and can be called for from any point and sent from the Library to any member, to be kept by him a reasonable time and then returned. In that way the benefit of the Library will be made co-extensive with the membership.

In regard to the finding of permanent quarters for the Library, I may state that several meetings have been held of the joint committee of this Society and the American Society of Civil Engineers, which committees were appointed to confer together with regard to the question of providing a joint library for the Engineering Societies. At the last meeting of that joint committee there were present by invitation representatives of the Institute of Mining Engineers and of the new Society of Electrical Engineers, and the project I perhaps ought also to say, is very cordially entertained by the other society—the Society of Civil Engineers. On the part of the Mining Engineers there is no reason why it should not be cordially entertained, as they at present, like ourselves, are without any library or permanent place for one. I think every one who has considered the subject at all is heartily in favor of and desirous of seeing carried out, a plan whereby the whole Engineering fraternity of the country can be brought together in one building, which shall be the engineering headquarters of this country; and all, of course, agree that New York City is the proper location for such a building. At present the fact that the Society of Civil Engineers has a building, and has quite a large library, makes it a little difficult to say just how the younger societies should unite with them without making, at the outset, some contribution which shall be proportionate to the value of what the Civil Engineers already have; but probably some solution of this question can be found. In general the proposition, so far as it has been discussed in detail at all, is that the building shall be located in the upper part of New York City, somewhere between Twenty-third and Forty-second streets, near Broadway or Fifth Avenue, and that the first effort should be to find some capitalist or capitalists who may be willing to furnish the money required for erecting such a building on the assurance of a reasonable return on the investment. This may require some guaranty provision on the part of the associated societies. A building in that part of the city, with accommodation adequate for all of our societies, and with proper utilization of the land, which would include a building with stores in its lower part and with other rooms for rental in the upper part, would be so much beyond

the financial possibilities of any or all of the societies, that such a plan can only be carried out by the aid of outside capital; but that capital may be available if it is clearly proved that the investment would be a paying one. To erect such a building in that location it is estimated would involve an expenditure of anywhere from a quarter to a half of a million dollars; but the accommodation in it would not merely suffice for the uniting societies, but would also give a good deal of other room which could be rented, and which would contribute to the revenue. If such a building were designed at present the purpose would be to have in it a suitable meeting hall, which should be available for any of the societies at the time of their annual or other meetings, a library hall adequate to contain the contemplated library and its additions, which would also be available to the membership of all the societies, and, in addition, a proper suite of rooms for each society, which would be their head-quarters, and where the secretary of each would have his office, and its business be conducted. After providing for all of these, a building of the height that it is now customary to erect in New York, and covering the area that would be needed, would contain also provision for three or four stores, and for a large number of rooms or offices to be rented to other parties, unless, as has also been suggested, the whole building should be applied to society purposes, and after providing for the Engineering Societies, the remaining rooms be rented to some of the other Scientific societies having their head-quarters in New York, such as the Geographical and Historical. There are a large number, I believe something over a hundred in all, of societies of that kind in the city.

All of this is, of course, looking a good way into the future, and yet it is looking toward what I think we may hope to see within a reasonable time. The committee can simply report progress at present in this matter of joint action, and as to our own library fund we report, as I have said, satisfactory progress for the past year, and urge that the membership should all join in contributing to it, in no matter how small amounts, and preferably by the method of annual subscriptions rather than by one subscription of a somewhat larger sum which would terminate the assistance of that particular member to the project.

At the conclusion of these reports, the Report of the committee of the Society on A Standard Method for Conducting Steam Boiler Trials was presented to the Society for its action upon it. Mr. Towne presented the following resolution:

Resolved, That the report of the Committee on Boiler Tests be accepted and the Committee discharged.

Resolved, That in accepting this report the Society hereby extends its thanks to the Committee for the very full, careful, and intelligent consideration which it has given to the important matters committed to it, and for the thorough and complete manner in which it has embodied the results of its labor in the report hereby submitted, which latter will prove of great interest to all engaged in steam engineering.

The resolution was carried, and the Report came before the Society for discussion. Prof. Trowbridge and Mr. Barrus had forwarded remarks which had been printed, and these as amended appear under number CLXVIII-A of this volume of Transactions. Messrs. Kent, Root, Webb, Babcock, Emery, and Barrus took part in the discussion as to the recommendations of the committee in reference to Standard Units. In closing the debate, Mr. Kent, as chairman of the committee, presented the matter of having the Society officially adopt the code and unit of boiler-power proposed by the committee by means of a letter-ballot. This suggestion brought on a prolonged discussion as to the policy of the Society in adopting any action suggested by a committee in this way. It has been thought advisable that this discussion should be printed in full in an appendix as putting on record the policy of the Society in this matter. While the trend of the discussion was decidedly favorable to the Report, yet the prevailing sentiment was averse to the formal adoption of it, as a matter of precedent and policy. The discussion was closed by the passage of the following motion presented by Mr. Durfee:

Resolved, That the Report of this Committee and the discussion thus far thereon, be printed in the next volume of the Transactions, and that further discussion of the Report be dispensed with, and the whole subject of its adoption by the Society be laid upon the table.

This motion was duly seconded and carried, the discussion having run over into the second session of the day.

At the close of this discussion, the Secretary read the paper, "Notes on the Steam Stamp," by Fredk. G. Coggin, of Lake Linden, Mich. Messrs. Couch, Sweet, A. H. Emery, and O. Smith took part in the discussion.

Mr. T. W. Hugo's paper on "Belts as Grain Conveyers," elicited discussion from Messrs. Kent, Holloway, Babcock, Thompson, A. H. Emery, Dodge, Stratton, Couch, and O. Smith.

The paper by Mr. W. E. Ward, "Early Experiences in the Flow of Metals," elicited discussion from Messrs. Durfee, A. H. Emery, Dodge, Hamilton, Kent, Hawkins, O. Smith, Webb, Babcock,

Towne, Walker, Partridge, and Stratton. At the close of the discussion before adjournment, the President announced the appointment under the Rules of the Committee to nominate officers of the Society, to supply the places of those whose terms of office expire November, 18 5. That committee consisted of

Geo. H. Babcock	New York City.
J. Sellers Bancroft	Phila., Pa.
Geo. H. Barrus	Boston, Mass.
Homer Hamilton	Youngstown, O.
Geo. H. Smith	Providence, R. I.

After some announcements by the Secretary, in reference to the evening and the following day, and in reference to special facilities for visiting Absecom Light-house, the session adjourned. Wednesday evening, a reception and reunion of the members and their ladies was held in the Hall. A number of the residents were invited, and refreshments were served at about ten o'clock. Music and humorous recitations were a pleasant feature of the evening.

Third Day.

The morning session, Thursday, May 28th, was called to order at ten o'clock. Mr. Kent presented his paper on "The Torsion Balance," which was illustrated by samples of scales as now made, and by an illustrative one made by the author without facilities for such work. Messrs. Emery and Hawkins took part in the discussion which followed.

Mr. Stirling's paper on "Shell and Water-Tube Boilers" was read, but discussion on it was deferred until the afternoon to allow Prof. G. I. Alden's paper on "Technical Training at the Worcester Free Institute" to be read and discussed. On the subject of Technical Education for engineers which was thus opened, Messrs. Webb, Kent, W. R. Jones, Partridge, Emery, Dodge, Stratton, Babcock and Green, spoke at some length.

The discussions on the papers so far presented had been so copious that but a few of the papers on the docket had been reached. It was therefore proposed that an extra session be held on the afternoon of Thursday for reading and discussion of papers, although this half-day had been set apart for an excursion by boat on the Inlet. This motion duly presented was carried, and the extra session was ordered. The ladies and some of the gentlemen carried out the original plan and went on the excursion, but the professional session was also a success.

In the afternoon session, Mr. Stirling's paper read in the morning, received discussion from Messrs. C. E. Emery, See, Towne, Kent, Durfee, A. H. Emery, Bancroft, Stratton, Babcock, Root, and W. R. Jones. Owing to the pressure of business, Mr. Stirling was to be allowed to append his rejoinders in writing to the remarks in discussion.

Mr. Hawkins.—Before reading the next paper I would like to ask a question for information. I expected to take a little part in the discussion this morning with reference to technical education. I had occasion to decide that problem for myself about what to do with a boy and I made some little investigations into that subject. I would like to know if it is in accordance with the rules of the association to publish in the Transactions remarks that were expected to be made but which were not made for lack of time. If such be the case I should like to contribute some remarks on that subject.

The Secretary.—I would answer Mr. Hawkins' question by saying that the Publication Committee have allowed the widest latitude in that respect. Where a gentleman submits remarks of which he has given due notice at or before the meeting, the committee receives them, and where no rejoinder is required, those remarks are usually allowed to appear in the printed Transactions. They are declined, however, if anything in them would necessitate rejoinder from other participants in debate. The Secretary will be very glad to receive under these restrictions any amplification of discussion which the members feel inclined to send in for which no time was afforded at the meeting.

Mr. Towne.—I simply wish to add that this practice is very customary in the English Society, and it is a very desirable one. It is often the case in the English proceedings to see the discussion of papers by members who were not present at the meeting and from foreign members who could not go to any of the meetings. No discussion about any topic is apt to suffer from its reduction to writing instead of being stated orally at the meetings. So I think in every way it is desirable to encourage participation in debate by filing written papers.

The Secretary.—It has been proposed that the paper on the "Finance of Lubrication," by Prof. Thurston, and the paper on the "Power required to Drive Shafting and its Cost," by J. T. Henthorn, should be read and discussed together.

The Secretary read both these papers.

Mr. Barrus read his discussion on the latter paper, and Messrs. Emery, Bancroft, Babcock, Hawkins, Sweet and Thompson took part in the joint discussion which followed. Mr. Thompson read his paper on the "Manufacture of Cotton-seed Oil," Messrs. Kent, Barrus, Babcock, O. Smith and C. E. Emery taking part in the discussion.

In the evening of Thursday, the subscription banquet was held at the Colonnade Hotel, and was an enjoyable occasion.

Fourth Day.

The sixth, and closing session, was held on Friday, May 29th, at ten o'clock. Mr. Weightman's paper on "The Oxidation of Metals and the Bower-Barff Process" was read first, and Messrs. Green, Davis, Woodbury and Towne discussed it. Messrs. Kent, Babcock, Barrus, Halloway and Partridge discussed the paper by Mr. J. W. Anderson on "The Adaptation of a Steam Generator for Warming Dwellings."

The paper by Mr. G. C. Henning on "Apparatus for Use in Testing Materials" elicited discussion by Messrs. Kent and Bond. Mr. Emery's paper on "A Polar Planimeter" received no discussion.

Mr. J. C. Hoadley had presented a very complete and exhaustive report on a series of tests made to determine the efficiency of an apparatus for supplying warm blast under a steam boiler furnace, utilizing the heat of the waste gases. This paper was too full and elaborate for advance publication but was presented in abstract. Messrs. Towne, Barrus, Woodbury and Babcock spoke after its presentation, and Mr. Towne presented the following resolution which was duly seconded and carried :

Resolved, that the thanks of the Society are hereby tendered to the corporations enumerated below uniting in the tests reported by Mr. Hoadley for their liberality in permitting the publication of the results arrived at. The thanks of the Society are also due to Mr. Hoadley for the exceedingly valuable nature of the Report in which he has embodied the results of so much work. The corporations are :

Obadiah Marland	Boston.
Pacific Mills	Lawrence.
Boston M'f'g Co.	Waltham.
Naumkeag Steam Cotton Co.	Salem.
Atlantic Cotton Mill	Lowell.
Massachusetts Cotton Mill	Lowell.
Great Falls M'f'g Co.	Great Falls.
Manchester Mills	Manchester.

D. S. Warren & Co.....................................Cumberland Mills.
Merrimack M'f'g Co.....................................Lowell.
Boott Cotton Mills.....................................Lowell.
Salmon Falls M'f'g Co..................................Salmon Falls.
Nashua M'f'g Co..Nashua.
Lancaster Mills..Clinton.

At the close of the Docket of Papers, the remaining questions on the list of the Committee on Queries were presented.

Messrs. Walker, See, Kent, Sweet, and Davis presented facts as to the relation of pitch to the linear speed of gear.

Giving facts in reference to the relation of heating surface to volume to be heated, Messrs. Towne, Barrus, Kent, Partridge, Walker, Geer, Durfee and Woodbury spoke.

The question as to where to put a dome on a locomotive boiler elicited no responses.

Messrs. Thompson, Kent, Partridge, Davis, Towne and Woodbury spoke in reply to queries as to the best system for cataloguing and indexing information and pamphlets.

On the query, How fast do steel springs open and shut on the release of their load, Messrs. O. Smith, Babcock and Bancroft spoke, up to the hour of adjournment.

After a few closing remarks by the President, referring to the success of the meeting, and the probability of the next session being held in the city of Boston, the motion to adjourn was put and carried, at one o'clock.

CLXX.

NOTES ON THE STEAM STAMP.

BY FREDERICK G. COGGIN, CALUMET AND HECLA MILLS, LAKE LINDEN, MICH.

THE steam stamp was evidently an offspring from the steam hammer, the first idea of which seems to have come from the fertile brain of James Nasmyth in 1836. Twenty years elapsed, however, before the idea was first adapted to the purpose of stamping rock, by Mr. Wm. Ball of Chicopee, Mass., who was the first to introduce the steam stamp.

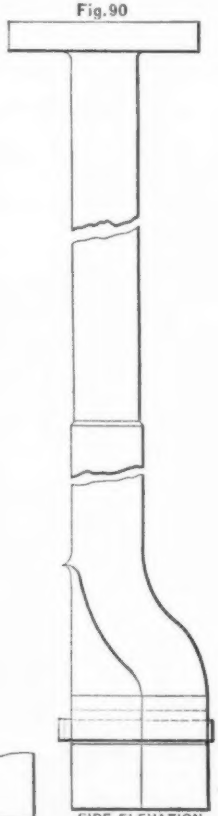
It would be interesting to trace the history of the steam stamp through all the changes of the next twenty-eight years from the first Ball Stamp of 1856. This was a crude machine, wasteful of fuel, with a stamping capacity of scarcely fifty tons per twenty-four hours, while the Leavitt Cut-off Stamp of to-day has an average capacity of 230 tons of conglomerate rock per twenty-four hours. Unfortunately for this early history, Mr. Wm. Ball has passed away, and it cannot be expected that a great deal of the desirable material which remains in the hands of his son, Mr. E. P. Ball, will ever be contributed to the stock of knowledge in this field. We must therefore be content with what data we can get, through the memory of those still living who were familiar with the construction and operation of the first stamps, and who have had experience with the improvements from the beginning up to the present time, and were witnesses of them.

In May, 1856, Mr. Wm. Ball took out his first patent on a steam stamp, Fig. 89 being a reproduction of the Patent Office drawing. His only claim was the long counter-bore a, into which the piston passed, when from any cause the stamp shaft dropped too low, allowing the steam to pass by it so that the stamp would stop. The counter-bore a^2, at the top, was designed, as he specifies, "in order that the piston may not be subjected to unequal wear." These recesses will be referred to further on.

The first stamps of Mr. Ball's design were made by the Ames Manufacturing Co. of Chicopee, Mass., for Commodore R. F. Stockton, for his mine in South Carolina, several of them being sent there in the latter part of 1856. The cylinders were 9″ in diameter with a

stroke of 24 inches. The stamp shaft was 6 inches in diameter, having an offset foot locally dubbed a "sheep's foot" (Fig. 90). Figs. 91 and 92 give the shape of the shoe and method of attach-

Fig.90


SIDE ELEVATION

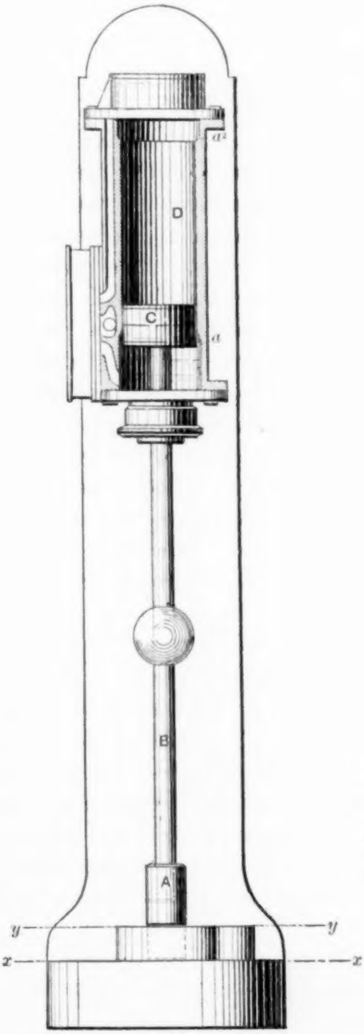
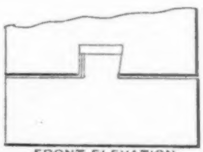
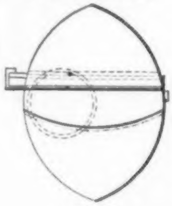


Fig.89


FRONT ELEVATION
Fig.92


PLAN
Fig.91

ment. The former has been changed, but the latter is the same to-day as then. It is both simple and effective.

The valve gear was driven independently of the stamp, and seems

to be peculiar to the Ball Stamp, no material change having been made in it since the first.

Its arrangement is shown in Fig. 93, in which C is an ordinary eccentric cam, connected directly with the valve stem. The cam shaft is driven by the eccentric gears BB', a being the driving-pulley, and the throw, e, of the cam is set at a right angle with $D\ D$, on the tangent diameter of the gears. This gives the valve its slowest motion when the cam is up, as shown, and the quickest motion when it is down—motions corresponding somewhat with that of the stamp. The throw and travel of the valve are such as to give a wide port at the top for the down-stroke, but only a partial opening at the bottom for the up-stroke—in present practice about $\frac{3}{16}$ of an inch. Fig. 94 shows the shape of the first mortars, which were cast very thick, having no liners or die at the bottom. The rock was fed into a hopper, water being admitted into the urn at the top, and the stamped material was carried through the screen which was upon one side only, extending about one-quarter round the mortar. The screen was usually $\frac{1}{8}''$ thick, punched with $\frac{1}{4}$-inch round holes. The stamps were usually set up in pairs, the valve-gears of both being driven by one shaft so as to give alternate blows. Stamps set up this way are still running in Houghton, Michigan. With 75 as the usual

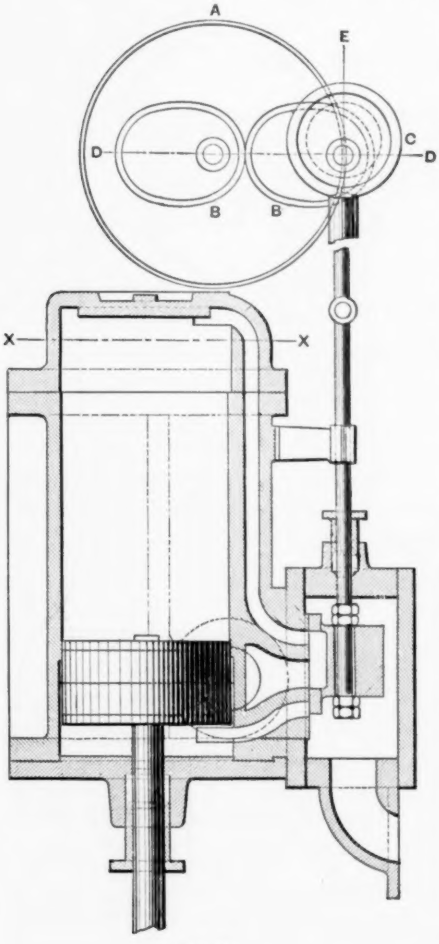


Fig. 93

number of blows per minute, and 80 lbs. the usual steam pressure, the capacity of each stamp was about 50 tons of rock per twenty-four hours, dependent, however, upon the character of the rock. Originally the stamp shaft was rotated by means of a gear and pinion, but this was superseded by a chain running round a sprocket wheel on the shaft, this method being the subject of a patent by

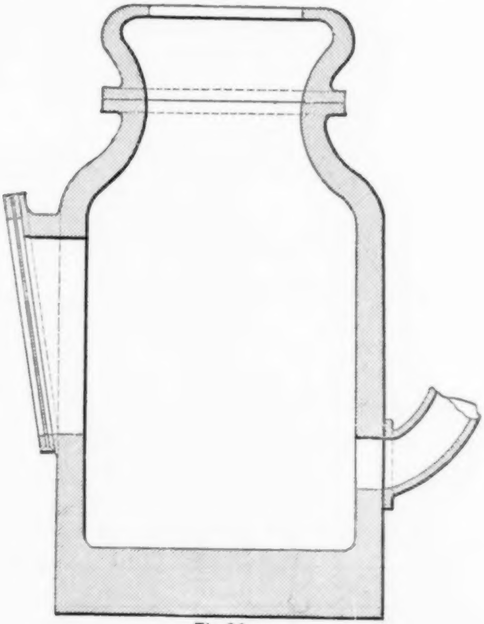


Fig. 94

Mr. Ball in 1867. This in turn was superseded by a belt, that being the present method.

The first shoes, as shown in Figs. 91 and 92, were about 12 inches wide, 15 inches long, and 6 inches thick, weighing about 300 lbs. They were cast of hard iron but not chilled. The weight of the anvil under the mortar was about 8 tons, and that of the reciprocating part, including the shoe, about 2,000 lbs. The stamp frame and sills were made of wood. Stamps of the above description were sent to the Copper Falls Mine in 1857, to the Pewabic in 1859, and to the Franklin in 1860—all in upper Michigan. Little or no improvement was made upon any of them until 1865, when, repairs being necessary at the Franklin, quite a number of improvements were made, some of which were suggested by those who had had

experience with the old stamps, but which were covered by patents taken out by Mr. Ball in 1867. The steam cylinder was increased from 9 to 12 inches in diameter. The "sheep's-foot" stamp shaft

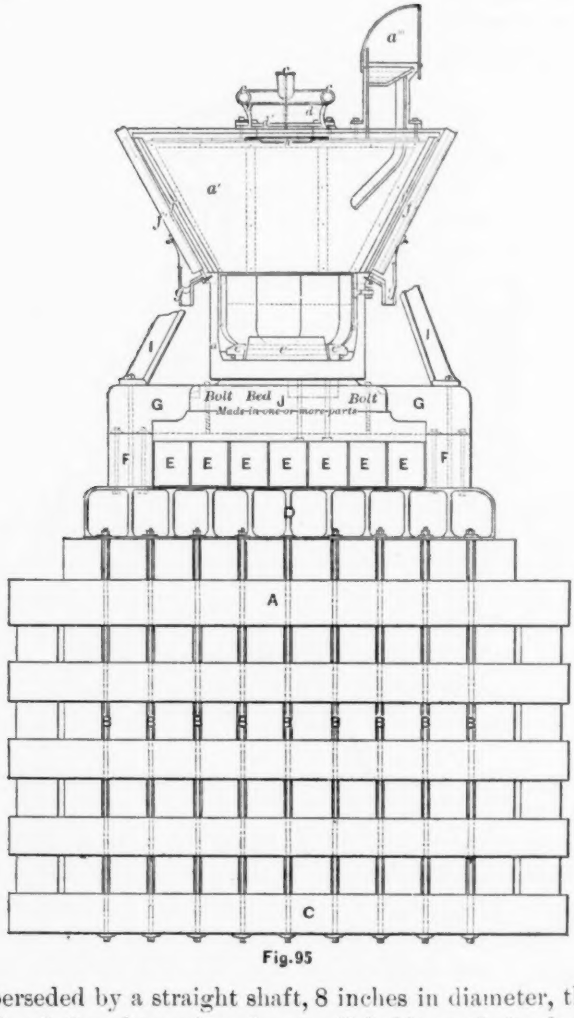


Fig. 95

was superseded by a straight shaft, 8 inches in diameter, the shape of the shoe being changed to give parallel sides and circular ends, as per Fig. 97, the weight being increased to about 500 lbs. From that time to the present the shoes have been made in a chill, and the same form is still used, but increased in size and thickness. The mortar was also changed to the shape shown in Figs. 95 and

96, this shape being still used. Fig. 95 is a longitudinal vertical section, and Fig. 96 a back elevation. This mortar is lined throughout, the die, ring and staves, *e*, being cast of hard iron and chilled. The screen surface was quadrupled. The depth of the mortar below the screen was increased, and the position of hopper changed.

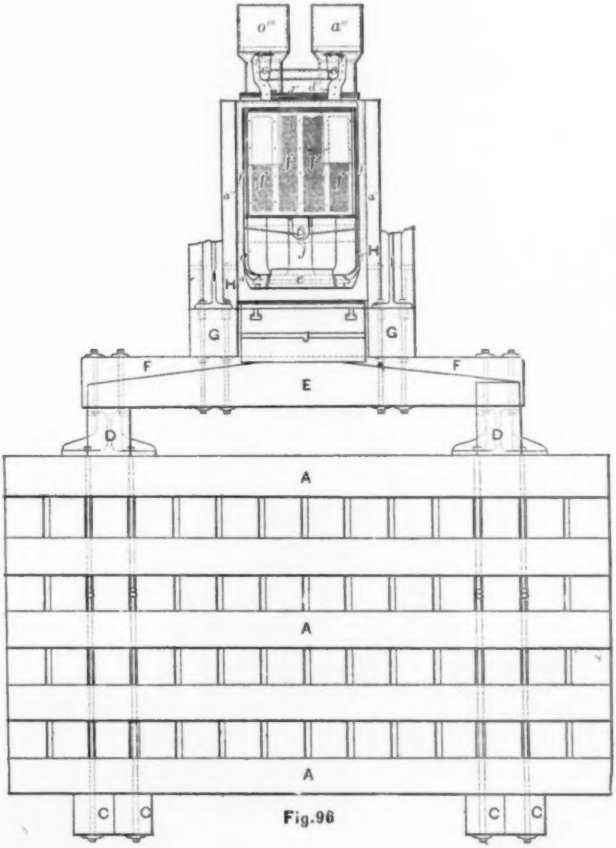


Fig. 96

The sills, which were made of wood, were changed to iron. Heretofore but three spring timbers were used, but seven are now put in, the same number and size being still used. They are 14″ wide, 18″ high, the best white oak showing the greatest fatigue.

All these changes conspired to bring the capacity of the stamp up to about 100 tons of rock per twenty-four hours, and left the construction of the stamp as shown in Figs. 98, 99 and 100, from

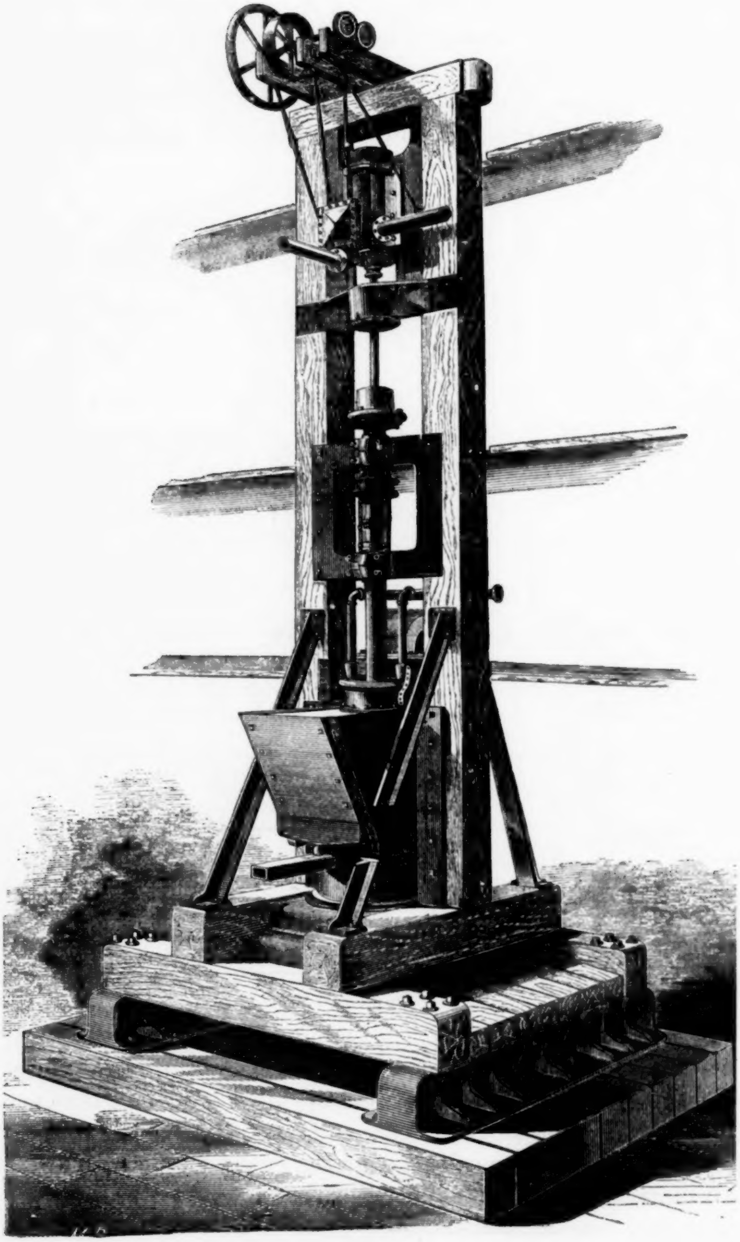


FIG. 98.

which no essential change was made until Mr. E. D. Leavitt, Jr., designed his stamp, having an iron pyramidal frame, when other parties appropriated the idea, applying it in the construction of the Ball stamps, several of which were made this way in 1883–84. In 1864 several of the Ball stamps, as shown in Figs. 98, 99 and 100, were set up for the Calumet and Hecla Mining Co. in Calumet, Mich., the diameter of the cylinders being still 12 inches. In 1875, three years after the removal of their mill to Lake Linden, the diameter was increased to 15 inches. The weight of the shoe was also slightly increased. The weight of the anvil was about 11 tons and that of the reciprocating parts about 4,500 lbs. The number of blows was also increased to about 90 per minute. These changes brought the capacity up to 150 tons per twenty-four hours. From this time up to 1879, little or no change, and no improvement whatever was made, though various attempts were made to improve the cylinder and valve gear. That improvement was needed will be seen by reference to Fig. 93, showing the old cylinder, and to Fig. 101, which is a fac-simile of a set of steam indicator cards taken from a Ball stamp running at its best in the Calumet and Hecla Mills. As the valve gear is driven independently of the stamp, the motion of the stamp shaft is limited at the bottom by the rock in the mortar and

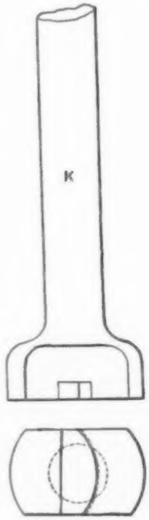


Fig. 97

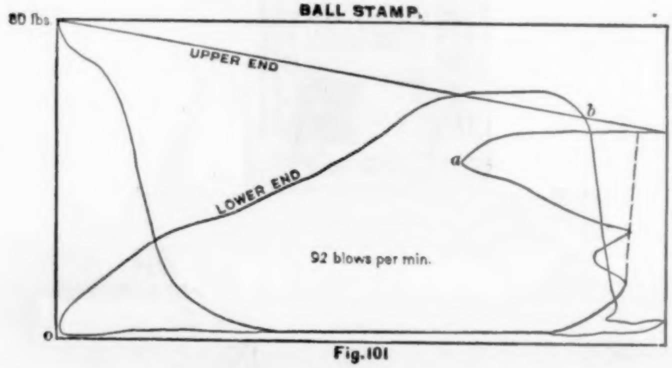


Fig. 101

at the top by the excessive lead shown in the card, or in the absence of it, by a rubber bumper, in the bumper-head, V, Fig. 99,

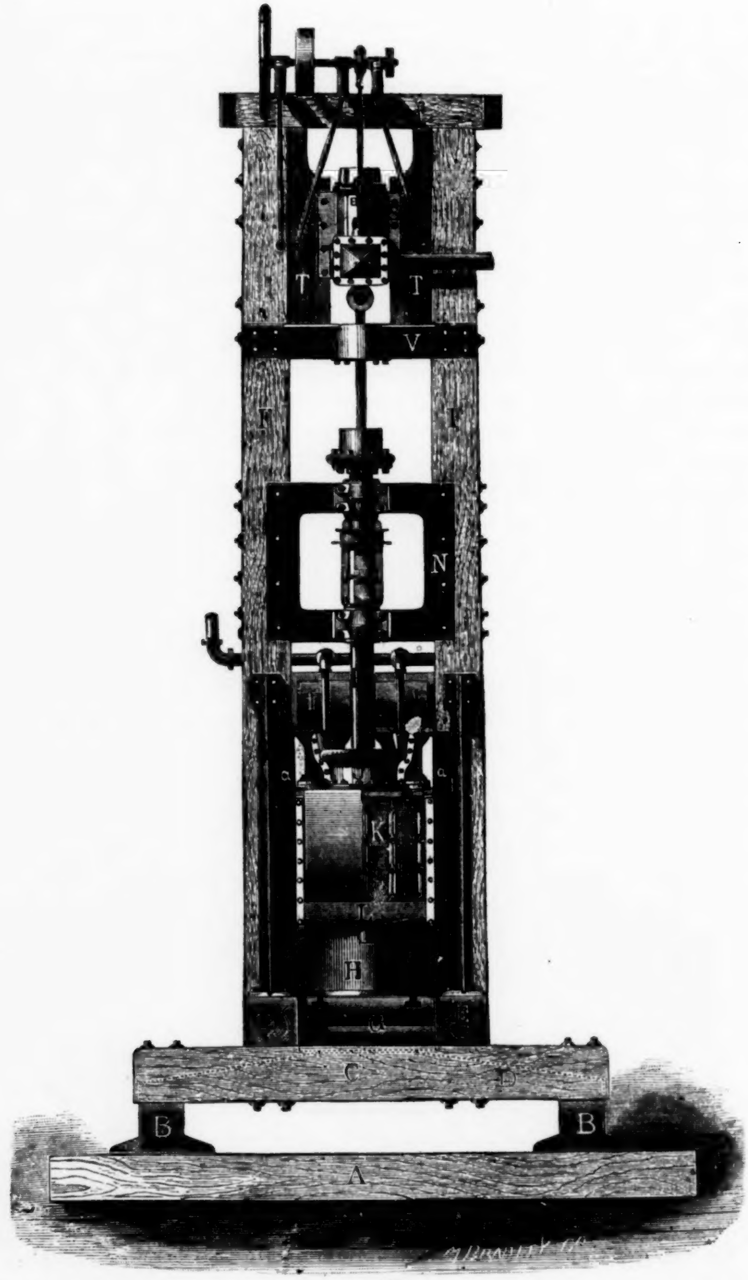


FIG. 99.

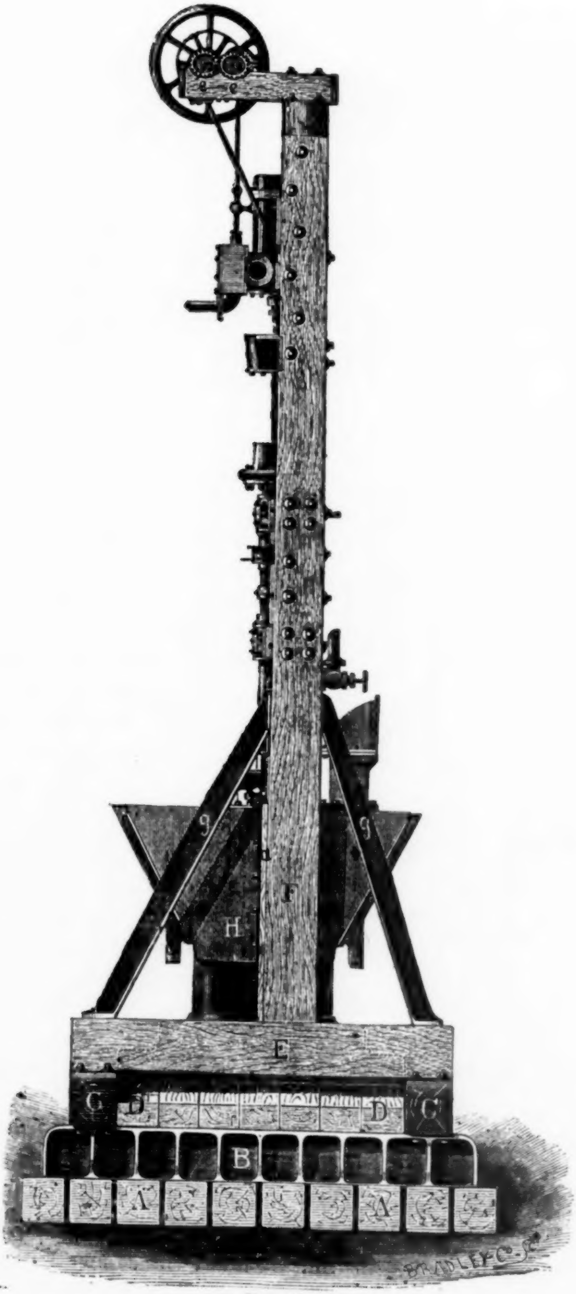


Fig. 100.

against which the bonnet B strikes. As the rock in the mortar varies, or may through carelessness be allowed to get low, a large clearance at the bottom of the cylinder is necessary, four inches being usually allowed. At the top, X, X, Fig. 93, represents the top of the stroke, a clearance of $2\frac{1}{2}$ inches being allowed for a possible compression of the bumper. To these large spaces must be added the counter-bores before alluded to, together with the large and long ports. By actual and accurate measurement, the clearance spaces in a Ball cylinder 15 inches in diameter foot up to 2,183 cubic inches, or over 50 per cent. of the cylinder for a full stroke, but it must be remembered that not one stroke in ten is full, either at the top or bottom, so that the loss by clearance is much greater than the above, and its enormity fully justifies the charge made at the beginning that a Ball stamp is a fuel-wasting machine. This enormous waste was brought to the attention of Mr. E. D. Leavitt, Jr., who had then become the consulting engineer of the Calumet and Hecla Mining Co., who thought of the subject often and long before the radical idea afterward embodied in the Leavitt stamp dawned upon him, as it did somewhat curiously. In coming up from the mine one day upon the man-engine, his mind was busy "mulling" over the subject of steam stamps, when, as he stepped upon the platform at the top, he exclaimed, "I've got it!" and thereupon he sketched the differential steam cylinder which formed the basis of future stamps, which were to supersede the Ball stamp in the Calumet and Hecla Mills. This

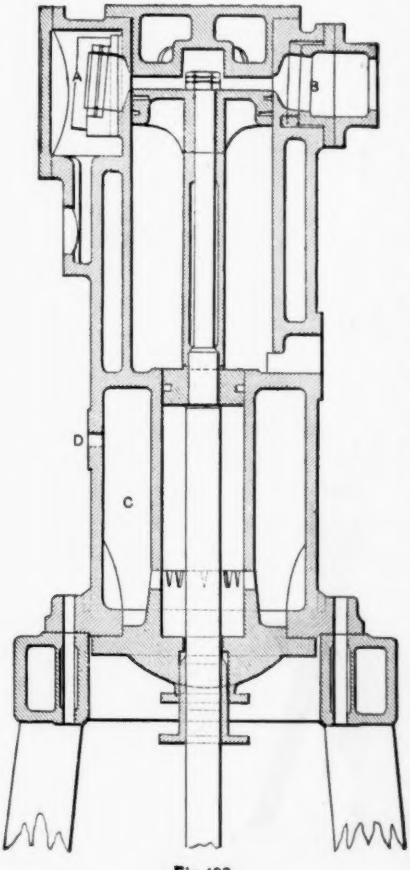


Fig. 102

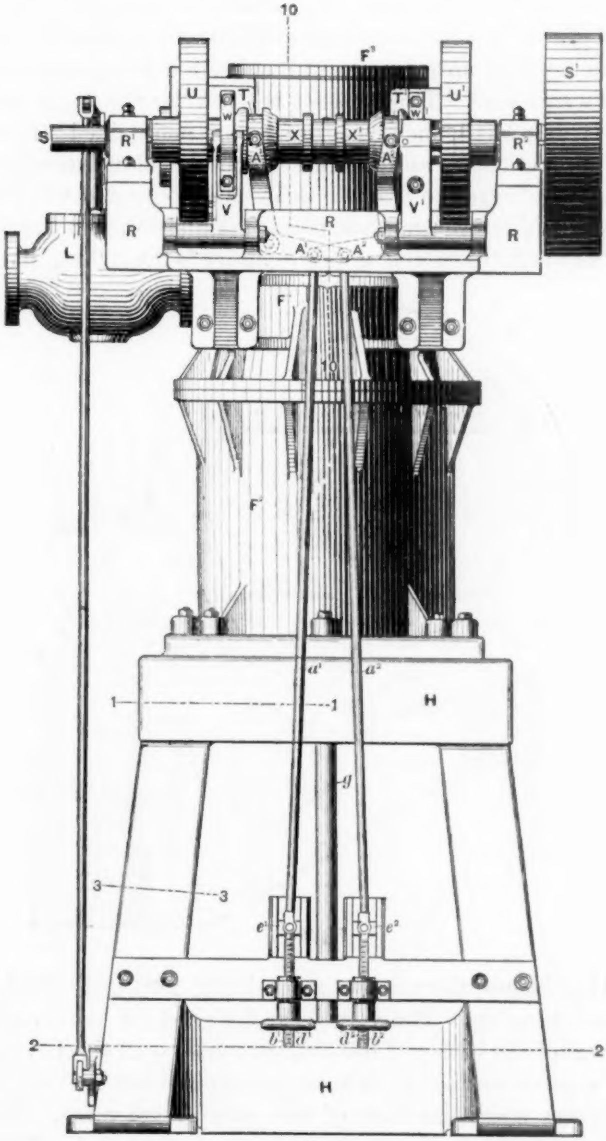


Fig.105

cylinder is shown in Fig. 102, its operation being as follows: The steam is admitted above the upper piston-head through an ordinary gridiron valve *A*, being exhausted through a like valve *B*, into the condenser, the office of the steam thus used being solely to make

the down stroke. The space *C*, around the lower cylinder and below its piston, is a receiver, into which steam is admitted through the opening *D*, a uniform pressure as desired being maintained by a Watt's regulator. This pressure is solely for the purpose of raising the stamp shaft, such pressure being maintained as will do it in the proper time, and against which the blow has to be made. The space between the two piston-heads is connected with the condenser, in which a constant vacuum is maintained. In the first cylinder the diameter of the upper cylinder is 20 inches, that of the

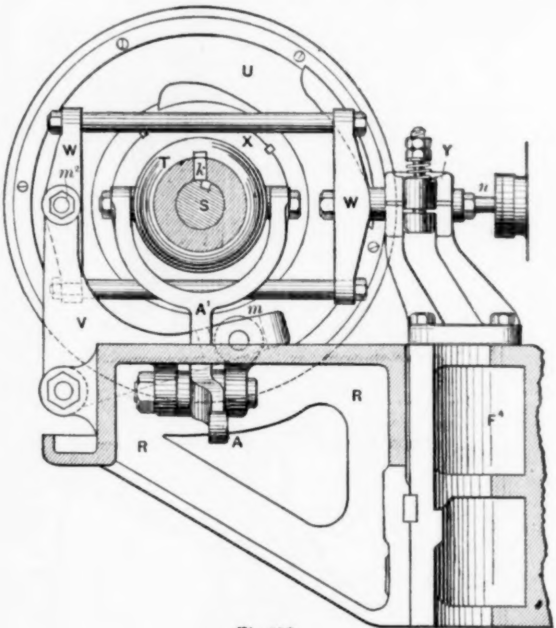


Fig. 106

lower $11\frac{11}{16}$ inches, the relative areas being 2.93 to 1. This cylinder is still running. The valve gear designed for the first Leavitt stamp was of the cam and roll type peculiar to all the Leavitt compound engines up to this time, and contemplated an independent cut-off valve upon the back of the main steam valve. This was soon found to be unnecessary, and it was discarded. There was but one roll for each valve, and it worked between two cams, an internal cam for one motion and an external cam for the other. The cam for opening the steam and that for closing the exhaust were fixed, while those for closing the steam and opening the ex-

haust were capable of adjustment while the stamp was running, as shown in Figs. 105 and 106, in which it will be seen that the cam *X* is thrown forward or backward by a feathered sleeve. The cams were set by degrees of a circle marked on a dial upon the cam shaft, so that if, for instance, the opening of the steam valve be taken at 0°, the closing point was set at 70°, the opening of exhaust at 110°, and the closing of the same at 355°. This style of cam and roll, which has worked so well upon the slow working compounds, proved a

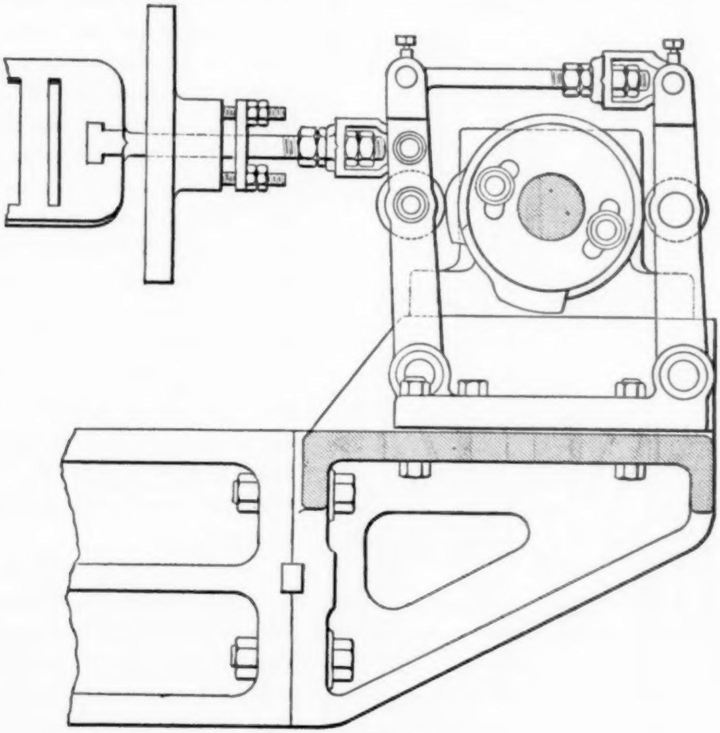


Fig. 107

failure on the stamp, where the constant reversal of the motion of the roll 90 to 95 times a minute, with the heavy valve and heavy moving parts necessary for such a machine, very soon cut both rolls and cams so as to make them useless, and the mechanism for changing the position of the cams as above described being found unnecessary, the whole valve gear as shown in Figs. 105 and 106 was superseded by that shown in Fig. 107, consisting of four outside cams with an independent roll for each motion of the valves. The open-

ing steam and closing exhaust cams were fixed as before, the closing steam and opening exhaust cams being bolted to the face of the others, yet so as to admit of adjustment as shown in the cut. This valve gear leaves nothing to be desired, as it is simple, efficient and durable. The upper part of the cylinder is steam jacketed, the whole cylinder being covered with felt and a wood lagging. The stamp with the cylinder above described is still running, with an average stamping capacity of 225 tons of rock per twenty-four hours. Some peculiarities of its working, however, seemed to indicate that the relative areas of the cylinders were not just right and that the receiver was too small, and for the second stamp the cylinder shown in Fig. 103 was designed, the upper cylinder being $21\frac{1}{2}''$ in diameter, the lower cylinder 14 inches, the relative areas being as 2.36 to 1, while the receiver was considerably enlarged.

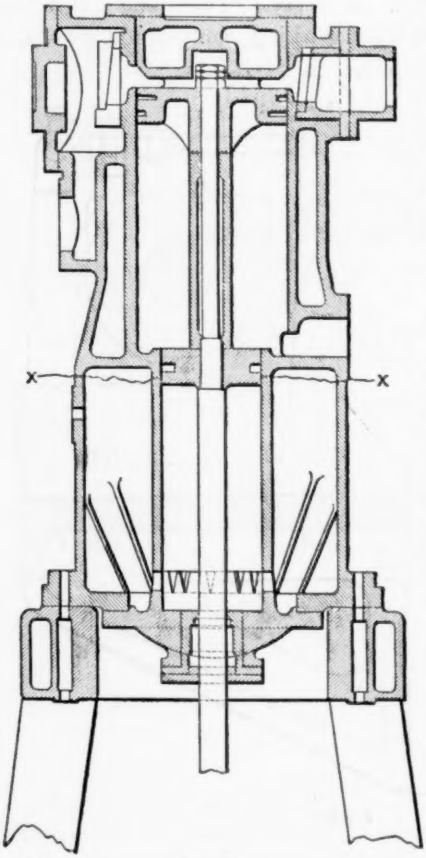


Fig.103

The mortar was also changed, being made four-sided, giving double the screen surface. This increased its weight two tons, while the weight of the anvil was increased to twelve tons—a total increase of four tons in the parts resisting the blow over and above the weight of the same parts in the first stamp. One other change of great importance was made.

With the rubber bumper before described, it was still necessary to leave a large clearance at the top, and in the first Leavitt stamp $2\frac{1}{2}''$ clearance was given as in the old stamp, and the diameter of the cylinder being larger, the clearance at this place was greater

even than in the old cylinder. The total clearance measured 1,628 cubic inches, or about 22 per cent. of a full stroke, so that while the stamp reached a greater capacity, little could be claimed for it on the score of economy, and it became evident that some change was necessary to secure the greatest economy. One suggestion after another resulted in substituting for the rubber bumper a dash-pot 25 inches in diameter and 4 inches deep, the stamp shaft bonnet being formed to serve as the dash-pot plunger. See Figs. 108 and 109.

With these changes the second stamp was constructed and started, a clearance at the top of $\frac{1}{8}$ inch being allowed when the bonnet bottomed in the dash-pot. The first dash-pot was made without the rubber rings *R* shown in the cut, and worked perfectly until shortly both pot and plunger became so worn as not to

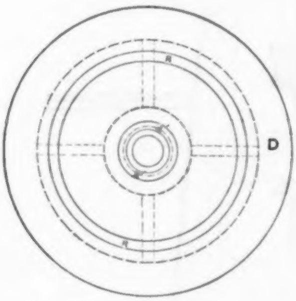


Fig. 108

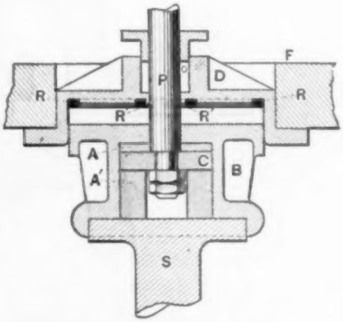


Fig. 109

prevent the escape of the air caught in the pot, and not enough could be retained as a cushion to prevent the bonnet striking the bottom of the pot, which would result in great damage to the stamp. To prevent this, the pot was grooved in the bottom, and the flexible rubber rings *R* were inserted, projecting beyond the surface about $\frac{1}{4}$ inch. The air caught within the rings forms an absolutely perfect cushion, and this improvement insured the efficiency of the dash-pot, which over three years of constant use have not impaired, but which otherwise would have been useless.

Not long after the stamp was started, a very curious accident happened, the cylinder breaking in two on the ragged line *x*, *x*, Fig. 103. The upper cylinder was lifted about a foot, and tipping forward, would have gone down through the three floors below but for the piston rod, which held it in an ominous suspense until it

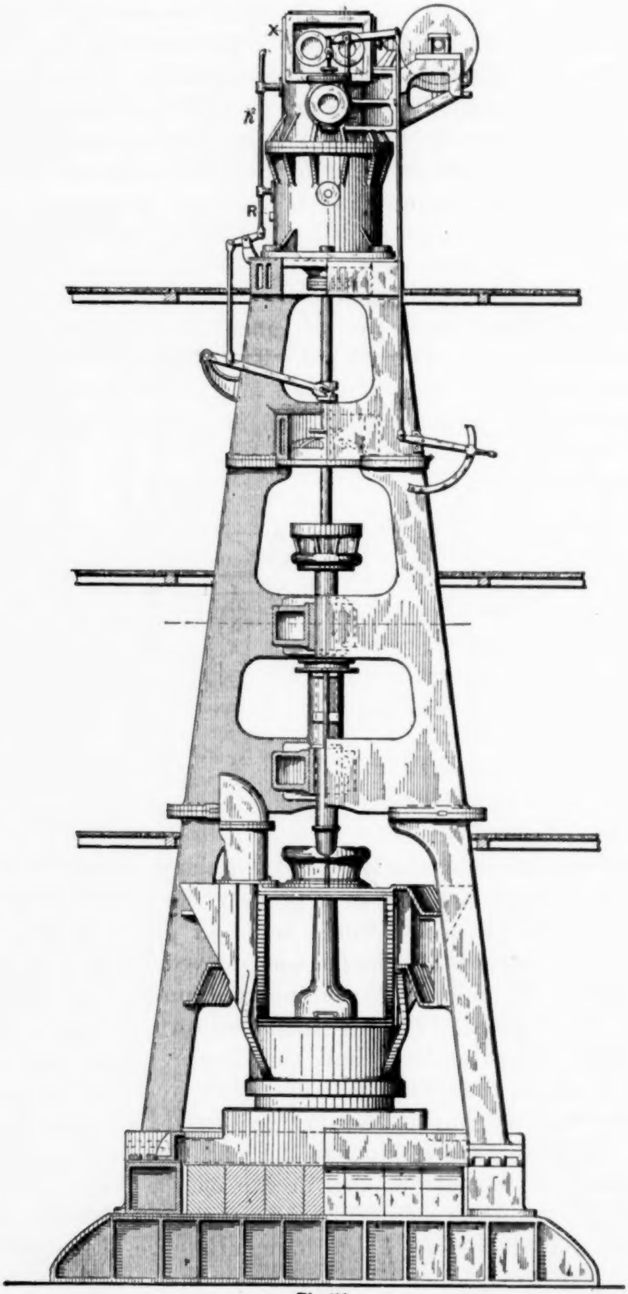


Fig. 110

was secured. The occasion of the break may be explained as follows. It will be noticed in the cut that the bottom cylinder head is extended upward to meet the lower cylinder, of which it is practically an extension, the steam pressure working through the *V* shaped spaces shown where the head and cylinder join. The object of this is to form a dash-pot into which the lower piston can cushion, when from any cause the stamp shaft should drop so low as to do damage to the parts. It is probable that the cylinder and head came nearly or quite together when cold, and that the extra expansion of the inner cylinder when heated was the occasion of the break. Fig. 104 shows the construction of the cylinder made to take the place of the broken one. It is made in two pieces, the lower end of the steam jacket space being closed by an annular expansion plate, *F*, which is bolted to the ends of both outer and inner cylinder, and which amply compensates for any unequal expansion, while the lower cylinder is left far enough from the head to allow for its expansion. This cylinder is perfect, and with the dash-pot the clearance is reduced to about 500 cubic inches, or 5.7 per cent.

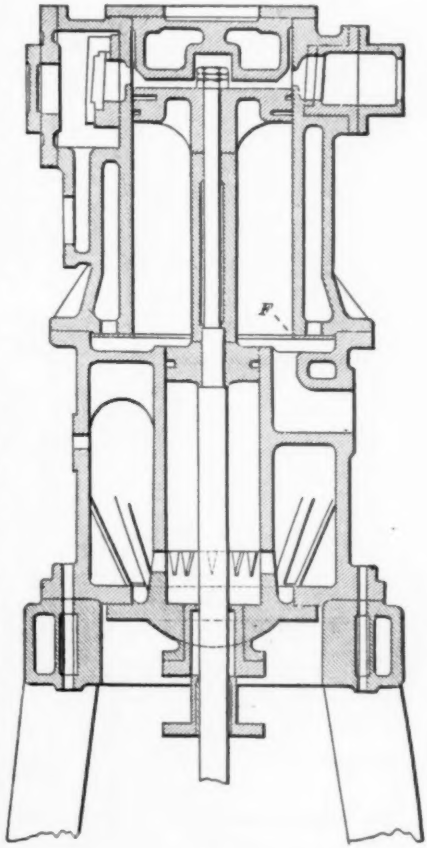


Fig. 104

of the contents of a full stroke of 24 inches. The average stamping capacity of the second stamp thus perfected, was for 1884, 230 tons per twenty-four hours, but this has since been increased to 240 tons per twenty-four hours. The saving of fuel of the first Leavitt stamp compared with the Ball stamp is about 10 per cent., with a gain in capacity of about 25 per cent. The saving of fuel of the

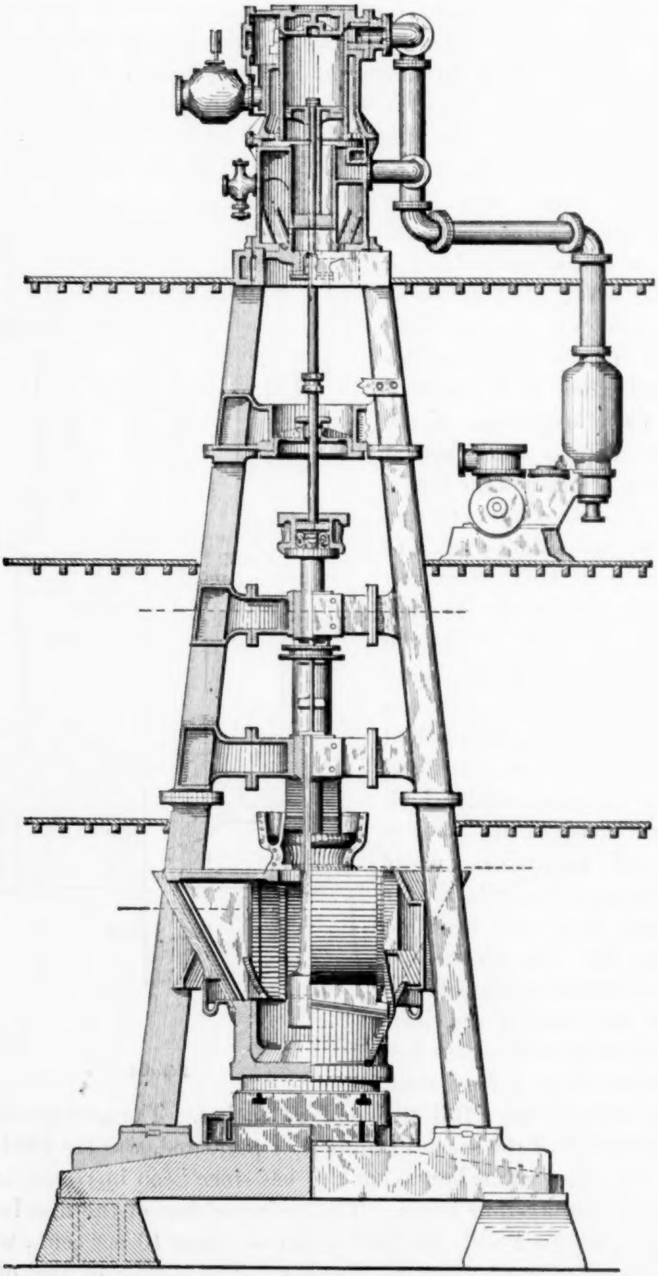


Fig. III

second stamp is 35 to 40 per cent., with an increase in capacity of over 50 per cent.

With this showing, it did not take long for the Calumet and Hecla Mining Co. to decide to replace all the Ball stamps with the Leavitt stamps, and to add a few more of the latter, until its stamping plant consists of twelve Leavitt stamps, all of which will be in commission within twelve months. Figs. 110 and 111 well illustrate the stamps as now constructed, the latter showing also the air pump with which each stamp is provided. The steam indicator is attached at *x*, Fig. 110, the motion for which is obtained by the system of levers plainly shown. Fig. 112 is a fair sample of the indicator cards from the Leavitt stamps, the line *a*, *b*, showing the receiver pressure, which increases about 12 lbs.

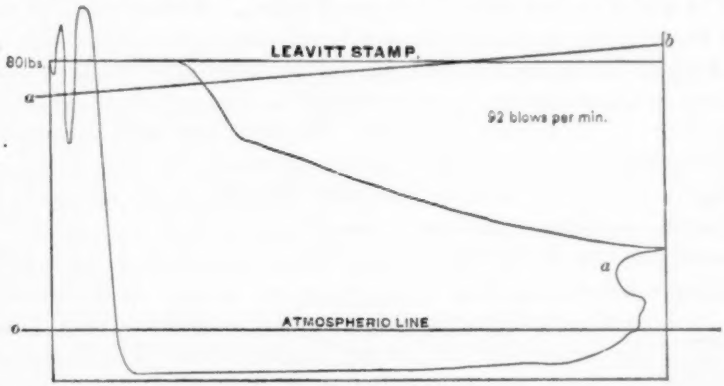


Fig. 112

as the piston descends. Comparing this card with Fig. 101, the contrast will be more clearly seen. In the old stamp the steam follows full stroke, the decreased pressure being due to wire drawing. In the new, the point of cut-off is well defined, and the expansion line a very good one.

The excessive lead is necessary to catch the up-stroke of the piston, but it is less in the Leavitt stamp than in the Ball. The extreme depression at *a* in both cards was caused by the reaction of the barrel of the indicator, the other irregular lines being due to the vibration of the spring timbers. The indicator rig shown in Fig. 110 is very stiff and has no lost motion, the barrel string being of cat-gut and as short as possible, but the sudden stop of the reciprocating parts of a stamp under a velocity of 18 or 20 ft. per second, with the constant shaking of the stamp, is not conducive to

fine lines in an indicator card. The back pressure in Fig. 101 was due to exhausting through heating pipes.

Since the improved Ball stamp of 1867, the method of attaching the piston to the stamp shaft has been the same, being clearly shown in Fig. 109, in which the collar *c* has rubber on both sides of it, to break the continuity of the metal. The rubber is $1\frac{1}{2}''$ on top of the collar, and $4''$ to $5''$ on the bottom, and is sufficiently compressed to prevent any motion of the collar. The piston rod where it goes through the head and collar is $2\frac{1}{2}''$ in diameter and 10 threads to the inch. No coarser thread can be used with safety, as the nuts would soon jar loose. The piston head of the Ball stamps is $7''$ long, the packing consisting of two outside rings (see Fig. 93), with one ring behind them. These rings are cut in one place and dowelled to break joints. They are set out to fill the cylinder when the follower is screwed tight against them, making it practically a solid head, and yet a cylinder requires reboring in about eighteen months, though as hard as it is possible to cast it and admit of being worked. The Wheelock sectional steam packing was used in the first Leavitt cylinders, the first cylinder being in good condition after five years' running, but in subsequent cylinders the sections wore very unevenly, and wore the cylinders tapering, so that it has finally been discarded, and at present the packing consists of a single ring cut in one place. It is made of bronze, having a T-shaped section, as shown in Fig. 113, the face of the ring being 2 inches wide. This packing has given the best satisfaction of the various styles tried, and has been adopted for both the upper and lower heads. It would be difficult to tell the proportion of the improvement in both capacity and economy over the work of the first Leavitt stamp, due to the various changes made in the second stamp. There was a gain in capacity of 15 per cent. over that of the first stamp, and from a saving of 10 per cent. of the fuel in the one there has resulted 35 to 40 per cent. saving in the other.

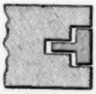


Fig. 113

The increase in capacity due to the doubling of the screen surface was not what was anticipated of it. Repeated and careful experiment showed that, other conditions remaining the same, but 7 per cent. more rock was stamped with the four screens than when half of them were blinded. There is no doubt that for most of the blows the stamp shaft receives an impetus from the dash-pot, which gives it a velocity it would not otherwise have, and which is clear gain; and the deeper the bonnet goes into the pot the

greater the impetus and consequent increase in velocity, and the less the clearance in the cylinder, from which it would seem that the dash-pot should have the most of the credit of the gain in both capacity and economy. As a matter of curiosity, the dash-pot was indicated by the writer with the result shown in Fig. 114, of four succeeding strokes. It has otherwise been conclusively shown in the Time Cards, to which further reference will be made, that the deeper the bonnet went into the dash-pot, with a consequent increased compression therein, the quicker the stamp acquired its maximum velocity, and the greater that velocity. The velocity of a blow in which the bonnet bottomed the dash-pot was 33 per cent.

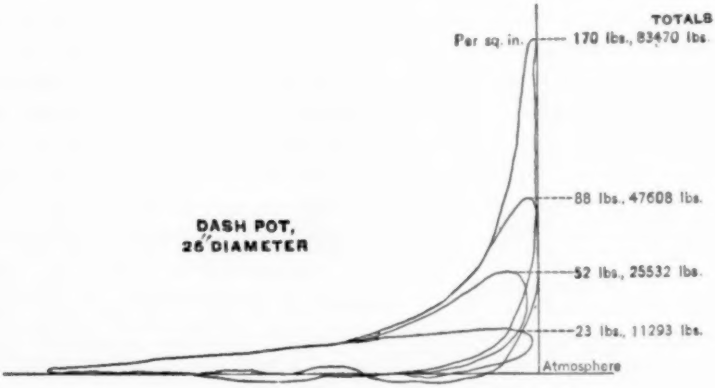


Fig. 114

greater than a preceding blow in which the bonnet did not enter the dash-pot.

It was an interesting operation to get at the velocity of the stamps in making the blow, made necessary in order to discover why the first Leavitt stamp had no greater capacity than some of the Ball stamps alongside of which it was running. As the new stamp worked under an entirely different principle from that of the old, no mere observation could determine the cause of the failure. Indicator cards had been taken from both old and new stamps, but gave no reliable clue, while there was a decided difference in the motion of the stamp shaft at the bottom of the stroke. In the Leavitt stamp the blow was quick and the stamp shaft began to rise immediately after it was made, while in the Ball stamp the blow seemed to be more as if it was simply the effect of gravitation, and the stamp shaft made a momentary stop at the bottom.

This peculiar rest was claimed by all the old and experienced stamp "bosses,"—and by the wiseacres who had had no experience —to be a necessity to the best work, reasoning with some show of logic that the stamp shaft remaining down, took the reaction of the spring timbers. They were so absolutely sure of this that the writer, who was then having his first experiences with steam stamps and the inventor of the stamp, began to feel that perhaps, after all, that peculiar blow might be the *sine qua non* to success, but how to accomplish it with a constant pressure used to raise the stamp, was a serious question. But it was not the writer's intention to fall into the ruts peculiar to the location, and the oft-repeated and universal expression that "the thing would never work," only increased the determination to make it work, and without wasting more time in speculation, it was determined to find the velocity of the stamps, as that was the only unknown quantity in the problem. This was accomplished in the following manner. To a bracket on the cylinder at *R*, Fig. 110, was attached a system of rolls as per Fig. 115, in which *a* is the driving roll, the motion of which was uniform, being taken from the valve-gear cam-shaft, *b* is a friction roll, capable of instantaneous contact with, or release from, the roll *a*; *c* and *d* are idler rolls. Around them and between the rolls *a* and *b* was passed a strip of paper *EF*, 5″ wide and about 35 feet long, or long enough to last one minute. To the indicator rod p^2 was attached a pencil-holder so arranged that the pencil could be quickly thrown on or off, the point of the pencil having a vertical motion at *g* against the roll *c*. There were also fixed points at the top and bottom of the roll which traced upon the paper the limits of a full stroke. The motion being applied to the paper, the pencil is also thrown in and the time accurately taken for one minute, when a series of diagrams were traced, of which Fig. 116 is one, from the first Leavitt stamp. The whole strip is then measured, and its length and the time being divided by the number of strokes, gives the average length and time of each card, from *a* to *e*. From *a* to *c* represents the up-stroke, from *c* to *e* the down-stroke. The divisions from *d* to *e* are in inches of the stroke. It will be noticed that the greatest velocity was acquired during the first 6 inches—at *P*, from which time to the end of the stroke it was uniform, being in this case 15.15 feet per second. During the time the card has traveled from *G* to *H* the

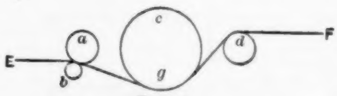


Fig. 115

stamp-shaft has traveled from *G* to *E*—one foot in .066 of a second, equal to the above velocity. This gives great accuracy.

As the height of this card corresponds to the length of the steam card, the latter is laid on to show the relation of the opening and closing of the steam valve to the motion of the stamp, *m m* being the opening, *n n* the cut-off.

From this card we are able to get the velocity at any portion of the stroke—both up and down—and to note the influence of the steam. The same course pursued with the Ball stamps produced the diagram shown in Fig. 117, upon which the steam cards for both ends of the cylinder are laid. This stamp acquired its velocity during the first 7 inches of the stroke, which was 16.16 feet

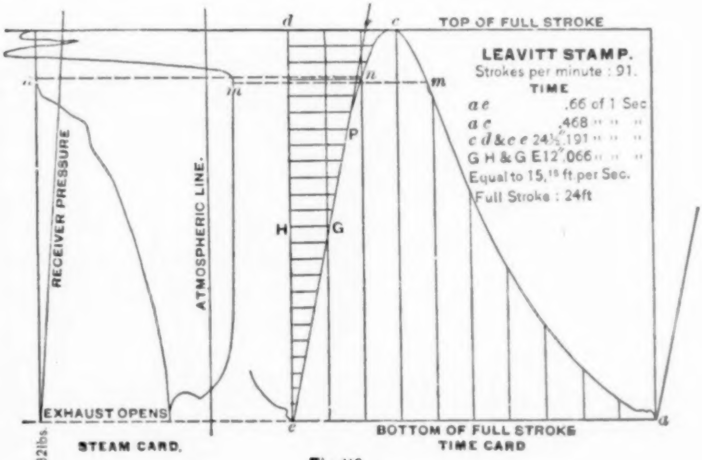


Fig. 116

per second, and was uniform for the rest of the stroke. This uniformity of velocity was a surprise to me, as I expected to find an accelerated velocity, and it seemed all the more strange, as the conditions under which the blow is made in the two stamps are so different, and yet, in every instance and in every card, in both stamps, the velocity has been uniform for at least the last fourteen inches of the down-stroke. No satisfactory explanation of this has yet been found. Most of the time cards from the Ball stamps have shown a greater velocity than this, perhaps 17 to 18 feet per second. A comparison of the two time cards will show more clearly what was alluded to as the action of the stamp-shaft immediately after the blow. These cards revealed the fact that in the Leavitt stamp, the velocity of

the blow was insufficient, and suggested a change in the closing point of the steam and opening point of the exhaust valve, which had been set as follows: opening steam, 0°, closing steam, 70°; opening exhaust, 128°, closing, 355°. It is not necessary to follow all the changes that were made, but experiments resulted in the following: opening steam, 0°, closing, 95°; opening exhaust 110°, closing 355°, the velocity being increased to about 19 feet per second and the capacity to 25 per cent. beyond that of the Ball stamps, and fully up to what was expected of it, while the capacity of the wiseacres for croaking was reduced to zero. It was thus conclusively shown that no peculiarity of the blow, outside of the weight and velocity, entered into the work performed, for at this time the screen surface

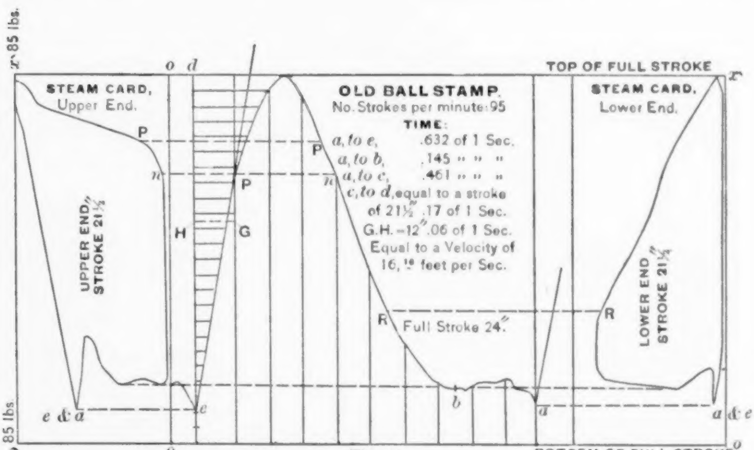


Fig. 117

had not been increased and the dash-pot had not been applied to the first stamp. It has since been applied, however, increasing the capacity and the economy. In the second and subsequent Leavitt stamps the average velocity was still further increased to 20 or 21 feet per second. How far this can be carried remains to be seen, but the present jigs belonging to the stamp cannot profitably handle more than is now being stamped. In some of the stamps sent to the Calumet & Hecla Mining Company in the spring of 1884, the weight of the anvil had been increased to 23 tons, on the supposition that the greater the inertia of the parts receiving the blow the better the effect; this has not proved true in practice, while the increased weight has proved to be more than the springs could well bear, and they gave out much sooner under the greater weight. The

life of a set of spring timbers varies from three to five months. The average wear of the shoes is about six days, being brought to their present fatigue through a system of graded prices, which required the shoe to last four days to be paid for, but which increased the price for every day's wear over that time, but competition has now taken the place of that system, and seems to keep the wearing quality up. A set of screens of the best steel $\frac{1}{16}''$ thick, punched with $\frac{3}{16}''$ round holes will screen about 10,000 tons of rock, when the amount of water in the mortar is about 800 cubic feet to the ton of rock. Absolute uniformity in the motive power driving the valve-gear of a steam stamp is a necessity to the best work, and if the main driving power of the mill is subjected to variable loads, it would be best to drive the stamp valve-gears with an independent engine made for the purpose.

DISCUSSION.

Mr. Holloway.—There can be no difference among engineers as to the question, that, while there is a great deal of literature extant, which is not of the right stamp, the literature of the stamp as presented by our member, Mr. Coggin, must not only receive our commendation, but that it must command our careful attention and study as well. Mr. Coggin has not only done the profession a favor, by the careful and exact manner in which he has described and illustrated the operation of a class of machinery but little known, but he has as well done an act of justice, in rescuing the early history of the steam stamp and its inventors, from the realms of a tradition fast dying out, and placing it in the archives of engineering literature where it will be studied with interest for generations to come. In the great haste to reach forward to new and untried experiments, we too often lose sight of what has been done in the past; and are far too apt to forget the early pioneers, who accomplished what they did under surroundings far more discouraging than can ever fall to the lot of the engineer of the future. So far as I am aware, the paper before us gives the most complete elucidation of the various elements, which go to make up the efficiency of this class of machinery, that has been published. While the writer has given in detail the various changes and improvements added from time to time, and has shown the result in economy of steam, and the increased output of the later stamps, he is not, as I understand him, quite sure as to just what particular change has brought

about these results. As the designer of the new stamps—Past-President Leavitt—is not with us, we shall not have the benefit of his discussions on this matter, but I am sure that if there are any members here who have had any knowledge of steam stamps, either as to the earlier stamps or as to the later ones, or as to the improvements upon any of them, it will be a matter of interest to us to have the result of their observations in regard to this paper.

Mr. Couch.—My knowledge of these matters has only been incidental. It has never fallen to my lot to design or manage machinery of this kind, though, of course, I have taken the general interest in it that most of us have, and this paper is very interesting. As early as 1854—I recollect the date with certainty in connection with other events which I remember perfectly well—as early as 1854 a Mr. Merriam, formerly of Pittsfield, Massachusetts, at that time managing a little machine shop and foundry at Athens, in Bradford County, Pennsylvania, showed me a photographic picture—I think it was a daguerreotype—showing a battery of four or five stamps, the valve motion of which was operated by a transverse shaft across the top, which itself was driven by a small steam cylinder with a fly-wheel and the usual eccentric valve motion. I am pretty sure that the valve-motion cylinder, as it might be called, was attached vertically near the upper part of the frame of the machine, and that its shaft ran across the top, and that the number of the stamps was four or five. It had been put into use some two or three years before that. I am very sorry that I cannot recall where, because such things would be of interest in connection with this matter, but it was as early as 1854, and it had been in use perhaps one or two years before that time. How well and durably and satisfactorily it operated I am not able now to say, although Mr. Merriam told me that it had worked well. Whether it continued to do so or not I could not say. I have mentioned it as a matter having perhaps some little historical interest.

Mr. Sweet.—As has been shown, the valves of these stamps are driven by independent valve-motion, and not each stamp working its own valve, as is the case with steam hammers. I presume the object of this is to prevent all the stamps of a gang striking at once, as they would be sure to do sooner or later if each worked its own valve. By working all the valves of a gang from a line shaft, setting the cams on the shaft in different positions insures

the stamps striking at different times. Practically uniform motion of the valve shaft is necessary, because if run too fast it would be likely to catch the stamps before they strike the ore, and if too slow it would allow the pistons to strike too hard on the bumpers at the top.

Mr. Oberlin Smith.—I should think that each stamp might be made to work the valve of the next one, on the principle of a duplex pump, by a sliding inclined groove giving the proper motion. Whether this could be obtained by a system of levers instead of by the grooves I do not know; very possibly it might. If such an arrangement could be carried out, they could of course be kept out of unison, but some long valve connections would have to extend from the first to the last stamp cylinder.

The President.—You will observe the cards made by the stamp show a very peculiar motion. Mr. Coggin, who perhaps has given more attention to this matter than any other man in the United States, after all the various changes he has made, hardly himself knows just what change has brought the stamp up to its present efficiency, so true is it that a very slight variation in any of the conditions makes a great difference in the result.

Mr. A. H. Emery.—In 1863 or 1864, I think it was, a proposition was made by Mr. Ball to me to join him in that enterprise. The proposition was made through Mr. Ames, who was associated with me in another matter at the time and who was the manufacturer of the Ball stamp. I contemplated doing so, but previous to making the connection, which for some outside reasons was not made, I went to study the stamp to see what could be done with it, and I came to the conclusion that the Ball stamp could be made much more efficient, and I worked on this for some time. The plan which I adopted as being desirable is one which could not have the trouble which he experienced in some instances of having the whole lower part of the cylinder destroyed by the first blow, by having the material under the stamp insufficient, and that was to have no bottom-head whatever, the cylinder being open at the bottom, and the stamp, which was to be some five or six thousand pounds in weight, raised by condensing the steam which had previously forced it down. I found by computation that I could get 80 strokes per minute with an average stroke of about three feet, but which would be changed by the depth of the material that the stamp was striking, and would accommodate itself to the depth of material, so that when the stamp had made

its full stroke the exhaustion of the steam through the condenser would let the air raise the stamp. Working with that length of stroke I could very well get about eighty revolutions a minute of the hammer, and such a weight of hammer that that number of revolutions would be very efficient. The hammer was to be rotated, and not to have a foot on one side as in the Ball hammer, but with two feet as it were, one on each side. The efficiency of the hammer and its own life would in this way be much improved by having the double foot, and by having but one head to the steam cylinder, which would then be used with a condenser. There would be no lower head to be destroyed. If the depth became too small the hammer would stop. The drawings were partly made, but some other operations I took up prevented their ever being completed.

The President.—I believe it is true that the efficiency of the stamp is lessened when the material under the stamp becomes thin. That is, a stamp with a certain mass under it at all times is more efficient and produces a better result than when the material is thin between the hammer die and the mortar.

Mr. A. H. Emery.—We needed a depth of material for two reasons; one was to use up the force of the hammer, and the other was that that force should mainly be used up in the stuff itself, crushing and grinding together the mass, so that the mass would be kept at a very considerable depth, and when it did not reach that depth the hammer was arranged to stop itself.

Mr. Couch.—Thirty years ago fuel and its economy were of less consequence than now, and Mr. Merriam avoided striking at the bottom of the cylinder by the more simple and wasteful means of allowing the stamp to rest at the bottom of the trough while the piston was still above the cylinder bottom, and at the top by using the steam cushion so well known in the old Nasmyth steam hammers.

The President.—Were stamps of that construction used at the copper mines?

Mr. Couch.—I do not think those stamps were designed for copper mines; I think they were used in some mining enterprise in one of the Carolinas. Stamps have been built with valve motion similar to that of steam hammers, the downward motion giving the opening for the upward, and in all cases the difficulty mentioned by Professor Sweet occurs; they will get into unison, and if they do not do that, they seldom or never can be made to have

the proper succession which is necessary to the most efficient working, and that difficulty prompted Mr. Merriam to connect the valves thus positively.

Mr. A. H. Emery.—I made use of steam as a cushion to stop the stroke at the top or upwards, but that was not wasteful; that force was used to condense or compress the steam, and as the steam was used expansively, the loss from using the steam as a cushion at the top of the cylinder was very small. The steam was compressed by the hammer's piston going beyond or above the steam's entrance, and that work of compression is given back again instantly before the cylinder begins to receive steam again on the downward stroke. It received steam just before finishing its upward stroke and then compressed that steam.

CLXXI.

BELTS AS GRAIN CONVEYERS.

BY T. W. HUGO, DULUTH, MINN.

BELTS for conveying grain are more extensively used in the Duluth Elevator system than in any other place in the world, as far as the writer is aware, and information on such subjects is very limited. These two facts have led to the presentation of this paper in the hope that, at least, attention might be drawn to this important feature in grain handling.

A description of the various belt conveyers in use in this place

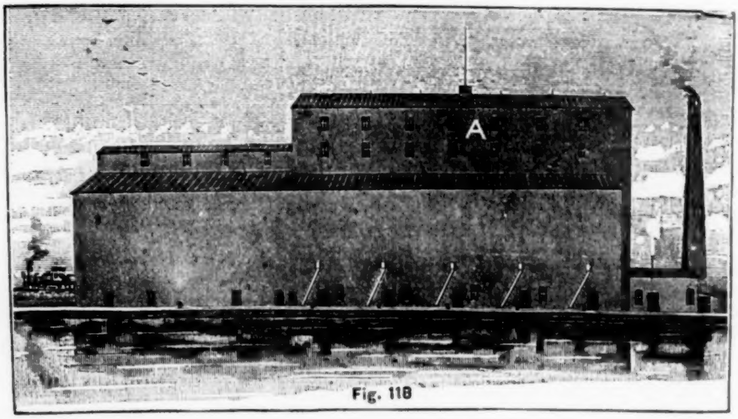


Fig. 118

will necessarily include mention of the different elevators, and they will be taken in order according to age.

In 1869, Elevator "A" was built with a capacity of 350,000 bushels (Fig. 118). It is a single track, five-car house, having five receiving and three shipping legs, and three cleaning machines, and the motive power is supplied by a horizontal non-condensing engine 24″ × 30″, with plain slide valve, cut-off on back, and a throttling governor. This engine makes 68 revolutions per minute, and was built by the N. W. Mfg. Co. of Chicago.

In 1879 an annex was built with a capacity of 210,000 bushels.

Fig. 118 shows a view of the house as it exists at present, the annex being distinguished by its cupola, which is one story less in height than the main building. Two conveyer belts are used, an upper belt to convey the grain out to the annex, and a lower one to bring it back. The power to drive the upper belt is transmitted from the main line of shafting in the cupola to a 24″ straight face pulley by a belt; this is the driving pulley for the 36″ rubber belt, which has a speed of 650 feet per minute, and is supported by and runs over concave wooden rollers, very nearly similar to *U* (Figs. 121 and 122), 9″ in diameter at the ends and $4\frac{1}{2}''$ in diameter in the center, placed at 4 feet centers. The frame work for these is built on the bin floor in "A" on an incline of 7 feet in 60 feet. This 60 feet brings the belt to the annex in which it runs horizontally, passes through the dumper, and at a distance of 155 feet from the driving pulley passes around a 24″ straight face pulley, which is made to serve as a tightener, and returns, being supported by con-

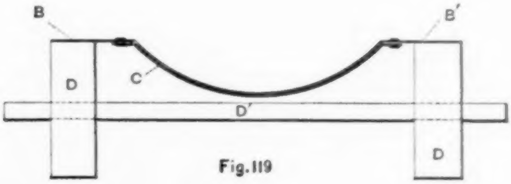


Fig. 119

cave wooden rollers placed at 12 feet centers. This belt will carry 8,500 bushels of wheat per hour.

For the purpose of conveying the grain back to the main building a combination belt is used, of which *B C B′*, Fig. 119, is a section. *B B′* are two ordinary 7″ rubber belts connected together and kept apart by $1\frac{1}{4}'' \times \frac{1}{4}''$ band iron riveted to each belt at a distance along the length of the belt of 4 feet centers; these distance strips are shaped like *C*, Fig. 119. Heavy canvas is also riveted to the rubber belts which bags down $4\frac{1}{2}''$ in the center, the whole forming a conveyer 3 feet in width. The rollers over which the belt runs are as shown in *D D′*, Fig. 119. Two wooden rollers are on an iron axle, the rubber part of the combination belt only touching the rollers. This belt travels 650 feet per minute, and from the extreme end of the annex runs horizontally for a distance of 95 feet under the floor through which the grain from the bins falls on to it. It then rises at an angle of 30° for a distance of 30 feet, passing around a 36″ straight face wooden driving pulley over which

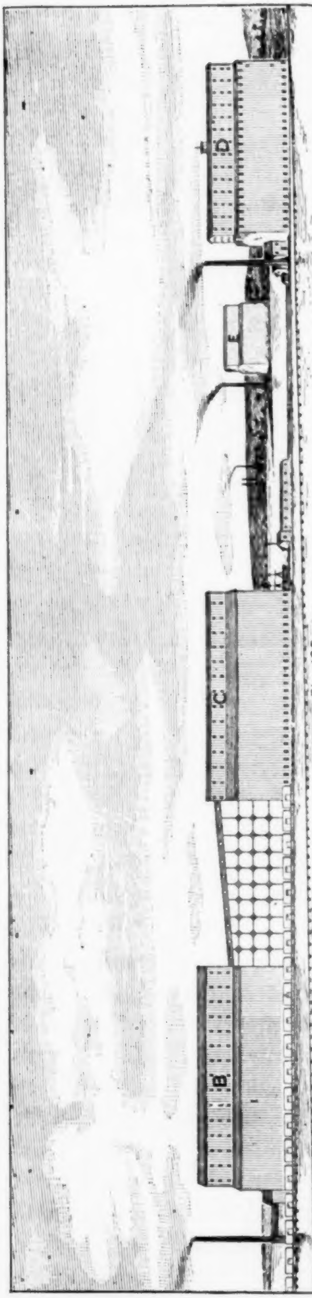


Fig. 120

the grain is discharged. This pulley is cut away in the center to allow the belt to pass around without touching the concave part. The capacity of this belt is 12,000 bushels per hour.

In the fall of 1880, Elevator "B," with a capacity of 1,000,000 bushels, started to receive and ship grain. This is a double track house for ten cars on a track, with ten receiving and six shipping legs, and having originally four, but now six cleaning machines. A vertical, overhead, condensing engine supplies the motive power, making 58 revolutions per minute, the cylinder being $42'' \times 42''$, with plain slide valve and cut-off on back and a throttling governor.

In 1882, Warehouse "C," with a capacity of 1,100,000 bushels, was built in a line with, but 250 feet from "B." Fig. 120 will give a view of the situation; the two being connected together by conveyers.

The power necessary to drive the upper conveyer belt is transmitted from the main line of shafting in the cupola in "B," through a belt, shaft, and bevel gears to the iron driving pulley, 48″ in diameter at edges, with $\frac{1}{4}''$ crown, which communicates motion to a 36″ four-ply rubber belt. For 88 feet it runs horizontally, then on an up-grade of $\frac{3}{4}''$ to a foot. At 16 feet from the beginning of the grade it enters a gallery 9 feet wide by 8 feet high and 250 feet long, built on trestle work, as shown in Fig. 120. It then enters "C" on the top floor of the cupola, passes through the

dumper, and after thus running 300 feet horizontally it passes over the tightener pulley at the end and returns between the track on which the dumper runs. The belt runs 775 feet per minute, and is supported by concave wooden rollers, *u*, 9″ in diameter at ends, and 5½″ in diameter in center, placed at 6 feet centers on the upper

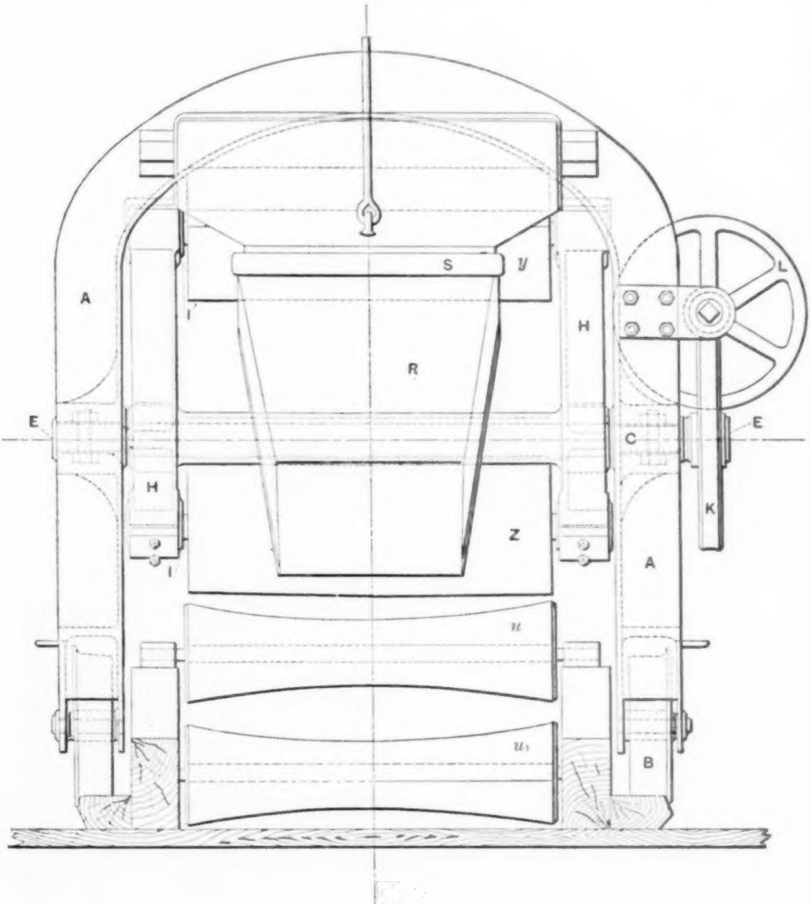


Fig. 121

or loaded part, and 12 feet centers on the under or return part. The framework on which are fastened the bearings for the rollers is made of 3″ × 8″ pine, gradually decreasing in height from 7 feet at the driving pulley to 14″ at "C," where the 2½″ × 5″ stringers, on which the lower rollers have their bearing, rest on the floor. The

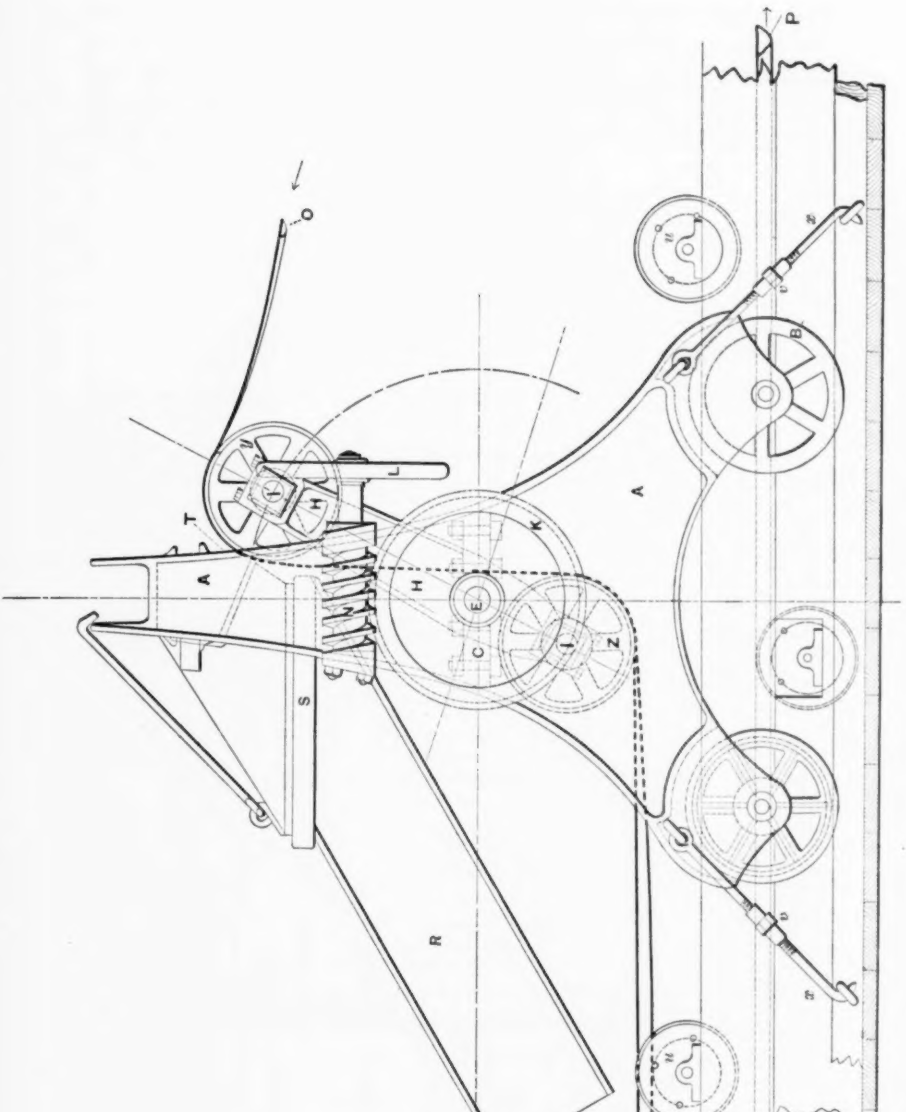


Fig. 122

upper and lower parts of the belt are made to run at a distance of 16″ from one another by means of intermediate idler pulleys.

The dumper mentioned above is shown in Fig. 121 and Fig. 122, in which *A* is a cast-iron frame running on and supported by wheels, *B*. It is cast in two pieces, bolted together at the center, as at *C*, and so constructed as to receive the shaft *E*, which passes through and is keyed fast to the movable casting *H*. This casting has a bearing fitted in the end of each long and each short arm in which run the journals of the rollers, *I* and *I′*. The gear, *K*, is fastened to the shaft, *E*, and the hand-wheel, *L*, is fastened to the worm, *N*. *O* is the upper belt and *P* is the lower, the arrows indicating the direction of the belt's motion. *R* is a sheet-iron spout swiveled at *S* to the frame *A*, so that it can be swung around to the side. Similar letters refer to similar parts in Figs. 121 and 122. In Elev. "B," close to the driving pulley, and along for a distance of 75 feet, are hoppers which receive the grain from the scales above. Those hoppers come down to within a merely clearing distance of the belt, the grain being discharged through long narrow openings regulated by slides.

Fig.122

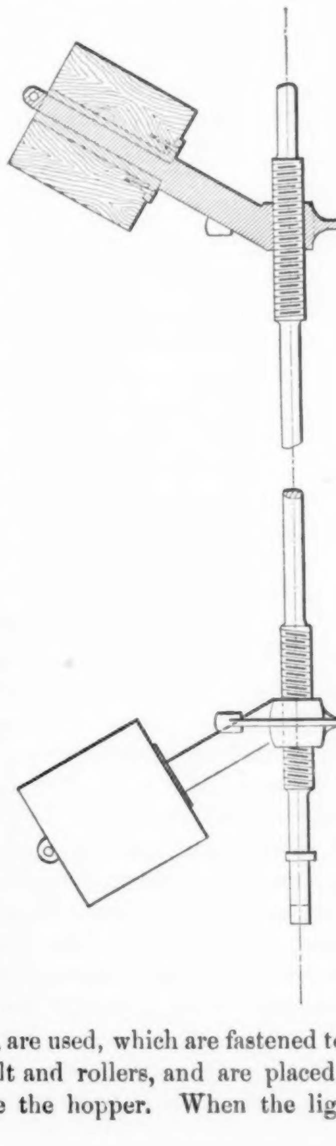


Fig.123

It is sometimes necessary to prevent the grain from jumping off the belt when falling from the hoppers, and for this purpose concentrators, Figs. 123 and 124, are used, which are fastened to the framework that supports the belt and rollers, and are placed one on each side of the belt opposite the hopper. When the lignum

vitæ rollers are brought close enough together by means of the screw, the sides of the belt are correspondingly turned up, thus preventing any grain from spattering off at the time it falls into the belt. After once it is there it lies as quietly as if glued on, and not a single kernel need be lost in the run of over 600 feet. A little of the light husks, jarred to the surface by the motion, will be blown off, but the grain will pile up until it is sometimes 6″ deep in the center of the belt, running out to nothing about 1″ from the edge. At from 35 feet to 40 feet from the dumper the belt leaves the rollers and ascends to a height of 45″ from the line of rollers, and as it passes around the upper roller of the dumper, *y*, at almost a right angle, the momentum of the grain is sufficient to carry it forward, an unbroken stream of grain, into the opening, *T*, in the dumper from whence it passes through the spout *R* through hoppers built in the floor, through wooden spouts into the different bins.

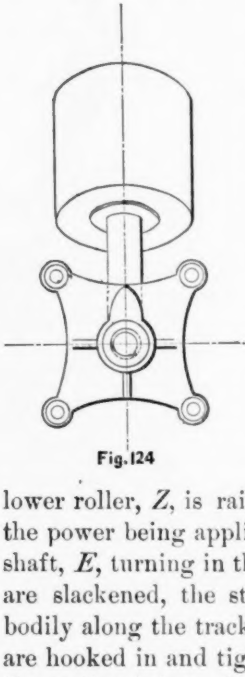


Fig. 124

There are ten of these openings or hoppers built in the floor at the side of the dumper track, close enough to be reached by the spout, *R*, when turned sideways. Each of these openings is over a circle of spouts which lead to the 180 bins in the house.

When it is necessary to move the dumper to another part of the house, the upper roller, *Y*, is lowered, and by the same motion the lower roller, *Z*, is raised by means of the worm, *N*, and gear, *K*, the power being applied to the hand-wheel, *L*; the casting, *H*, and shaft, *E*, turning in the bearings, *C*. The right and left nuts, *V*, are slackened, the stays, *X*, unhooked, and the dumper pushed bodily along the track to its destination. When in place, the stays are hooked in and tightened, and by means of the worm and gear the casting is brought to the position shown in Fig. 122. The belt can be started immediately, the operation thus consuming very little time. This belt will carry 13,000 bushels of wheat per hour.

The power for the lower conveyer belt is taken off the end of the engine shaft, using a sprocket wheel with friction clutch, link belt, shaft, bevel gears, and a wire rope 262 feet between centers of shafts to transmit to the driving pulley 48″ diameter with ¼″ crown. Belt and speed are the same as in the upper conveyer.

Starting from the end of "C" where the tightener is placed, the belt runs 300 feet horizontally on rollers similar to *u*, supported by a raised framework under the floor, then up-grade, $\frac{1}{2}''$ to a foot, through a gallery built on the dock, 250 feet long, and 38 feet into "B," where it runs over the driving pulley, the grain on it being discharged over the end into a hopper, from whence it is spouted to the different elevating legs. Holes are cut in the lower floor in "C," and hoppers with long narrow openings are built at convenient distances over the belt into which the grain from the bins overhead is spouted. This belt has carried 14,000 bushels of wheat per hour.

In the St. Paul and Duluth Elevator a conveyer belt is used for the purpose of shipping grain into vessels, the elevator being built back from the dock. The belt runs in a gallery elevated on posts about 25 feet high, leading from the end of the elevator at an angle of 45° to the dock front, a distance of 300 feet. The belt is a 50″ four-ply rubber belt, running on rollers somewhat similar to *u*, Figs. 121 and 122, pitched at 6 feet centers on the upper, and 12 feet centers on the lower part, which are 10″ in diameter at the ends, and $5\frac{1}{2}''$ in the center. The power to drive this belt is taken from the main line of shafting below, transmitted by a quarter twist belt to a 48″ straight face wooden pulley. The grain drops on the belt close to this driving pulley, travels 600 feet per minute on an incline of 2 feet in 300 feet, and discharges over the 24″ tightener pulley at the end into a hopper, from which through two 12″ iron telescope spouts it shoots into the vessel. Only portions of two cargoes were shipped last year, so that no reliable data are at hand. A horizontal Hamilton-Corliss engine, 18″ × 30″, non-condensing, supplies the power.

In the fall of 1884 Elevator "E," with a capacity of 800,000 bushels, was built. It is a double-track house for five cars on each track, with five receiving and five shipping legs, and five cleaning machines. An overhead vertical condensing Reynolds-Corliss engine, 34″ × 30″, making 66 revolutions per minute, supplies the power. Work has begun on Warehouse "F," with a capacity of 1,250,000 bushels, to be built in a line with "E," but 250 feet distant from it, and connected by conveyers in a manner but slightly differing from "B" and "C," Fig. 120. The upper conveyers will be 160 feet in "E," 250 feet between "E" and "F," and 300 feet in "F," so that there will be 710 feet between centers of shafts at ends. The lower belt will be 50 feet in "E," and the same as the upper one be-

yond that, making a total of 600 feet between centers of shafts at ends.

Also in December, 1884, Elevator "D" was built, with a capacity of 1,200,000 bushels. This is a double-track house for nine cars on each track, with nine receiving and eight shipping legs, and eight cleaning machines. A steeple compound condensing overhead vertical Cuyahoga engine furnishes the power. The cylinders are 24″ and 44″ × 48″, arranged for 66 revolutions per minute; the steam is cut off in the small cylinder only, and the dead space reduced very much by making the bottom of the small cylinder serve as the top of the large one, and in this way the cylinders are brought as close together as a steeple engine can be got. A throttling governor is used. This engine has given excellent satisfaction, and considering the very variable nature of elevator work, the necessity for providing for heavy loads at any time without warning, and the surety of running a large engine two-thirds of the time with little more than friction loads, it is the writer's opinion that a properly constructed compound engine is pre-eminently fitted for elevator work, and will prove itself so, all the factors of fuel, interest, maintenance, being counted in.

Work has been commenced on "G" with a capacity of 1,500,000 bushels, to be connected to "D" by belt conveyers, the general arrangement being similar to that already described. The upper belt will be 280 feet in "D," 250 feet between "D" and "G," and 350 feet in "G." The lower belt will be 50 feet in "D," 250 feet between and 350 feet in "G," making a total length between centers of pulleys at ends of 880 feet for the upper conveyer, and 650 feet for the lower.

As stated at the beginning of this paper, its object is to add something to the very small amount of information that is public property on this subject. For that purpose the writer made some tests with a view to ascertain the amount of power required for the different conveyers in "B" and "C," and intended to submit the results to the society, but since this paper was begun his attention was drawn to Mr. D. K. Clark's observations in his Manual, on this subject, and these disagreeing very materially from the results of the writer's tests, it was decided to withhold those results for the present until an opportunity presented itself for verification by more extensive experiments. The opportunity will be afforded as soon as "F" and "G" are completed, and in conjunction with the shipping belt of the St. P. & D. elevator will

present means for eliminating errors possible in a solitary example.

Enough has been said to show that it is a satisfactory method of handling grain, and not grain alone, but there are various articles that could be expeditiously and cheaply handled by this plan, or modifications of it to suit the articles to be handled.

With grain the belts give no trouble whatever, even with belts over 1,300 feet long, and no trouble is anticipated with the long one (almost 1,800 feet) which will be used in "G." The warehouse can be built very much cheaper than a regular elevator, and will be as efficient as a warehouse. Very few extra men are required to attend to it; paying work is made for the motive power when it would otherwise be idle; the rate of insurance is lessened; the cost of maintenance is very small; and with larger rollers, or sectional rollers lubricated through a hollow shaft, the friction can be very much decreased. Should it be desired, by a proper belt speed, angles of 45° can be ascended, and the grain thus elevated without buckets on the belts, and experience with the longest belt conveyers in the world has proven their usefulness, their reliability and their economy.

DISCUSSION.

The President.—I would say in regard to this paper of Mr. Hugo's that it is a very interesting one, not only for the reason that it illustrates and explains a mechanical contrivance, which I am sure is new in its application to most of us, but also for the further reason, that it is a mechanism now most extensively in use at a point in our vast territory, which but a few years ago was looked upon as the uttermost verge of civilization. The laugh caused by the speech made in Congress by Proctor Knott, of Kentucky, in which he spoke of Duluth, as the "City of the unsalted seas," had scarce died away when the sound of the hammer and saw was heard in that land, and immense grain elevators arose, which have made the place second only to Chicago as the wheat center of the world. The apparatus which Mr. Hugo has so well described, I had the pleasure to examine about a year ago. It was not at the time in operation, but from a careful examination of the mechanism I felt satisfied it was a complete success, and that a description of it would be of interest to engineers. I noticed that no wheat was discharged along its pathway, and I inquired if the wheat was not inclined to roll off the belt,

and was told that it not only did not roll off, but that it seemed to huddle together in the center of the belt. Mr. Hugo's description has made the matter so plain, that there is little need of further explanation. But if any members present have seen this, or other flying conveyers in operation, we shall be glad to hear from them. The paper is before you for discussion.

Mr. Kent.—I have nothing to say about the belt, as I am not familiar with it, but I would like to ask a question about the engines. The paper says: "A steeple compound condensing overhead vertical Cuyahoga engine furnishes the power," and that it is the writer's opinion that a properly constructed compound engine is pre-eminently fitted for elevator work and will prove itself so, all the factors of fuel, interest and maintenance, being counted in. He says, also, that the engine runs two-thirds of the time with little more than frictional loads. That is a very important question in engineering—what proportion of the time the engine has to run. The smaller the proportion of time the engine runs loaded to the time it is unloaded, the greater the waste of fuel will be and the greater the cost of maintenance, labor, interest, and everything else proportionate to the work done. Therefore, it is frequently found the better economy to get a cheap engine if it does waste fuel, if the engine is to be run only a short time. Here is a compound engine, and therefore an expensive one, and it has only to run one-third of the time loaded. Is it likely that it will be more economical than a cheaper engine? I would like to ask if the President can give us any information on this point.

The President.—I do not know that I can give the information Mr. Kent asks for. In the first place, the use of engines for the purpose of lifting grain, in elevators, which I suppose you are not specially familiar with here, is one in which there is a greater variation of power, in a given time, than in any machine I know of. A train of eight or nine loaded cars is brought in, and the doors are thrown open at once, and the grain all goes into the elevators, and must be hoisted at once, and it requires an immense amount of power to take the grain up so suddenly to the top of the house, and when that is done, the engine has little or nothing to do, until another train comes in. That shows the necessity of using a large amount of power a portion of the time, and using very little power at other times. As to the question of economy in using that kind of engine, I can only say that, as far as my experience goes among elevator men, the question of economy is

looked after, to a very considerable extent, in Chicago, Milwaukee, and in Duluth; and there is a great deal of striving for the most economical results, as far as fuel is concerned, among the various elevator-engine builders; and that there has been a good deal of rivalry in producing the results with the least fuel. There is a necessity for this engine, and for this machinery, to be ready at any moment. It must be ready always to take the grain as it may come in. It might be true, as a matter of engineering and financiering, that a cheap engine which for the time being might use steam wastefully, would, in the long run, be productive of economy, because the engine would not cost so much; but it would have to be a large engine.

Mr. Babcock.—I think Mr. Kent has a little misunderstood the conditions. When an engine runs but a small portion of the time—if it were standing still one-third of the time and running another third—then a cheap engine, probably non-condensing, would be better economy, because it would save more in interest, possibly, than the difference in the cost of fuel, for the time it was running. But that is not the case here, where there is a surety of running a large engine two-thirds of the time with little more than frictional loads. It is imperatively necessary in this case that that engine shall be a condensing engine; otherwise it will be discharging a cylinder full of steam at every revolution into the atmosphere and against atmospheric pressure; but with a condensing engine the loss of steam is comparatively small for the time when the engine is running light. Whether a compound engine, thus conditioned, has any special advantage over a single cylinder might be a question, but it is undoubtedly true that a condensing engine is absolutely necessary for economy under these circumstances.

Mr. Thompson.—In the manufacture of cotton seed oil, we frequently use screw conveyers such as are used for short distances in wheat mills; but they use these also. Where seed is to be delivered in different places, say to different bins, the spiral conveyers cannot deliver it, for the seed is all carried to the end of the conveyer. The belt moves in a trough. Suppose you want to deliver the seed at several different points along it; the side of the trough is cut and there is a door hinged in the trough, so that it can be opened. The belt running down this way, the door catches the seed and delivers it to the spout.

Mr. Albert Emery.—Do I understand that the door shoves the seed off the belt?

Mr. Thompson.—Yes, sir. When you want to deliver it to some other point the door previously in use is closed.

Mr. Dodge.—I would like to ask the gentleman why a spiral conveyer will not deliver at any point?

Mr. Thompson.—It would with grain or anything that is smooth, but with cotton seed, which is covered with a little fuzz, it will sometimes get choked.

Mr. Stratton.—I would like to ask for information as to what this belt is composed of, in long elevators.

The President.—The horizontal belt described in Mr. Hugo's paper is a rubber belt. It runs over rolls which are a little smaller in diameter in the middle than at each end, but the deflection is very little. It is one continuous belt of rubber, running horizontally at very high speed, and the grain is dumped down on the belt, in the direction in which it runs. You would naturally suppose it would fall off, but it soon acquires the velocity of the belt, and when acquired it clings to the belt, and moves with it.

Mr. Oberlin Smith.—It has been implied that there was not much information about this method of conveying grain in the East. Those of us who attended the last annual meeting of the American Society of Civil Engineers in New York, remember being taken over to Brooklyn and shown an entirely similar apparatus at "Dow's Stores," in the lower part of Brooklyn. The belts were running, I think, at about this same speed. They were long rubber belts on slightly concave rolls. We did not see any compound belts. They were conveying grain up and down, and delivering it and receiving it here and there. The grain went by its own momentum from the belt into the hopper, and flew over a space of several inches without spilling. The whole concern seemed to be very much the same as this one in question. There were more than a dozen of the belts, I should say.

Mr. Dodge.—I should like to say that the dump apparatus has been used in England a long while, largely in sugar refineries. It runs on a track, and is locked in place by seizing hold of the track. When the dump is to be removed they get the power from the belt itself, which can be used to shift it by suitable gearing on one of the legs. No scraper is required for a conveyer of this kind, but the same arrangement is used as is shown in the paper.

Mr. Couch.—On page 409 near the end sectional rollers are mentioned. I wonder if that relates to cutting the concave roller into a number of sections, or, if not, I wonder what the effect is of a

loaded belt traveling over a roller at six or seven hundred feet a minute, when the velocity of the middle of the roller is about half as great as that at the end, and whether the brief reference to sectional rollers has not some allusion to a remedy for that apparent difficulty.

The President.—From my observation, which was somewhat hasty, the rollers were very slightly concave.

Mr. Couch.—Twice as large at the end as at the middle, according to the figures given.

The President.—I think that where the grain first goes on the belt, there is an excessive concave there. My recollection of the rollers is, that they were all slightly concave. After the grain had acquired the velocity of the belt, there seemed to be no necessity for the belt being concave. The grain would pile up in the center of the belt without reference to its being concave.

Mr. Smith.—Those in Brooklyn were rollers of about six inches diameter, and I think they were concave only about an inch or less on each side.

Mr. Babcock.—On page 488 the author says :—" The belt is a 50 inch four-ply rubber belt, running on rollers pitched at 6 feet centers on the upper, and 12 feet centers on the lower part, which are 10 inches diameter at the ends, and 5½ inches in the center. Those are the rollers.

The President.—I think it must refer to the rollers where the belt first receives the grain. I do not think the other rollers are anything like as concave.

Mr. Babcock.—The paper certainly gives that impression.

Mr. Partridge.—I would like to ask if any of the gentlemen present can give any details of that pneumatic system of handling grain by means of a current of air in tubes. I believe they are experimenting with it at Detroit in transferring not only from cars to elevator, but also from car to car, without shoveling or trimming.

CLXXII.

THE MANUFACTURE OF COTTON SEED OIL.

BY ERWIN W. THOMPSON, THOMASVILLE, GA.

THE commercial history of an industry, which, ten years ago, consumed only 100,000 tons of cotton seed, transforming it into $2,380,000 worth of products, and which now annually consumes nearly five million dollars' worth of seed, is perhaps most easily read on the accompanying diagrams. Figure 125 exhibits the price of cotton seed at Memphis, Tenn., taken on January 1st of each year, and the price of prime crude oil and summer yellow (refined) oil at New York, taken on the first of each month.

Figure 126 shows the total production of cotton seed and the amount consumed by oil mills in the United States during the past ten years.

The great distance apart of these lines shows even now what a vast surplus of this great staple remains practically unused. Perhaps 23 per cent. of this difference is now used for planting, while the remainder is most wastefully used as a fertilizer.

Within the past three years this fact has become so generally known that new mills have sprung up all over the country, and this increased production has so lowered the price of oil and by-products, that the industry has passed from an enormously profitable business to one which has to be as closely watched as the manufacture of cotton itself. The imposition of an almost prohibitory import duty by the Italian government has also operated against the price of oil. In former years refined oil was largely exported for the purpose of adulterating olive oil. But early in 1881, Italy increased the duty from about 6.5 cents per gallon to about 16.6 cents. The result of this action was to reduce our total export of cotton seed oil from 6,997,796 gallons in 1879–80 to 415,611 gallons in 1882–83. Since that time one of the great problems of the business has been to discover domestic uses for the oil. It has been tried for almost every use for which oil can be made, and has met with varying and rather decreasing success. It has been

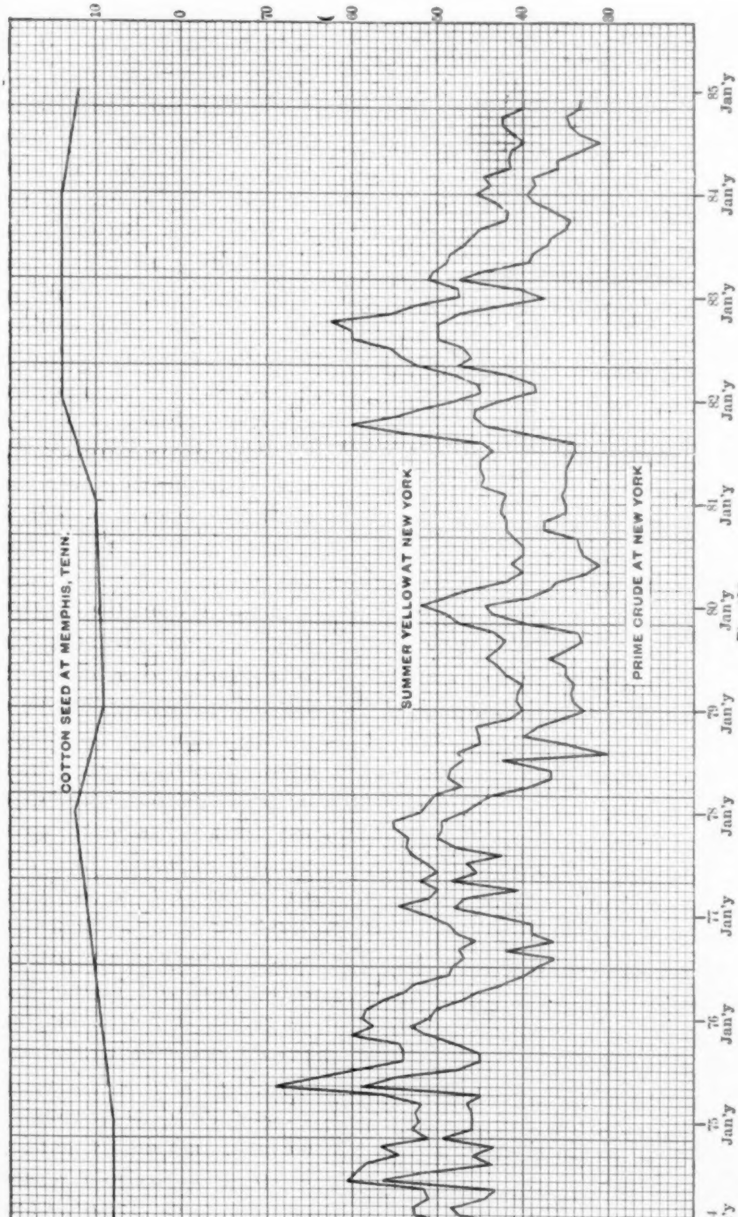

COTTON SEED AT MEMPHIS, TENN.
SUMMER YELLOW AT NEW YORK
PRIME CRUDE AT NEW YORK
DOLLARS PER TON
CENTS PER GALLON
10
0
70
60
50
40
30
1874 Jan'y
75 Jan'y
76 Jan'y
77 Jan'y
78 Jan'y
79 Jan'y
80 Jan'y
81 Jan'y
82 Jan'y
83 Jan'y
84 Jan'y
85 Jan'y

Fig. 125

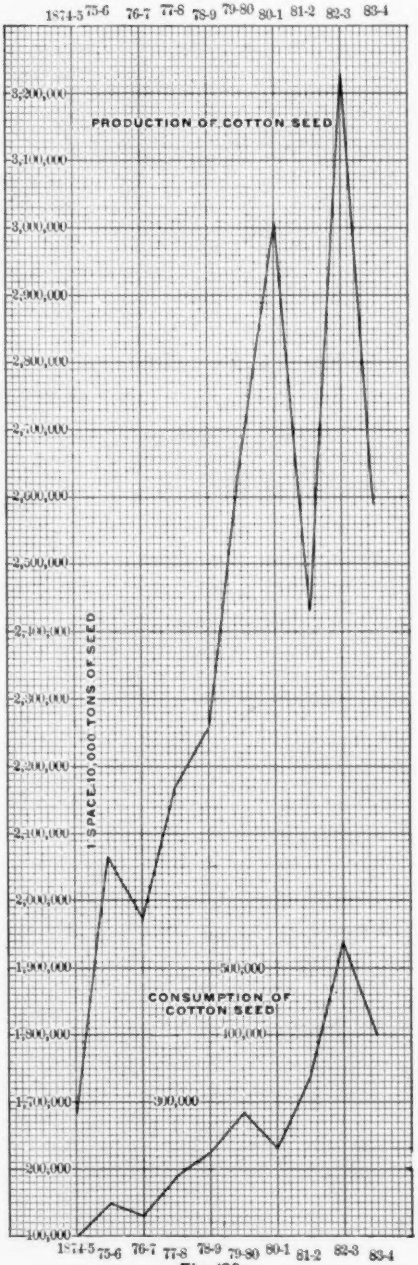


Fig. 126

used as substitutes for linseed, olive, lard, and lubricating oils, besides being made into lard and butter when these products were high. Perhaps the most extensive and successful uses to which it is now put is for the packing of fish. It is difficult for any one not an expert to discover the difference between fish packed in olive oil and those packed in the best refined cotton seed oil. As an adulterant for lard it is meeting with remarkable success, and it is said to improve the quality of the lard.

At one time the best refined cotton seed oil was largely used as a substitute for lard in cooking, but now, owing to the inferior oil put upon the market by ignorant or unscrupulous dealers, its use as such has been almost entirely abandoned. This use could yet be easily revived if cotton seed crushers would be very careful to keep separate all oil intended to be refined for cooking purposes, using only the best quality of seed, and keeping all storage tanks thoroughly cleaned, and finally putting it up in entirely new and clean barrels. It is the universal custom in the majority of mills to use second-hand

kerosene barrels for crude oil, but it is manifestly impossible to remove by the ordinary steaming process all the kerosene from saturated staves.

The oil as first expressed from the seed is known as prime crude, crude No. 1, and crude No. 2, according to quality. As yet there has been no regularly authorized standard by which these oils are accurately graded. The specific gravity of average prime crude is .926 and cold test about 36° F. Prime crude is otherwise known by its bright amber color, freedom from sediment, and freedom from rancid or bitter taste.

Refined oil is known as winter white, winter yellow, summer white, and summer yellow. The refining consists in part of neutralizing the vegetable acids in the crude oil by means of alkalies, generally caustic soda. Refining is regarded as a business distinct from the production of crude oil, and will not be considered in this connection.

The production of prime crude oil is dependent very largely upon the quality of seed. It can never be made from seed which have undergone any amount of heating. For this reason the greatest proportion of the seed should be stored in sacks to give ventilation. The sacks commonly in use are coarse "Dundee" or "Burlaps," holding from 100 to 150 pounds each. When stored in bulk, cotton seed occupy about 80 cubic feet per ton, though they can be loosely stored to occupy 90 feet or easily packed into 65 feet. The legal bushel in the State of Georgia weighs 30 pounds. The amount of warehouse room necessary for storing cotton seed in the ordinary course of business is about 400 square feet of floor for each ton worked per day.

It is customary for oil mills to run night and day, in order to crush all the available seed in the shortest possible time before they can spoil by heating. A great need in this business is some method of storing cotton seed, so that they will remain sound in large quantities for a year at a time. If this could be accomplished, the mills might operate with a smaller investment in machinery, for they could work ten or twelve months in the year instead of being compelled to rest idle for an average of seven months. Several plans for ventilating seed by blowers have been tried, but, so far, have all failed.

The machinery now in use for manufacturing cotton seed oil is an outcome of long and costly experiment. Until within the last six years the matter has been in the hands of comparatively ig-

norant operators, and the experiments were of the crudest sort. The first attempt in the United States to extract the oil from cotton seed was made in 1826. This was on a very small scale, and no attempt was then made to bring the oil into commercial notice. The first real oil mill was built at Natchez, Miss., in 1834, but it resulted in financial failure. Mills were built in New Orleans, Memphis, St. Louis, and Providence, several years later, but they were all failures. There were really no practical successes until after the war. At present there are about 118 mills in actual operation, many of them paying fair dividends.

Some of the principal machines in the cotton seed oil mills of the United States for the crushing of upland seed (which is the only kind yielding prime oil) are shown by outline sketches.

The sand and rubbish screen, in its usual form, is shown in Fig.

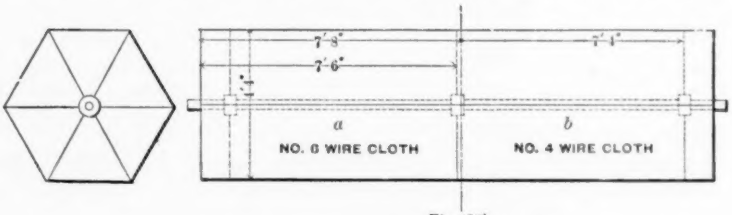


Fig. 127
SAND & RUBBISH SCREEN

127. The part a, designed to remove sand from the seed, is covered with wire cloth, having 36 meshes to the square inch, generally known as No. 6 mesh. The wire is .041″. The seed are delivered into this end of the screen, and after passing to the part b, drop through the meshes into a spiral conveyer which delivers them to some mechanism for removing odd pieces of iron and stone which escape the screen. An exhaust fan is generally connected with the sand-box under a, to remove the dust and carry it out of the building. The part b of screen is designed to remove all articles larger than the seed, such as stray locks of cotton, corn cobs, sticks of wood, and innumerable foreign articles that are always present in commercial cotton seed. The screen is covered with wire cloth of 16 meshes to the square inch, known as No. 4 mesh. The wire is .065″. This screen is run on an incline from the horizontal of $\frac{3}{8}''$ to the foot, and is driven by some kind of universal joint. It is occasionally built horizontal, with spiral ribs on the inside to convey the seed from one end to the other, but, for practical reasons, this plan is not recommended.

The correct proportions for this screen is for part *a*, about 8 square feet of cloth, and for part *b*, about 7 square feet, for each ton of seed worked per day. It should revolve at a surface speed of, as near as possible, 360 feet per minute. It is very important that this speed should be correct, for should it be too fast the seed would adhere to the sides and not pass through from one end to the other, while if it should be too slow, the seed would not be sufficiently agitated to allow of a separation from the foreign substances.

The arrangement in general use for removing heavy substances from cotton seed is shown in Fig. 128. The speed of the blower is governed by so many practical considerations that it can only be determined by actual experiment in each individual case. Magnets

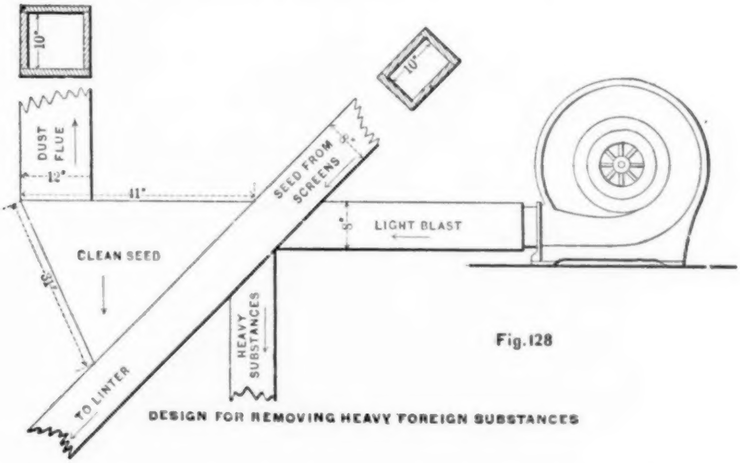


Fig. 128

DESIGN FOR REMOVING HEAVY FOREIGN SUBSTANCES

have been frequently tried for removing iron and steel from seed, but they are not generally successful, because they so easily clog the spouts and passage-ways. Throughout the construction of a cotton seed oil mill, this difficulty must be confronted. The seed are covered with a fine down or lint which is left by the cotton gin, and this prevents their gliding over each other like grains of corn or wheat. They can never be relied upon to go down any wooden spout which has a less angle than 45° to the horizon, and then it is necessary to have ample room. Any kind of a hopper for cotton seed, where they are expected to accumulate and then flow out, is considered a practical impossibility.

The first machine to which the cotton seed pass for actual manipulation is the linter, which is designed to remove the lint left on seed

by the gins. This is done to obtain the lint (which is an important by-product), and also to prevent the clogging of other machinery. The linter is in many respects similar to the ordinary cotton gin. It generally has twice the number of saws, and they are considerably closer together. There are two types of linter in use, one which rolls the seed in the breast, in exactly the same manner as a cotton gin. The most prominent linter of this type and the one mostly in use, is the E. Carver. The other type has only a single representative, known as the Payne linter. In this machine the seed pass by means of a spiral spike conveyer over the top of the saws,

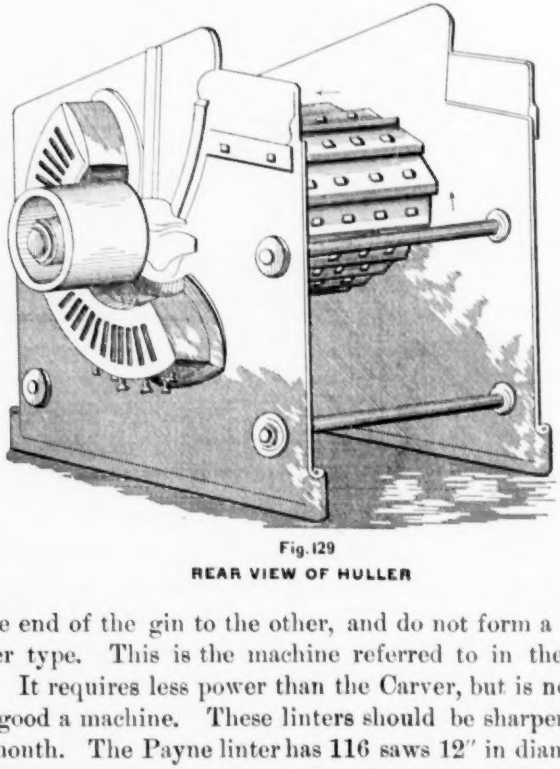


Fig. 129
REAR VIEW OF HULLER

from one end of the gin to the other, and do not form a roll as in the other type. This is the machine referred to in the table of powers. It requires less power than the Carver, but is not considered so good a machine. These linters should be sharpened about once a month. The Payne linter has 116 saws 12″ in diameter, and is rated for a capacity of ten tons of seed per day. The E. Carver linter has 106 saws, and is also rated as a ten ton machine. The surface speed of linter saws should be about 1,000 feet per minute.

The next machine to which the seed is taken is the huller, one form of which is shown in Figs. 129 and 130. This is known as the Wells huller, and is the one in most general use. They are at

present made in only two sizes, the smaller of which (with 20″ knives) is the one to which the table of powers relate. It will hull easily about 20 tons of seed per day. Both the cylinder knives and the concave knives (those in the frame) have four edges, so that when they become dull, they can be taken off and reversed without much delay. When the cylinder knives are changed the cylinder must be re-balanced. This is done by shifting leaden

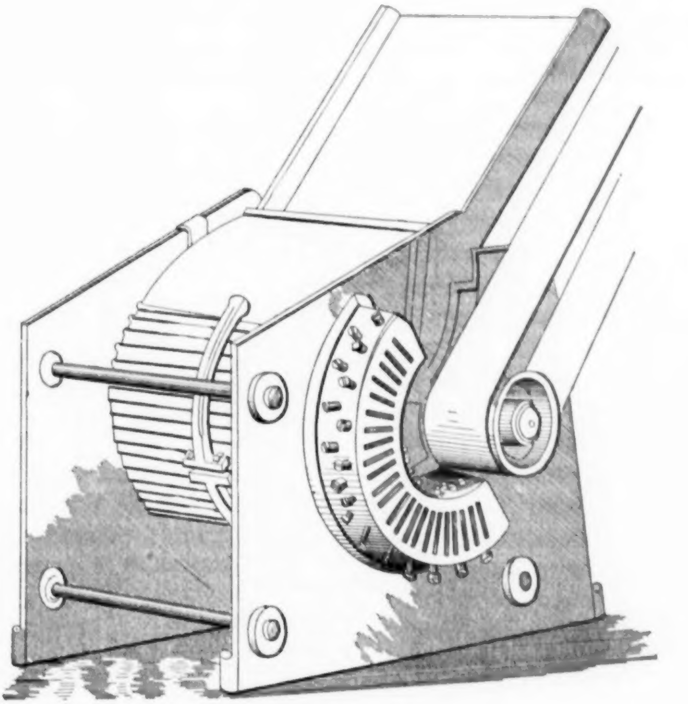


Fig. 130
FRONT VIEW OF HULLER

weights, secured on the inside. Each set of 20″ knives with the four edges will cut about 2,000 tons without being re-sharpened. The knives should have a surface speed of about 4,500 feet per minute. This machine does not separate the kernels from the hulls, but simply cuts the seed into pieces. These pieces are then conveyed to a revolving screen made like Fig. 127, except as to the covering. It is all covered with wire cloth having 25 meshes to the square inch, known as No. 5 mesh. The wire should be .057″. The

axis of this screen should have an inclination to the horizon of 1″ per foot, and it should revolve as near as possible at a surface speed of 290 feet per minute for ordinary sizes. There should be about eleven square feet of wire cloth for each ton of seed worked per day. Sometimes in large mills this is put on two or more screens, to avoid making them too large. The seed as they leave the huller drop from the elevator into the higher end of screen, and as they gradually roll toward the lower end, the kernels drop through the meshes and are carried away by a spiral conveyer. The hulls roll out of the lower end. These screens are often supplemented by "shakers," which are flat screens, having a reciprocating motion in

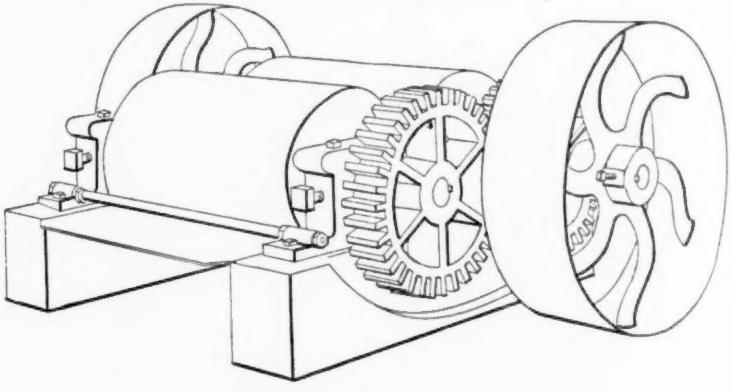


Fig. 131
ROLLS

their own plane. They are placed at a slight inclination to the horizon. The hulls from the screen fall on one shaker and the kernels fall on another, and thus the work of separation becomes complete. The hulls pass out of the mill to a room adjoining the boiler-house, where they are used for fuel. The kernels are conveyed to the crushing rolls, the simplest form of which is shown in Fig. 131. By this form of machine they are rolled flat to about .02″ thick and delivered to the heater.*

The rolls are often made without gearing, though it is not so good a plan to rely on the friction of the belted roll to drive the other. The object of dispensing with the positive action of gear-

* These crushed kernels will hereafter be referred to as meal. This is the term generally used in the mills, but it is ambiguous from the fact that the cotton seed meal of commerce is really the result of finely grinding the cake left after the oil is pressed out of kernels.

ing is to allow the rolls to have a slipping or grinding action on the kernels, to reduce them more effectually. The writer would propose that they still be geared, but that the required amount of slip be obtained by a slightly greater number of teeth in one gear than the other. This would obviate the great annoyance caused by the driven roll frequently stopping and choking. The position of the scraper is of some importance. It should make such an angle with the tangent to surface of roll that it will always be slightly crowded against it. In this position it will present a sharp enough

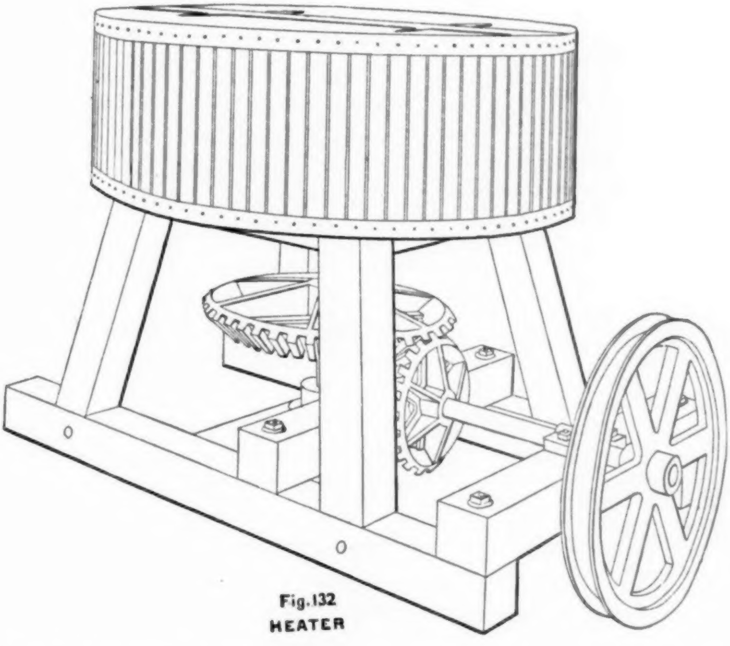


Fig. 132
HEATER

edge to remove the crushed kernels which have a tendency to plaster to the rolls. There is generally some simple mechanism, such as a fluted roller, for distributing the kernels evenly to the rolls, but sometimes this can be accomplished by a very flat and wide spout delivering the kernels. The most approved method now in use for crushing the kernels is by a kind of gradual reduction process. About five rolls are placed in a frame one above the other, and the kernels pass from the top set, which commence the process, to the bottom set, which leaves them very fine. The object of rolling these kernels is to crush the oil cells, and also to allow of their being more thor-

oughly cooked. Rolls should have a surface speed of about 550 feet per minute. They should be made of chilled iron and carefully ground in a lathe. Ordinary soft iron is badly indented by the accidental grinding of nails; they are also very easily worn out of round. The meal is now cooked in a large circular cast-iron kettle, having a boiler-iron steam-jacket, and some kind of revolving agitator in the center. One form of heater is shown in Fig. 132. By the process which has been in use ever since the business was commenced on a successful scale, the cooked meal is placed in woolen bags, and these are spread out and equalized in thickness on "mats." These mats are closely woven from horse-hair, and covered with a leather back, the whole very much resembling a book which opens at the end. The mat is closed up on the woolen bag of meal, the open end of bag being folded over itself next to the hinge of mat. It is then placed in a compartment or "box" of the press, shown in Fig. 133. When all of the boxes (usually six in number) are thus filled, they are subjected to a hydraulic pressure of from 2,200 to 4,000 lbs. to the square inch upon the ram. The common practice is about 2,500 pounds, and the rams are generally 16 inches in diameter. Upon a ram of this dimension there would be a pressure of 502,656 pounds. The cakes formed are usually 9 inches wide at one end, 10 inches at the other, and 24 inches long. They are therefore subjected to a pressure of 2,205 pounds per square inch. The cakes are kept under this pressure for fifteen or twenty minutes, in order to allow the oil time to escape freely. Channels in each box conduct the oil to back of box, where it runs through a hole to a box below, and finally to a trough which leads to a settling tank. When the press is lowered, the woolen bags are stripped from the cakes and again filled with cooked meal for

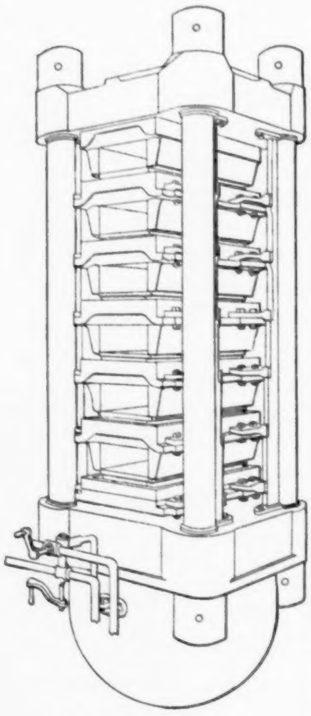


Fig. 133
BOX PRESS

another operation. The cakes are generally trimmed on the ends, to remove small portions which still contain some oil. When these trimmings accumulate, they are ground up and pressed, yielding a somewhat inferior oil. The cakes are stacked up in ventilated piles and allowed to dry twenty-four hours or longer, and then they are ground into fine meal for domestic consumption as fertilizers or cattle food. Sometimes the cakes are packed up whole for the ex-

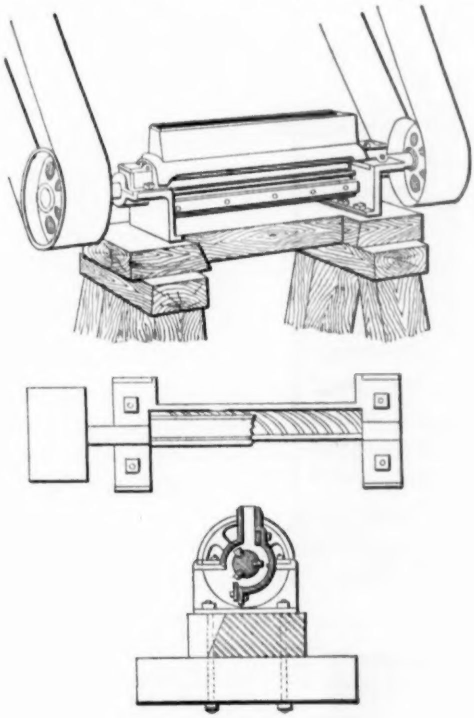


Fig. 134
CAKE BREAKER

port trade. Nearly all foreign countries prefer it in this shape, owing to their fear of adulterated meal.

Fig. 134 shows a common cake breaker in use in small mills for cracking the cake into pieces small enough to be ground in an ordinary corn mill.

The ground cake is commercially known as cotton seed meal. It is usually put up in 100-pound sacks called "centals." Its value as a fertilizer is shown by the following analysis of an average specimen of meal:

	Per cent.
Ammonia (Nitrogen Det.)	8.44
Phosphoric acid	3.55
Potash	1.55

Cotton seed meal is largely used as a supply of ammonia in all Southern fertilizer factories. In the North and East it is most highly prized as a cattle food. Its value as a food is due to the large proportion of protein, starch and fat.

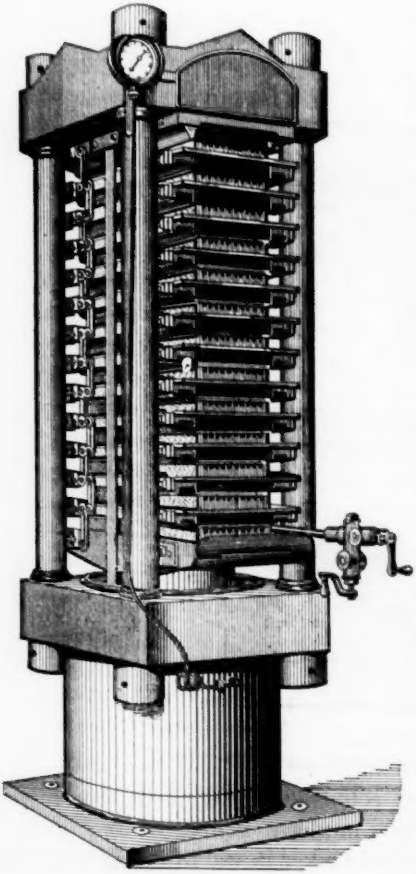


Fig. 135
PLATE PRESS

The hydraulic pressure used in cotton seed oil mills is usually produced by a power pump driven by a belt, and arranged with pistons of different sizes, the largest being used in starting the presses, and the smallest being thrown in when the pressure begins to increase. Within the past few years, direct-acting steam-pumps have been used with considerable success in this business. In respect of being independent of the other machinery of the mill, and of being more positive in action, they are of great advantage, but are generally very wasteful of power.

Fig. 135 shows the latest design of cotton seed oil press, intended to render unnecessary the woolen bags and horse-hair mats. This type of press will eventually supersede all the box presses, owing to its great capacity for oil, and to its compactness, but most of all to its great saving in expense, both in operation and in the mats and bags. Hundreds of experiments have been made to find some cheap substitute for horse-hair mats used with box presses, but in the end they have all

been unsuccessful. The real solution of the problem lies in adopting the new style of press, which uses iron, steel or brass plates to take the place of boxes, mats, and bags.

Plate presses of various kinds have been in use in linseed mills for many years, and, to a great extent, have been used in cotton seed mills in England. But the decorticated meal resulting from the upland seed used in our Southern mills cannot be successfully worked in a press which might give good satisfaction in a linseed mill, or in a cotton seed mill, where, as is the case in England, the seed are ground up and pressed without being separated from the hulls. There is but one mill in the United States working seed from long staple or sea island cotton. The seed are very smooth, and, owing to the difficulty of separating the hulls from kernels, they are always ground and pressed in the English fashion.

The exact shape of plate for decorticated cotton seed has only been lately determined. The great difficulty has been in the tendency of the meal to squeeze out around the edges of the plates, and run with the oil, and even now it is a source of some trouble. With these presses a "former" is used which moulds the meal into cakes of the exact shape of plates in the press. The pressure on the "former" is now generally produced by the action of a ram similar to that of the oil press, except that it is operated by steam instead of hydraulic pressure. This is evidently a very wasteful method, since it uses a whole cylinder full of steam for moulding each cake.

The pressure exerted on this mould is just a little less than is necessary to bring the first oil, and this relieves the hydraulic press of considerable travel. By this means twelve or even thirteen cakes can be pressed in the same space which six cakes with their mats would occupy in a box press. The cakes made by the plate presses are larger in area than those from box presses, an eleven pound cake being 11½″ by 32″. In order, therefore, to exert with the same ram the same pressure per square inch on these as on the others, it is necessary to increase the pressure upon the ram; but this is always possible, because the greatest danger of breakage in presses is in the upper movable parts, and the plates have proved less likely to break than boxes. Thus it is quite practical to change a box press into a plate press without great expense.

For the storage of oil there should not be less than two large iron tanks in each mill. The oil made each day is pumped from the small tanks under the presses (designed to hold about one day's run) to one storage tank, while oil is being drawn into barrels from

the other. The capacity of the tanks should be such that it will not be necessary to draw oil for at least three days after the last oil is pumped in that tank. This is to allow the heavy oil and stray particles of meal to sink to bottom of tank. The pipe for drawing oil into barrels should enter the bottom of tank and be continued by a leather hose or pipe with flexible joint in such a manner that the receiving end inside can be raised and lowered at will. In this way oil may be drawn at all times from near the surface, and the operator can see just when all the light or prime oil is drawn. The heavy oil should be drawn about one-third as often as the light oil. The heaviest of this is called "soap-stock," and the other "tankage."

The cooking of the meal is probably the most important operation in the whole mill. Upon that depends both the quality and quantity of the oil. The durability of press cloth and mats also depends upon this operation. The length of time for cooking a certain amount of meal cannot be definitely stated, because it depends upon the quality of seed as to dryness, and upon the steam pressure used. This matter having never received any scientific attention of which the writer is aware, he has made a large number of experiments with the heater shown in Fig. 132, the results of which are appended.

The proper speed for the two 30-inch agitating arms seems to be 31 revolutions per minute. Faster than this throws the meal too close to the outside wall, and destroys the efficiency of the steam surface of the bottom. A lower speed allows the meal to advance in bulk ahead of the arm, and it is therefore not sufficiently stirred. The proper way to ascertain the exact length of time for cooking meal from a given kind of seed, with a certain steam pressure in any particular make of heater, is by testing the temperature of the cooked meal. This should be frequently done, and the required number of minutes for all steam pressures, within the ordinary range, tabulated for the use of the attendant. This, however, has not been done in practice. The judgment of an expert attendant is relied on to determine by the touch and smell when the cooking is complete. But the most common way is to cook for a stated number of minutes regardless of steam pressure. This is obviously incorrect, except where a regulating valve is employed to keep steam at a constant pressure. The opinion of the writer is that the proper temperature of cooked meal is under all circumstances 220° F.

The amount of steam necessary to produce this amount of heat depends of course upon the specific heat of the meal. This has

been determined by the writer from a series of experiments by the method of mixtures. For the seed found in Southern Georgia and Northern Florida, the mean of all the determinations was .413. The data and method of one determination, which gives a result quite near the mean, is here submitted. A heavy wooden tub was used for a calorimeter, and no account taken of the heat transmitted to that or to the wooden stick used to stir the mixture. There are such variations in meal from different kinds of seed, that it would be of no great consequence to take into account these refinements. In order to equalize the temperature and prevent too great a transfer of heat the experiment was performed one time in the tub before data were observed.

Let W_1 = Weight of Meal.
W_2 = " " Water.
t_1 = Temp. of Meal.
t_2 = " " Water.
t_3 = " " Mixture.
x = Specific Heat of Meal.

Then $x = \frac{W_2 (t_3 - t_2)}{W_1 (t_1 - t_3)}$.

In this determination
W_1 = 10 Pounds.
W_2 = 20 "
t_1 = 225 Degrees F.
t_2 = 62 " "
t_3 = 90 " "

$$x = \frac{20\,(90 - 62)}{10\,(225 - 90)} = 2\,\frac{28}{135} = .415.$$

The other determinations range from .389 to .447. In order to obtain practically the amount of steam used in cooking the meal, the water of condensation which resulted from the cooking of one ordinary charge to the proper temperature was weighed several times, with results not greatly differing from each other. The data of one such test is appended.

Weight of cold water in calorimeter	**22 lbs.**
Temperature " " " "	**62° F.**
Weight of mixture after water of condensation was added	**48 lbs.**
Temperature of same	**128° F.**
Weight of meal cooked	**282¼ lbs.**

Average steam pressure .... 51 lbs.
Time .... 20 min.

Heat in the cold water 62 × 22 .... 1,364 B. t. u.
Heat in mixture 128 × 48 .... 6,144 B. t. u.
Heat represented by water of condensation 6,144 − 1,364 .... 4,780 B. t. u.
Weight of water of condensation 48 − 22 .... 26 lbs.
Temperature of same 4,780 ÷ 26 .... 184° F.
Total heat in 1 pound of steam at 51 lbs. pressure ..... 1,205 B. t. u.
Total heat in 1 pound of water at 184° F. .... 184 B. t. u.
Heat expended per pound of steam 1,205 − 184 .... 1,021 B. t. u.
Total heat expended for 20 minutes 1,021 × 26 .... 26,546 B. t. u.
Total heat expended per hour .... 79,638 B. t. u.
Units of evaporation 79,638 ÷ 965.7 .... 82.47 U. E.
Horse-power of steam 82.47 ÷ 34.5 .... 2.4 H. P.

If, therefore, the water of condensation be trapped back to boiler without practical loss of heat, it will be necessary in designing a mill to allow steam capacity equal to 2.4 standard horse-power for each heater of this capacity used, or say, in practice, for each twelve tons of seed worked per day. If, however, a steam trap be not used, and this water of condensation be allowed to waste, as is frequently done, the total heat expended each twenty minutes is 1,205 × 26 = 31,330 B. t. u.

Total heat per hour .... 93,990 B. t. u.
Units of evaporation .... 97.33 U. E.
Horse-power of steam 97.33 ÷ 34.5 .... 2.8 H. P.

This shows a saving of $14\frac{2}{7}$ per cent. by using a steam-trap.

In order to form a check upon the determination of specific heat as found above, the water of condensation which accumulated in the empty heater was weighed. The difference between the heat consumed in elevating the temperature of meal and heater to the degree observed, and that naturally radiated from the empty heater for the same length of time, is the amount actually consumed in heating that amount of meal. The results with empty heater are as follows:

Weight of cold water in calorimeter .... 10 lbs.
Temperature of same .... 60° F.
Weight of mixture after adding water of condensation ...... $15\frac{1}{4}$ lbs.
Temperature of same .... 108° F.
Average steam pressure .... 56 lbs.
Time .... 10 mins.

Heat in cold water 60 × 10 .... 600 B. t. u.
Heat in mixture 108 × $15\frac{1}{4}$ .... 1,647 B. t. u.
Heat represented by water of condensation 1,647 − 600 .. 1,047 B. t. u.

Temperature of water of condensation 1,047 ÷ 5¼........199.4° F.
Total heat in 1 pound of steam at 56 lbs. pressure.......1,206.0 B. t. u.
Total heat in 1 pound of water at 199.4° F.............199.4 B. t. u.
Heat expended per pound of steam 1206.0 − 199.4....1,006.6 B. t. u.
Total heat expended for ten minutes 1006.6 × 5¼....5,284.6 B. t. u.
Total heat expended in 20 minutes in empty heater..10,569.2 B. t. u.
Do.—full heater.............................26,546.0 B. t. u.
Heat actually expended in elevating temperature of 282¼ pounds of meal (from 80° to 220°) 140° = 26,546.0 − 10,569.2 = 15,976.8 B. t. u.

Therefore the specific heat is $\frac{15976.8}{282.25 \times 140} = .404.$

The remarkable degree of proximity of this result to that determined by the standard method of mixtures (.413), considering the comparatively crude manner in which both tests were made, suggests that the noting of the amount of condensation of steam required for raising a given amount of any substance a certain amount in temperature might form a valuable method of determining the specific heat in many instances where it would not be convenient to use any of the other methods. This is therefore proposed as a new method for determining specific heat.

The power required to operate the principal machines in a mill consuming seed at the rate of 13 tons per day of 23 hours is shown in the annexed table.

Machine.	R. P. M.	I. H. P.
Engine and Shafting		4.42
Screens and Elevators		1.69
Linter	350	2.76
Huller	1,000	9.43
Rolls	156	2.62
Heater	31	1.79
Cake Mill	550	1.48
Cake Breaker	440	.35

Total, exclusive of hydraulic pump, which in this particular mill is an independent steam pump.............................. 24.54

In order to show approximately the calorific value of the fuel generally used in cotton seed oil mills, namely, cotton seed hulls, the following observations, taken in the course of a boiler test, are submitted.

Root's Wrought Iron Water Tubular Boiler, having 64 tubes...4″ × 12′.
Date of Trial.......................................Mar. 9, 1883.
Duration of Trial...................................2 hrs. 20 min.
Grate Surface.......................................25 sq. ft.
Average Steam Pressure by Gauge.....................57.3 lbs.

HULLS BURNED.

Total	1,552.0 lbs.
Per Hour	665.2 "
Per Boiler Horse-power	13.1 "
Per sq. ft. Grate Surface	26.4 "

FEED WATER.

Average Temperature	72.0° F.
Total Pumped into Boiler, corrected for inequality of Water Level and Steam Pressure at beginning and end of Test	3,506.5 lbs.

EQUIVALENT WATER EVAPORATED FROM AND AT 212°.

Total	4,118.7 lbs.
Per Hour	1,765.2 "
Per Pound of Hulls	2.65 "

HEAT DERIVED FROM FUEL.

Total	3,977,422.9 B. t. u.
Per Hour	1,704,609.8 "
Per Pound of Hulls	2,562.7 "

HORSE-POWER.

Total	51.17 H. P.
Per Pound of Hulls	.08 "

In the above calculation the H. P. is taken to be 34.5 lbs. water evaporated from and at 212° F. per hour. No account has been taken of the quality of steam as to dryness. It is a matter of some regret that it has not been possible to test this same boiler under the same conditions, using anthracite coal in the place of hulls, in order that a practical comparison might be made between the two fuels. Presuming, however, that under the existing conditions one pound of anthracite coal would have evaporated 9.5 pounds of water from and at 212° F. per hour, or would have imparted per hour 9,174.2 B. t. u., the value of cotton seed hulls as a fuel may be roughly estimated at .28 of anthracite coal.

If the production of hulls be 50 per cent. of seed worked, as has been generally estimated, a mill, using 51 H. P. of steam if running with hulls only for fuel, would have to work 15 tons of seed per day of 23 hours.

This is about all that can be done with this amount of steam. In the large mills, working 50 to 100 tons per day, the amount of steam per ton of seed worked is not so great, and where the steam plant is at all economical, they easily produce more hulls than is required for fuel, because the power necessary to drive the machinery, and notably the steam necessary for other purposes, is much less in proportion.

The yield of oil per ton of seed varies greatly with the character of soil upon which the seed grows, and with the seasons. In the Mississippi bottoms where the black, rich soil is several feet deep, and in favorable seasons, seed will yield as high as 38 gallons per ton by the "box" process, while seed grown on Georgia pine barrens, worked by the same process, will not yield, in an unfavorable season, over 29 gallons per ton.

The following table, representing the mean of a large number of determinations, exhibits the ordinary production from a ton of cotton seed grown in South Georgia. These determinations were made in a mill using box presses, and the other machinery generally in use in this part of the country.

TABLE SHOWING YIELD OF ONE TON OF COTTON SEED.

Product.	Pounds.	Per Cent.	Pounds.	Per cent.
Oil (31.5 gallons)			236.6	11.83
Soap stock			21.2	1.06
Cake			771.3	38.57
Linters			16.1	.81
Hulls			917.2	45.86
Waste (as below)			37.6	1.87
Motes (from linters)	4.1	.20		
Sand (from screen *a*)	23.3	1.16		
Rubbish (from screen *b*)	10.2	.51		
	37.6	1.87	2000.0	100.00

DISCUSSION.

Mr. Kent.—I would like to ask a question in regard to this boiler test. Did the fuel smoke much when it was burned under the boiler?

Mr. Thompson.—In the form of boiler which I used and with the arrangement which I used, having a forced draught, there is no smoke to speak of. It would smoke ordinarily on the grate without a forced draught.

Mr. Kent.—Does this cotton seed when used as fuel contain any moisture?

Mr. Thompson.—No, sir, except the small degree of moisture that it naturally absorbs from the plant.

Mr. Kent.—I am of opinion then that the result of this test is not a sufficient indication of the caloric value of this fuel. Cot-

ton seed hulls should contain chiefly woody fiber and some oil that had not been expressed out.

Mr. Thompson.—A very small proportion of oil indeed. The oil is contained in the kernel.

Mr. Kent.—When wood is used as fuel, an evaporation of four pounds of water is generally obtained per pound of wood. The reason you did not get four pounds evaporation with cotton seed hulls was probably on account of burning the fuel in a furnace not suited to that particular fuel. On account of the nature of cotton seed hulls they are almost instantly converted into gas when fire is applied to them and a good deal of that gas gets up into the tubes and goes off unburned; but if this was burned in a separate oven, as tan-bark is burned, you would get much better results.

Mr. Thompson.—I am obliged to you for the suggestion.

Mr. Kent.—I have known a Root boiler which failed completely because they did not fire it right. They fired it with shavings.

Mr. Barrus.—What kind of forced draught was used.

Mr. Thompson.—Just the exhaust similar to a locomotive.

Mr. Barrus.—What kind of a grate.

Mr. Thompson.—Similar to the Adams Grate: about a quarter of an inch air space. I can give you a sketch of the grate. (Mr. Thompson made a sketch of the grate.)

Mr. Barrus.—What was the dimension of this heater?

Mr. Thompson.—About five feet in diameter and twenty-two inches deep, made of cast iron on the inside with a wrought iron jacket on the outside and wood outside of that; a steam space between. (The speaker made a sketch of the section.)

Mr. Babcock.—I think we are very much indebted to Mr. Thompson for this paper. It gives us information in a line that is quite new and upon a subject that we know very little about.

Permit me to relate a rather amusing fact.

When I was in Milan, in 1882, I was talking with the United States Consul, Mr. Crane, in regard to the condition of the people and the products and exports of the country, when he stated that the exportation of olive oil had almost entirely ceased; that in fact they were exporting little or no olive oil from the country. I asked him why that was? "Well," he said, "because they have put a prohibitory duty on *the importation of cotton seed oil.*" The custom had been, so he stated, to import cotton seed oil very largely and putting a little olive oil with it, to export it for pure olive

oil, and we ate it as such at our tables in America. But since the Italian Government had put such a duty upon it, the exportation of olive oil had ceased to be profitable! and we in America were obliged to eat our cotton seed oil pure.

Mr. Oberlin Smith.—I want to ask Mr. Thompson where most of the machinery for the working of cotton seed oil is made. I know I saw some at the New Orleans Exposition that seemed to be of new design, and in some cases all the different machines were arranged upon a common base, in a row, so that the thing was rather portable. There appeared to have been a good deal of study put into the matter of convenient manipulation. This particular machine was made in England, and it occurred to me to inquire whether the business had been worked up there more than here.

Mr. Thompson.—The very first machinery that was ever used for the manufacture of cotton seed oil was brought from England. It was manufactured by Rose, Downs & Thompson, at Hull. The machinery that is now used in cotton seed oil mills in this country is manufactured in this country. Most of it is made in Dayton, Ohio. The form of press illustrated in this paper is manufactured in Dayton, Ohio. There is a firm in Brooklyn that manufacture a press outfit all on one base—three or four of those box presses on one solid, heavy base with a heater on one end and a power pump on the other, a very compact arrangement. It is all out of date now because of these new plate presses.

Mr. C. E. Emery.—I feel gratified to see that mechanical engineers are going down there to bring about new methods and making these investigations as a basis of finding a better way to do it. I join Mr. Babcock in personally thanking the gentleman for the information he in many ways has given us to-day.

The President.—The paper presented by Mr. Thompson is interesting—first, as it accurately describes a process of manufacture with which I fancy few of our members are familiar, not so much owing to the fact that it has so recently assumed so large proportions, as to the fact that this manufacture is carried on in a part of the country not abounding in Mechanical Engineers.

In illustrating the value of the oil produced, etc., the author has adopted the graphic method, and a glance at his diagrams shows that his terminal lines, like those of commerce and manufactures generally, are all pointing downward. It would seem that the process, and the mechanism used for the production of cotton

seed oil resemble those used for the production of linseed oil, except as to the machines used to cut the seed previous to its going to the rolls. I would like to ask Mr. Thompson, if he is aware of any experiments that have been made, as to the making cotton seed oil by a process which is now being adopted generally in the West at least, for making linseed oil, in which the presses are entirely dispensed with, the extraction of the oil being accomplished in a chemical way. I do not know whether it has ever been applied to cotton seed oil or not; but it is claimed for the process that it is a much more rapid method of making oil from flax seed, and that a much larger percentage of oil is obtained in that manner.

Mr. Thompson.—I would say that this method has been adopted experimentally in the manufacture of cotton seed oil, but never has been adopted commercially. It is done by the use of naphtha or some of the higher products of petroleum by leaching the oil out after the seed has been crushed or passed through some other process, and then this lighter oil is distilled leaving the cotton seed oil. It is found almost impossible to take the odor of this petroleum out of the cotton seed oil, and it has another disadvantage, that of a terrible fire risk. As it is now the insurance companies charge us from 5 to 6 per cent. insurance on our mills and the inspectors are constantly complaining. Though we make the mills in the very best way the inspectors are constantly finding fault. I had intended to make some allusions to the requirements of insurance in this paper, but it ran to such a length that I had to leave it out. There is a basis adopted by the tariff associations for a standard cotton seed mill on which the rate is 2 per cent., and they add so much for each deficiency. The mill, as provided for by these associations, is to be built of brick and each separate machine placed in a separate house, and that makes it almost a commercial impossibilityto operate the mill.

CLXXIII.

ON THE THEORY OF THE FINANCE OF LUBRICATION, AND ON THE VALUATION OF LUBRICANTS BY CONSUMERS.

BY ROBERT H. THURSTON, HOBOKEN, N. J.

It is proposed by the writer, in the following paper, to consider the errors of the customary methods of finance, in their relation to the subject of lubrication, to exhibit the correct theory of such finance, as it appears to him, and, finally, to show what is the proper method of valuation of lubricants. The singular and important losses which often arise from the common system of purchase, application, and valuation of the various unguents, will be exhibited, and illustrations will be presented, both of the present and of the proposed systems.

This subject was first studied by the writer some years ago, and it was shown, in a work published at that time,* that very serious losses may follow the application of lubricants to machinery with no other guide to economy than the relation of their market prices and their friction-reducing power and endurance, under the given conditions of use. The equations here presented were, in most cases, subsequently developed in illustration of the principles outlined in that work, and were first presented for publication in a paper read before the American Society of Civil Engineers, as a portion of a discussion at a recent date.† The whole subject has also been traversed in the later work of the writer on the subject of friction and machinery,‡ in which a chapter is devoted to the matter of finance in this connection. The following paper is intended to give a more detailed exposition of the subject than has hitherto appeared, and to present a larger collection of facts and data than could properly be given in the later work. A large pro-

* "Friction and Lubrication." New York, 1879.

† "On the Real Value of Lubricants," etc., Jan. 7th, 1885; Trans. Vol. XIII., p. 476.

‡ "Friction and Lost Work in Machinery and Mill-Work," New York, 1885.

portion of this information also was not available at the earlier dates.

The writer is under obligations to Messrs. C. J. H. Woodbury, J. C. Hoadley, and T. N. Ely, for information which they only could satisfactorily supply, as well as to a number of other gentlemen who have kindly contributed facts and figures of great interest in this connection.

The Cause of Lost Work, in machinery, will be found, on the most casual examination, whatever the kind of mechanism, to be due, in every instance of well-designed and properly managed apparatus, to the friction of parts moving in contact and under pressure. Further examination will show that it is the almost universal custom to endeavor to reduce this waste of energy to a minimum, by the use of lubricants which are interposed between the rubbing surfaces. The inference at once follows that the unguents form a class of materials the importance of which can hardly be overestimated. The efficiency of these substances, and their value in the market, thus become matters of supreme importance, since the efficiency of the lubricant determines to what extent it is possible to reduce such losses of work and energy, and the price determines to what extent this is commercially allowable, and the cost of such economy of work and power. There is evidently some relation of price, efficiency of unguent, and value of work saved or wasted, which will determine just what lubricant may be best used. It is evident that the relation of either two of these quantities does not indicate which is the proper lubricant, or give the real value to the consumer of that which he may select for use.

It is obvious that the real value of any friction-reducing material has no necessary or direct relation to its market price, except in so far as that price comes in to determine the financial aspect of the question which arises when it is necessary to choose which of any number of available materials is, on the whole, best for a specified case. Its real value is actually dependent, not only upon its own intrinsic properties, but also upon the value of the power which is to be saved by its application. Its value to the consumer is thus dependent upon economical conditions entirely apart from those of its production by the vender—conditions which include all which influence the total cost of the power saved or wasted, indirectly, as well as directly.

The losses due to the friction of the working parts of machinery include vastly more than the mere loss of power in overcoming that

friction. They involve, often, an enormously greater amount of expense in the meeting of incidental losses, in the wear and repair of bearings and of journals, in the expenses arising from accidents traceable, more or less directly, to the friction of working parts, and in other less easily determined losses. Fires are sometimes caused by the use of improper lubricants, or by inefficient lubrication; costly steam engines, and other machinery, have been ruined by "break-downs" in consequence of the excessive friction and abrasion caused by the use of such unguents; and fine mechanism is often seriously injured by the change of form and the "cutting" so produced.

The common system of valuation of lubricants, and of purchase, if it can be considered a system, consists simply in a comparison of the market price of the available kinds with their friction-reducing power and their endurance, where these qualities are known. Even these essential quantities are seldom determined with any degree of accuracy, and the buyer is compelled to take what he can find, try it upon his machinery, and, if it produces no perceptible ill effect, to purchase at the best rates which he can obtain, and without being able to ascertain to what extent he is the gainer or the loser by changing from one kind to another. It is usually assumed that of two oils having prices proportional to their endurance, or to their friction-reducing power, the purchaser may take either with practically equally satisfactory commercial or financial results. The advisability of purchasing is considered to be settled by this comparison. No comparison is made between the costs of wasted power and the expense of purchase of lubricants, as a rule, even by the most experienced buyers. It is perfectly obvious that such a system must be absolutely wrong, as must any method which does not take into account every item of profit and loss dependent upon the use of the unguent, and which does not thus make it possible to make up a balance-sheet including all such items. The real question is not whether the difference in the price of any two oils is justified by the difference in their intrinsic qualities, but whether the profit to be made in the purchase of the one, rather than the other, is not more than compensated by the cost of the difference in power, and in running expenses, produced in the mill, or the shop, or on the railway train, by such substitution. If a dollar expended for oil may be made to show a profit of 25 per cent., in any given case of a change of lubricant, as represented on the books of the purchaser, it by no means follows that he has gained by the operation. It is

very likely to be—and, indeed, is often—the fact, that a comparison of the total running expenses of the establishment will show that this apparent profit has been made by the production of an actual net loss, in cost of power and other expenses, vastly exceeding the saving in purchase-money. To make the change advisable, it is evidently necessary that the total cost of operating the establishment must be reduced permanently, or so long as the new oil is used, by the substitution of the latter for that formerly in use. To determine whether the change is profitable, therefore, it is necessary to ascertain just what are the items of expense affected, and to what extent the proposed change will alter their total amount.

The total expense to be charged to the friction of machinery consists of the following items, as principals, and probably of minor and less easily determined expenses :—

(1.) The cost of power produced, only to be wasted by that friction.

(2.) The expenses incurred in wear and tear of running parts, and in the replacement of parts destroyed, either by direct strains or by gradual wear due to such exceptional resistances as are the effect of excessive friction.

(3.) The casual, the indirect, and often unperceived, yet none the less serious, losses throughout the system which are not included in the above.

(4.) The cost of the lubricating materials applied for the purpose of ameliorating these losses.

The first item includes a part of the expense of the prime mover, such as cost of fuel and oil used on the motor, interest on the capital invested in the machine, and in the machinery of transmission, wages paid engineer and fireman, or other attendants, insurance and taxes upon that part of the plant, including so much of the building as is properly chargeable to the motive-power department. The second item includes costs of repair, refitting, or replacement of journals and of bearings, the repair of break-downs caused by excessive friction, or by hot bearings seizing the journals, and, often, the cost of throwing out the whole machine and introducing a new one to take its place. The conditions determining the life of the machine, in fact, are what are included under this head. The third item includes the exceptional damages resulting from friction of excessive amount, and which may be more likely to occur with one oil than with another. Its amount can never be calculated with

any great degree of exactness. A hot journal may cause the delay of a train, and consequently a collision involving loss of life and destruction of property; a chronometer suddenly changes its rate, in consequence of the abrasion of some dry spot on its arbors, and causes the wreck of a steamship freighted with human beings and valuable cargo; the quality of the fabrics made in a cotton-mill making fine work, because of defective lubrication, is altered by the breaking of threads, by the imperfect action of looms, or by the lubricant spotting the cloth. Such losses are experienced very frequently in manufacturing, and in every mechanical operation, and are very seldom exactly calculable. The fourth and last item is usually, it is probable, the least important of all. It is, however, one which appears most prominently, and which is, therefore, most certain to appeal directly to the mind of the interested proprietor, and to the buyer of the oil. It will be seen, later, that this is so small an item, in many instances, that it becomes absolutely unimportant, as influencing the choice of lubricant.

The differences in the magnitudes of the losses comprehended in the first three classes of expenses, as above, are enormous; those occurring in the costs of oils and greases, as affecting those expenses become utterly insignificant in comparison.

Such are the several classes of costs, the variation of all of which must be considered in any system of valuation of lubricants, and in any systematic theory of the finance of the subject. It is obvious that a gain is effected, on the whole, only when the reduction of total cost, as above measured, is reduced; and it becomes economical to buy a cheaper lubricant only when its use leads to a decrease of expense in its purchase which is not compensated by an increase in the sum of the first three items above enumerated. Where all four items are diminished, the advisability of the change is indisputable; but when, as is, probably, usually the fact, decrease in cost of lubricant implies increase of friction and loss of power, a careful balance must be struck.

Before the real value of any lubricant to the consumer can be determined, therefore, and whether any proposed change is desirable on the score of economy, it becomes necessary to ascertain the total expense chargeable to friction, in the manner already indicated, and to compare the difference of cost of unguents with the difference in costs of other items of expense produced by change of lubricant in the manner intended. In making this comparison it is first necessary to determine in what terms these expenses may be

best expressed, and in what magnitude they enter the equations representing the problem.

(1.) The cost of power wasted may be expressed in the usually adopted terms, the cost in dollars per horse-power and per hour, or per year; or it may be given in foot-pounds of work, irrespective of time. The first of these methods of valuation is the more common. This quantity varies greatly in different localities and in different establishments; its average and fair value for ordinary cases will be given presently.

(2.) The cost of wear and tear, and of depreciation, is an even more variable quantity than that of power. It cannot be stated definitely and generally; but it may usually be very fairly measured for any given case. An allowance is customarily made based upon the value of all machinery subject to this kind of depreciation. It will always be permissible to take this expenditure, in any establishment considered, as proportional to the power employed, and to include it thus in the first item. It will be so included in the treatment here adopted. It sometimes happens that a decrease of the total power wasted by friction is accompanied by an increase in the amount of wear: in such cases the oil producing this remarkable effect, which is usually a mineral oil, without admixture, having too little body for its work, should be rejected without further investigation.

(3.) The third item, the casual and irregular losses, should, where possible, be made a constant and regular item of charge by securing insurance against all such kinds of loss. Where this cannot be done, the proprietor should insure himself by accumulating a fund to meet this expense, assuming a rate of accumulation which experience may determine to be safe, for a series of years. This item then becomes chargeable as so much insurance, and can be introduced, with other insurances, in the first of the above divisions.

All three of the charges above described may evidently be thus brought to one method of charging, and may be entered upon the account as so much per horse-power and per hour, or per year, or per foot-pound of work, if preferred.

(4.) The fourth and last item, the cost of lubricant, is measured by the charge per gallon, and by the number of gallons used per hour, or per annum, or for the specified work. This is, in every case, ascertainable by observation, or trial, either in the establishment in which it may be used, or upon the testing-machine, under precisely the conditions, if attainable, as to pressure, speed, temperature, and

condition of bearing surfaces, of its application, in the shop, the mill, or on the axle of a railway train.

Having thus reduced all expenses arising from the existence of friction to one measure, the dollar, and the cost of the lubricant being expressed in similar terms, it becomes easy to solve the most vital of the economical problems arising in this connection. These two items, thus determined, being ascertained in magnitude, for each kind of lubricant, a comparison of the totals settles the question of profit or loss. In every case, the exact value of the items being ascertained, the conclusion becomes evident and certain. It is further evident that the uncertainty arising, in any attempt to make application of these principles, comes, in every case, not from inexactness of the principles, but from the difficulty, should any exist, of determining the exact values and precise conditions.

The algebraic theory of this case is constructed very easily, and in a very simple manner, as follows:

If, in any case, U be taken as the measure of the power wasted, or of the work lost per hour or per annum, and if k' be its total cost, as above, for the unit of time, and if q be the quantity of the lubricant used in the same time and k is its cost, including its application to the journals, the total expense chargeable to friction, being called K, must be measured by

$$K = kq + k'U \qquad (1)$$

But the work done is equal to the product of a constant, a, dependent upon the units employed, the value of the coefficient of friction, f, the total load, P, and the space passed over by the rubbing surfaces, S, which last is also equal to the product of the velocity of rubbing, V, and the time, t, taken as the unit of comparison. Then

$$U = afPS = afPVt \qquad (2)$$

and the cost, K, thus becomes

$$K = kq + k'afPS \qquad (3)$$

or,

$$K = kq + bf \qquad (4)$$

in which b is a constant, in any given case, and equal to

$$b = ak'PS. \qquad (5)$$

and the equation, as thus simplified, may be applied to all cases.

The value of f is to be ascertained by experiment, under the exact conditions of use, and this being determined, the total cost

becomes calculable, and a satisfactory comparison may be made. That lubricant is best which, all things thus considered, gives the lowest value of K, and where wear of serious amount occurs, as may happen when using oils badly adapted to the pressure and speed, the cost of such additional wear must be put in as an additional charge. With properly chosen oils, in regard to which, only, any doubt can arise, in the endeavor to make a selection, this may be probably usually neglected.

The value of the oil, in terms of the total cost and of cost of wasted power, is

$$k = \frac{K - b\,f}{q} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

which equation shows that the value is the greater as the quantity demanded is less, and that it also increases when the coefficient of friction decreases. These equations, also, show that the total cost is very nearly proportional to the coefficient of friction when, as is the usual case, the cost of lubrication is small in comparison with that of the power wasted. The value of the lubricant is then very nearly proportional to the reciprocal of the coefficient of friction, and has no necessary, or direct, relation to the market price. It will be seen, later, that this conclusion is correct, and that the price of an oil, and even the quantity consumed, are matters of little importance, as a rule, in comparison with the amount of gain or loss of power by variation of friction of moving parts due to change of unguent.

Two oils being compared, the costs of wastes are, respectively,

$$K_1 = k_1 q_1 + b\,f_1; \qquad K_2 = k_2 q_2 + b\,f_2 \quad . \quad . \quad . \quad . \quad . \quad (7)$$

and the saving effected by the use of the better lubricant is

$$K_1 - K_2 = k_1 q_1 - k_2 q_2 + b\,(f_1 - f_2) \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

If the saving in value of power lost is just equal to the difference in cost of lubrication, it is evident that the change will not affect the total cost, and it is a matter of indifference whether it is made or not. That is to say, if $b\,(f_2 - f_1) = k_1 q_1 - k_2 q_2$, we shall have $K_1 = K_2$, and profit and loss are equal. When $b\,(f_2 - f_1) > k_1 q_1 - k_2 q_2$, the result is evidently a gain; and when the first member of the inequality is less than the second, a loss is effected. Thus we have a criterion of the advisability of changing the lubricant in the equations:

$$K_1 = K_2; \quad k_2 q_2 - k_1 q_1 = b\,(f_1 - f_2) \quad . \quad . \quad . \quad . \quad . \quad (9)$$

$$k_2 = \frac{k_1 q_1 - b\,(f_2 - f_1)}{q_2} \quad . \quad . \quad . \quad . \quad . \quad (10)$$

If two oils are compared, therefore, it is seen that, the first having the price, k_1, giving the coefficient, f_1, when used in the quantity, q_1, the second may be profitably used in the quantity, q_2, if giving the coefficient, f_2, only when it can be purchased below the price k_2 of equation (10), the two prices being considered as including the cost of application to the bearing and of removal. Should the oils compared have so little body that wear takes place to any appreciable extent, the cost of the wear is to be added to the cost of power, in each case.

If, in any case, as often happens, the quantity used is practically the same, whichever oil is used, $q_1 = q_2$, and the criterion becomes:

$$k_2 - k_1 = \frac{b}{q_2}(f_1 - f_2); \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

$$k_2 = k_1 - \frac{b}{q_2}(f_2 - f_1) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

The allowable purchasing price is below the value thus obtained.

Where the same oil is used, but may be applied in greater or less quantity, we may obtain, similarly, a criterion for the quantity to be profitably used. It is evident that the advantage of increasing the quantity is to be found in the reduction of the cost of power and incidental losses. If, in any two cases, we get

$$k(q_2 - q_1) = b(f_1 - f_2) \, . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (13)$$

the gain just balances the loss, and the criterion becomes

$$q_2 = q_1 + \frac{b}{k}(f_1 - f_2) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

and, assuming it to be found, as is usual, that a decrease of power follows increase of freedom of supply of lubricant beyond the amount customarily given, the limit is reached at the above amount. This statement must, however, be qualified by the reminder that it is often possible to supply oil as freely as may be desired, without important loss, by the use of a good system of collection and renewal, with occasional purification. The comparison then lies as in the first cases, the costs including those of purification and replacement. As the friction of lubricated surfaces is sometimes affected to a very great extent by variation in the rate of supply, especially at high speeds of rubbing, this case becomes important. The lower the cost of the oil, in any case, and the higher

the cost of power, the more freely should the unguent be supplied to the bearing.

When the relative durability and the coefficients of friction of oils proposed in any case, are known by direct experiments made under the precise conditions of intended use, it is equally easy to determine, wear being neglected, what are their relative money-values. If an oil already in use be taken as the standard, and if an oil to be compared with it is found to have e times the endurance of that standard, the quantity used should be $q_2 = \frac{q_1}{e}$. If the second oil have a coefficient of friction h times as great as the first, the work of friction will be correspondingly reduced or increased, and the cost of that work will $b\,h f_1 = b\,f_2$. The total costs will then become

$$K_1 = k_1 q_1 + b f_1 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (15)$$

$$K_2 = k_2 q_2 + b\,h f_1 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (16)$$

and the criterion is obtained, as before, by making these expressions equal, whence

$$K_1 = K_2; \; k_2 = \frac{[k_1 q_1 + b f_1 (1 - h)]\, e}{q_1} = e k_1 + b e f_1 \frac{1-h}{q_1}. \quad (17)$$

Any cost, on the journal, less than this value of k_2 is profitable; any higher cost will produce a loss. At this cost, the user will neither gain nor lose by the change of lubricant.

It is obvious that the value of b may be expressed in any convenient units of cost quite as well as in those above taken. It is usual to measure costs on railways by amounts per train-mile, as, for example, pounds per train-mile, in measuring expenditures of fuel, miles run per quart, or per gallon, in rating expenditures of oil. As has been seen, these are but a part of the total expense; but it may sometimes be convenient to obtain an approximate estimate of the relative values of lubricants, on the assumption that wear and other costs may be neglected, and the value of b is then to be expressed in the money value of fuel used per train-mile; which cost will evidently be proportional to f. The relative costs will be proportional to the values of h.

In railway practice, it is often found that the cost of wear is a very serious item, but probably only when using an oil which is unfitted for use as a lubricant, and which should never be used at all, as some of the black mineral oils. Could the lubrication be as effectively carried on as in other cases, and could the dust be perfectly excluded, the

loss from this cause would become insignificant. Assuming this to be the case, we may write the equation for this case,

$$K = k\,q + d\,f \qquad \ldots \ldots \ldots \qquad (18)$$

in which $d\,f$ is the total cost of power per train-mile.

The criterion is found as before, substituting d for b, in the several equations already derived,

$$k_2 = \frac{k_1\,q_1 - d\,(f_2 - f_1)}{q_2} \qquad \ldots \ldots \qquad (19)$$

As, in all cases, the cost of power wasted is a function of the quantity q, if the law of variation can be ascertained and expressed algebraically, the most economical rate of supply can be ascertained in making $\frac{d\,K}{d\,q} = 0$, for a minimum. Similarly, the best value of k may be determined, for the case in which the same quantity is always used, by making $\frac{d\,K}{d\,k} = 0$.*

The numerical value of the items which have been found to constitute the costs of lubrication and wasted power must be determined before application can be made of the theory of finance just developed. Among these items, the price of oil, as has been remarked, is the least important; but it is that which is most prominent, as a rule, in the view of the buyer, as well as of the seller. These prices are continually varying, and it is impossible to find values for k which may be taken at all times probably correct, or as being always fair. The real value of the figure to be assumed is, as already stated, the cost of the lubricant on the journal, and includes the cost of application, as well as the market price. The figures given below will be taken here as fair values of the unguents named, as now sold in the market. In each case, the oil is assumed to be a good representative of its grade, such as may reasonably be expected to be obtainable in the market by any skillful and experienced buyer who may choose to secure it.

PRICES OF LUBRICANTS.

Sperm Oil, per gallon,	**$1.10**	**Heaviest Mineral Oil, per gal.,**	**$0.75**
Neats-foot, " "	**1.00**	**Medium Machinery Oil, " "**	**0.50**
Tallow Oil, " "	**0.70**	**Light Lubricating Oil, " "**	**0.25**
Lard Oil, " "	**0.70**	**Crude Well Oils, " "**	**0.20**
Greases, per lb.	**0.25**	**Kerosene (unrefined) " "**	**0.10**

* *Encyclopedia Britannica.* Art. Lubricant.

These figures can only be taken as illustrative. The prices obtained in the market for the machinery oils vary enormously, and without any fixed relation to their values. One maker has no spindle oil at a lower price than $0.40; while others make what they call spindle oils at one-half that price. Other oils and the greases, animal and vegetable, are subject to similar, but usually smaller, variations of price. In one case, the maker obtains less than 15 cents per gallon for a machinery oil; while the vender of a trade-marked oil uses the same grade, buying at that price and selling at a profit of several hundred per cent.

The quantities of oil used for the various purposes of lubrication differ quite as much, where distributed by different hands, as do the prices. It may probably be estimated that at least one-half of all the power expended in the operation of the average manufacturing establishment is applied to the work of overcoming the friction of lubricated surfaces. The coefficient of friction will average a high or a low figure according to the kind of machinery. The heavier the latter, the lower the friction coefficient. Light machinery gives a high value of friction, which is therefore very great on the spindles, and on the machinery generally, of the textile manufactures, lower on the heavy machines of the iron-working trades, and very low, comparatively, on the axles of engines and railway cars. The range is probably from twenty, down to one, per cent., or even less. It will be here taken as averaging, in good practice, ten per cent. in mills, five per cent. on heavy machinery, and one per cent. on railways. The oil must evidently be selected with a view to its use, the heavier pressures and lower coefficients being necessarily obtained with heavy lubricants—oils or greases—and the lower pressures and higher coefficients being given where the lightest possible oils are properly, and customarily, employed, with more copious supply.

The variation of friction with pressure, above alluded to, is illustrated by the following values of the coefficient of friction, as obtained by the writer, the journal being of steel, in good order, the bearing of bronze—gun metal—working at a speed of rubbing of 150 feet per minute, and running barely warm to the hand, conditions common in practice.

FRICTION AT VARYING PRESSURES.

OIL.	PRESSURE: LBS. PER SQ. INCH. KGS. PER SQ. CM.							
	4 0.3	10 0.7	25 1.8	150 10.5	200 14.1	250 17.5	300 21.1	500 35.2
Sperm	0.12	0.08	0.04	0.01	0.01	.01	0.005	0.003
Lard	....	....	0.06	0.014	0.013	0.011	0.006	0.004
W. Va	....	....	....	0.012	0.009	0.008	0.005	.....
Grease	....	....	....	0.025	0.020	0.015	0.012	0.011

The journal was lubricated, in the usual way, by means of an oil cup, the oil feeding down by a wick. As the cup was kept full, the supply was very free, and the figures are probably good for the case taken. These figures may be doubled by the selection of an unguent ill adapted to the work, and may be increased to almost any extent by abrasion and cutting. On the other hand, they may be decreased at least one-half by freer supply.

The amount of lubricating materials used in cotton mills has been investigated by Mr. Edward Atkinson, who finds that, in fifty-five mills, working on similar fabrics, and among which a variation of 20 per cent. should not have been expected, the actual range was 350 per cent. Subsequently, careful management reduced the average from \$10.03 to \$6.67 per 10,000 pounds of cloth made. Another and still more important effect of this investigation, and the publication of its results, was the expulsion from that market of inferior and dangerous oils. The oils found in use, and tested, varied in quality to the extent of 300 per cent. The best oils for these mills were reported by Mr. Woodbury as being mineral oils mixed with some sperm or lard, and having a gravity of about 28 to 32 Beaumé (S. G. 0.886 to 0.864).*

The total power used in these mills is found by Mr. Henthorn to average about 0.75 horse-power per loom, or 15.75 horse-power per 1,000 spindles, and to vary from 0.5 to nearly one horse-power per loom, or from 11 to 22 horse-power per 1,000 spindles. The same authority finds the power demanded by engine and shafting alone to form from 17 to 34 per cent. of the whole. The smaller of these figures represents the best practice, the higher figures show what may be expected with faulty arrangement and bad lubrication. The quantity of cloth made in New England mills, according to data furnished the writer, mainly through Messrs. Hoadley and

* Transactions of the American Soc. Mech. Engineers, Vol. IV., p. 319.

Woodbury, ranges from about 2,000 pounds per annum, per horse power, up to above 3,000. A variation of temperature, such as occurs between winter and summer, causes a variation of ten per cent. in the production of cloth, the greater amount being obtained in summer. A mill making print-cloths, 64 threads to the inch, and of No. 32 yarn, with frame-warp and mule-filling, produces about one pound of cloth per spindle per week, and demands about 16 horse-power per 1,000 spindles. Thus 10,000 pounds of cloth per annum requires 200 spindles, and proportional plant, costing about, at present prices, or a little above, $11 per spindle for all machinery and buildings, exclusive of stock, land, and live capital, and the power demanded is not far from 3.3 horse-power. A New England mill, well known to the writer, contains 44,752 spindles, makes 7,800,000 yards of cloth per year, weighing 1,240,000 pounds, the mill running 3,000 hours per annum, using 625 horse-power, and consuming 700 gallons of sperm and 2,900 gallons of mineral spindle oil per year. The sperm oil cost, at last reports, $1.22 per gallon, and the spindle oil 25 cents. Another large mill, near Boston, working 55,000 spindles, makes, annually, about 3,000,000 pounds of cloth, requiring 1,300 horse-power, and using oil costing $1,300 per year. A 37-set woolen mill makes 750,000 pounds of cloth and uses 300 horse-power.

On the whole, it may be said that the quantity of oil used in cotton mills varies from 10 to 30 gallons per 10,000 pounds of cloth made per annum, averaging not far from two gallons per horse-power per annum, and costing from 20 cents to $1 per gallon, averaging probably not far from 50 cents.

A machine shop uses, properly, a much heavier oil than a cotton mill, and much less in proportion to power employed. One of the largest and best known steam-engine building establishments in the country uses 120 horse-power, and consumes 350 gallons of sperm oil for the heaviest machinery, such as planers, and for the wood-working tools, 100 gallons of finest heavy mineral engine oil for the engine, 300 gallons of mineral or mixed oil, at about 40 cents per gallon, for shafting and tools, and expends a total, on lubrication, of about $600 per year, an average of about 80 cents per gallon. Another and smaller shop, also well known to the writer, employs but 30 horse-power, and uses 175 gallons of oil per year for lubrication, the average cost being but 18 cents per gallon. Still another establishment, building tools and light machinery, uses an estimated amount of 100 horse-power and 700 gallons of oil at 20

cents per gallon, and for grease used on the main line of shafting, 25 cents per pound. A similar shop of somewhat larger size, reports to the writer an expenditure of but 40 gallons per year on 550 bearings, the oil being supplied by hand, a certain number of drops at a time, at regular intervals. Probably a fair estimate for the heavy class of machinery found in such establishments may be about 0.0002 gallon per horse-power per hour.

The Lowell Pumping Engine, designed by Mr. Leavitt, uses about 0.00015 gallon per horse-power per hour, a very low consumption for that class of machinery.

Railway work is probably more exacting in its demands, and more variable in its practice, in regard to quantity of oil used, than any other class of machinery consuming lubricants. Its character is such that it should be comparatively easy to select the best lubricant for the case; and yet there is probably no class of machinery on which a wider range of quality and price of oil is to be found in use. The trade in oils for this work is in a singularly unsatisfactory state; and no system is generally practiced by which to determine precisely which oils are best for the purpose. Consequently, the losses due to mistakes in selection and in the use of oils are often of enormous magnitude. In no direction is a definite method of test, purchase, and use of lubricants more desirable than in this.

The consumption of oil is usually reckoned per train-mile, and the following are the figures given by one of the best of the Massachusetts roads for one month on the engines alone:

	Coal—Lbs. per Mile.	Oil—Gallons.
Best Express Engines	43	0.009
Best Freight Engines	55	0.0094
Average Passenger Engines	50	0.009
Average Freight Engines	61	0.0084

The Boston and Albany Railroad, in November, 1884, reported the following:

Cost of fuel per mile	$0.179
Cost of lubricants per mile	.0052
Cost of repairs per mile	.0441
Total	$0.2283
Miles run per ton of coal	39.18
Miles per quart of oil	28.93

On the Boston and Maine road, the cost of fuel is given at about ten cents per train-mile, that of oil about one-half cent. The average of all Massachusetts roads in one year, as given to the writer,

was for wages on the engine, $0.28, for fuel, $0.115, and for oil and waste, $0.0105. On the Pennsylvania Railway, between New York and Pittsburgh, in 1883, the totals were reported as follows:

Total mileage	15,625,478
Coal, tons of 2000 lbs	743,020
Wood, cords	13,685
Oil, quarts	613,478
Tallow, lbs	721,992
Waste, lbs	267,158
Tons moved one mile	2,996,893

The coal averaged in price but $1.00 per ton, exclusive of freight charges to the road, the wood cost $2.88 per cord, the oil 28 cents per gallon, the tallow and waste, each 8 cents per pound.

The New York, Lake Erie & Western Railway reports for December, 1884, as follows:

PERFORMANCE OF LOCOMOTIVES.

AVERAGE, MONTH OF DECEMBER, 1884.

Length of Road .... 563 Miles.
Total No. Locomotives, 241. No. making Mileage, 205.
Miles run per Locomotive 2474.30
No. of Cars per Trip—Passenger 5.0, Loaded Freight 18.9
Percentage of Empty Freight Cars of all hauled 14.4
5 Empty Freight Cars rated at 3 Loaded.

MILES RUN TO

1 Ton of Coal by Locomotives 20.46, by Cars hauled 332.21
1 Pt. Lub. Oil & T. by Loco's 13.55 " " 179.90
Lbs. Waste used per 100 miles 0.32

COST, ETC., PER MILE.

		Loco. Mile.	Car Mile.
Fuel used,	pds.	95.36	7.19
Cost of Fuel	cts.	6.16	0.47
" Oil, Tallow and Waste	"	0.52	0.03
" Repairs	"	4.02	0.26
" Loco. Furniture and Fixtures	"	0.13	0.01
" Wages Engineman and Fireman	"	6.34	0.39
" Repairers and Cleaners	"	1.01	0.06
Total Cost		18.18	1.22

Cost of Wood per Cord, $2.74. Coal per ton, $1.25.
Time table mileage allowed and 5 m. per hour for Terminal Switch.

COST PER MILE, ETC., FOR

Repairs of Passenger Cars, 0.53 cts.	Repair of Freight Cars, 0.25 cts.	
Lub. Oil, Pass. Train Cars, 0.06 "	Freight Train Cars, 0.01 "	
Miles per Pt. " " 56.56 "	" " 210.32 "	

The cost of wear of journals and of bearings does not appear, as a separate item, in the preceding statistics of railway economy; but, as already stated, it is often a very serious expense. It has been found that, in some instances, the average wear is about one pound of journal for each 75,000 miles run, and the same weight of bearing for each 25,000 miles. Could the lubrication be made as perfect and the dust be excluded as thoroughly as in indoor work, the wear would be reduced to a fraction of one per cent. of these amounts, and would become insignificant. In one case reported to the writer, it was found that the cost of oil, for 100 cars moved 100 miles, was about $1.00, that of power $8.00, and the expense for wear of journals and bearing was $5.00 nearly. The estimate for cost of power is perhaps rather low. This remarkable case was observed where the lubricant was "black oil." The tendency of this oil to produce injury of surfaces was called to the attention of the writer, some years ago, by a well-known railroad superintendent, who sent him samples of staybolt rods whose threads had been cut, one with lard, and the other with black oil. The first was as smooth as could be desired; the second was rough, ragged on the edges of the thread, and in places the thread was completely stripped. No difference had been made in cutting except in the choice of the oils.

The cost of steam power, which is usually the principal item included in the cost of the wasted work of friction, varies greatly with size and kind of engine, character of fuel, expenses of operation, and with all the items, such as insurance, repairs and depreciation, incidental to its use. A fair figure is, perhaps, for ordinary mill engines of moderate power, $0.02 per horse-power per hour, and double this amount is not unusual. Of this total, from 50 to 80 per cent. may be assumed to be, on the average, the cost of fuel. Adding the incidental costs, it may be considered a fair estimate, for such cases, to take the total charge at $0.03 per horse-power and per hour. As the values of all items of expense, in every case, in practice, will be determined by direct experiment* and by observation, it is unnecessary to enter into this division of the subject very minutely, when, as here, only illustration of the principles developed is proposed.

The actual cost of steam power in mills ranges from as low as $50 to as high as $100 per horse-power, according to circumstances. The latter figure represents the cost of the best modern machinery.

* "On the several Efficiencies of the Steam Engine," Trans. Am. Soc. Mech. Engrs., Vol. III., p. 245, Journal of the Franklin Institute, May, 1882, Art. XIII.

The interest on first cost may be assumed at 6 per cent., the appropriation for a sinking fund at $2\frac{1}{2}$ per cent., the working expenses at not far from 10 per cent., in good cases, and the cost for fuel, on the average, at about 20 per cent. of the cost of the plant. The total cost, thus calculated, will vary from, perhaps, $35 to nearly, or quite, $100 per annum, per horse-power. It has been taken, above, at $60, and about fifty per cent. of its own amount added for miscellaneous costs not included in the direct calculation.*

In illustration of the application of these principles, the following cases, which are examples of practice falling within the experience of the writer, may be given:

(1.) In a machine-shop using about 100 horse-power, of which one-half is supposed to be applied to the overcoming of the friction of lubricated surfaces of journals and their bearings, it is found that the cost of power is very nearly $100 per horse-power per annum, inclusive of all of the incidentals above mentioned. The average coefficient of friction is not far from 0.05; the oil used costs, on the average, $0.50 per gallon, consisting mainly of lard, and heavy mineral oils, and is supplied at the rate of 0.02 gallon per working hour, the working year consisting of 3,000 hours.

Then, if 50 horse-power should be used in the work of overcoming frictional resistances, the cost of power would be $5,000 per annum, or $1.67 per hour, which is represented, in the equations already given, by $b\,f_1$. Since f_1 is found to be 0.05, b is equal to $33.33\dot{3}$. The oil used being found to cost, in place on the journal, $0.50 per gallon, and to be used at the rate of 0.02 gallon per hour, the total cost of lubrication is $0.01 per hour. Hence we have (Eq. 4):

$$K_1 = k_1 q_1 + b f_1 = 0.01 + 1.67 = \$1.68 \quad . \quad . \quad . \quad . \quad \text{(A)}$$

or $5,040 per annum.

Should it be proposed to make a change of oil, using oil costing but $0.25 per gallon, and of which 0.03 gallon per hour will be demanded, and which will make the coefficient of friction 0.06, the cost of power will be increased one-fifth, and that of oil diminished one-fourth; the equation then reads:

$$K_2 = 0.25 \times 0.03 + 33.33\dot{3} \times 0.06 = \$2.0075 \quad . \quad . \quad \text{(B)}$$

equal to $6,022 per annum.

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* For details of such estimates, see the papers and reports of Messrs. J. C. Hoadley and C. E. Emery, especially on the Watuppa Reservoir Case, and estimates presented to the American Society of Civil Engineers.

The gain effected in cost of oil is one-fourth of one cent per hour; while the loss, in cost of wasted power, is $33.33\dot{3}$ cents per hour. In other words, a gain of \$7.50 per annum on the books of the purchasing agent, or proprietor, is to be charged against a loss, in the cost of running the establishment, of \$1,000. The net loss, is \$982.50 per annum.

Should it prove possible to adopt a system of oil-bath, or other method of free lubrication, so as to bring down the coefficient to 0.02, as is not at all unlikely to prove practicable, and assuming that four times as much oil, of the second quality, is used as in the last case, we shall have

$$K_2' = 0.03 + 0.66\dot{6} = \$0.696 \text{ per hour} \quad . \quad . \quad . \quad . \quad \text{(C)}$$

\$2,088 per year, producing a gain of two-thirds the total cost of lost work, as in the last case. This amounts to nearly \$4,000 per year, or, as compared with the present running expense, as given in the first case to nearly \$3,000. The annual cost of oil, in the three cases, amounts to \$30, \$22.50, and \$90, respectively, and it is at once seen that, in this example of application, the saving, actual or possible, to be effected by any bargain made in the oil market, is absolutely insignificant in comparison with that to be produced in the shop by careful lubrication. A system of collection and purification of the oii running off the journals into the drip-pans may, in nearly all cases, be easily adopted, which will at once reduce the cost of lubricant, and make its first cost a matter of still less consequence.

Finally, suppose a grease used in this shop, such as now costs 25 cents per pound, and assume that it is given as a sample, costing the proprietor nothing, but bringing up the coefficient of friction, as an average, to 0.10: The cost of power is now the total expense, and this becomes

$$\$3.33\dot{3} \text{ per hour} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(D)}$$

or \$10,000 per annum; while the loss to the owners of the establishment, on their bargain, is \$5,000 per annum.

It will next be asked: What price represents the limit which may not be exceeded, without loss, in the purchase of the oils proposed to be substituted for that first used in this instance? This question is answered by the application of the criterion established by equations (10), and (14). Thus, comparing cases (A) and (B), we have

$$k_2 = \frac{k_1 q_1 - b\,(f_2 - f_1)}{q_2} = \frac{0.01 - 0.33\dot{3}}{0.03} = -\$10.78.$$

The second oil causes a loss of \$10.78 for every gallon used, and, hence, cannot be used without loss, unless the user is paid that sum to take it and apply it to his machinery.

Comparing cases (A) and (C), using equation (10),

$$k_2 = \frac{0.01 + 1.00}{1.2} = \$0.84\,;$$

and it is found that the second disposition of the poorer grade of oil is of such advantage that it is as well worth \$0.84 per gallon as is the better oil worth \$0.50, used as at first proposed, and as is customary. But it would be a still better investment, in all probability, to purchase the better oil, and to use as in the case compared.

Comparing cases (A) and (D), using equal amounts, per gallon,

$$k_2 = \frac{0.00 - 33.33\dot{3} \times 0.05}{0.02} = -\,\$83.44\,;$$

and the heavier lubricant is found to subject the user to an expense amounting to over \$10 for each pound used. It must not, however, be from this inferred that it is always wasteful to use the greases. They are often advantageous, where exceptional pressures are used or troublesome bearings are met with, and are sometimes absolutely indispensable, saving large amounts by their reduction of expenses in the cooling and preservation of journals and renewal of bearings. In the above case, it is probable that a much smaller quantity of grease than of oil would have sufficed, which would have reduced the total cost of grease, if purchased, but would have proportionally increased the loss to the proprietor, both absolutely and as reckoned per pound of unguent applied.

(2.) As a second illustration, assume a cotton mill to use a good oil, averaging \$0.70 per gallon, at the rate of 0.7 gallon per hour, with a mean coefficient of friction 0.10, on machinery demanding 400 horse-power, of which 120 horse-power is required to overcome the friction of surfaces lubricated by the oil. Taking the value of the power at \$65 per horse power per annum, and 3,000 working hours, we have $b = \$26$. If it is proposed to substitute for the oils used in this mill others averaging a cost of \$0.40 per gallon, giving a mean coefficient of friction of $f = 0.12$, and of which one gallon will be used per hour, we shall have

$$K_1 = 0.49 + 2.60 = \$3.09,$$
$$K_2 = 0.40 + 3.12 = \$3.52,$$

and a gain of 9 cents per hour, or \$270 per annum, in buying oil,

is to be set against a loss of 52 cents per hour, or \$1,560 per year, in increased expenses on the account of operating the mill, the net loss amounting to above one thousand dollars a year. Had the coefficient of friction been increased to a greater extent, the loss would have been correspondingly greater. The differences among the lubricants sold for mill purposes in the market are sometimes enormously greater than assumed above, and a loss of \$10 per horse-power, annually, is probably not an unknown case, and this is equivalent to about double that sum per horse-power expended on the friction simply.

Applying the criterion to this case, we have

$$k_2 = \frac{0.49 - 0.52}{1.0} = -\$0.03,$$

as the loss on each gallon of the second lubricant. The owner of the mill cannot afford to accept it, in substitution for the better oil, as a gift. The substitution of an engine oil, on the spindles, for the best spindle oil, might readily double the expenditure of power absorbed by the spinning machinery, and thus increase the cost of both lubrication and power, the former having both a higher coefficient of friction and greater price than the latter.

(3.) In further illustration, assume a railway train to be supplied with a good standard lubricating oil for engine and axles, costing, on the journal, \$0.25 per gallon, and to use 0.02 gallon per train-mile, the coefficient of friction, when everything is in good order and all journals cool, being 0.01. Taking as a fair figure, \$0.10 per mile for costs of power and incidentals variable with power, and presuming that, under the circumstances, wear may be reduced to an unimportant amount, and may be neglected, the relative costs of lubricating material and of power may be introduced into equations (18) and (19), as in the above examples. We thus obtain

$$K_1 = k_1 q_1 + d f_1 = 0.005 + 0.10 = \$0.10\tfrac{1}{2},$$

as the total money loss due to the existence of friction.

If it be proposed to substitute for the oil in use a cheaper oil, costing, on the journal, \$0.15, and of which fifty per cent. more will be used, and which will give a coefficient of friction 0.015—a not uncommon case—the total cost becomes

$$K_2 = 0.15 \times 0.03 + 10 \times 0.015 = \$0.15\tfrac{1}{2};$$

and it is found that a gain of one-twentieth of a cent, per mile, in cost of oil, is met by a loss of one hundred times as much, or five

cents per mile, in cost of power. Should the second oil give increased wear, its cost must be added to the account of losses produced by the change. Had the second oil been used in the same quantity as the first, one and a half cents per mile would have been saved, over the last figures, and the loss would be then $3\frac{1}{2}$ cents per mile.

To determine what could be paid for the second oil, as used, in order that no loss should take place in consequence of the change, equation (17) is to be used, and this gives

$$k_2 = \frac{0.005 - 10 \times 0.0005}{0.03} = \$0.00;$$

that is to say, the real value of the oil to the consumer is just 0, if the oil at first used was worth 25 cents per gallon. In many instances, in every-day practice, losses occur many times as great as those just estimated.

The conclusions to be drawn from the principles and the theory which have been presented in this paper, and from the examples of application to practice which have been introduced as fairly representing their use in various departments of engineering, are obvious and definite:

(1.) To secure the highest possible efficiency of machinery, and maximum economy in the operation of establishments in which it is employed, lubricants must be very carefully selected with reference to the precise conditions, as to pressure, velocity of rubbing, etc., met with in the individual case.

Where, as in machine shops and mills, for example, there exist great differences in these respects, it will be found advantageous to use different oils, as heavy oils on the engine bearings, special "cylinder oils" in the steam cylinder, lighter oils on the shafting, and the lightest of the better classes of lubricating oils on light machinery, as on spindles.

(2.) Differences in price of oils, or other lubricants, are usually of exceedingly slight importance in comparison with differences in costs of power; and the value of the coefficient of friction is, therefore, of vastly greater consequence than either the price of the unguent or its endurance.

(3.) The best oils for specified purposes should be taken, as a rule, whatever their market price; while the oils which are not well adapted to the purpose in view cannot be economically purchased at any price.

It will often be found that the best quality of oil is not necessarily the best oil for any one specified purpose. An oil may be intrinsically excellent, and may be a very expensive oil, but may, nevertheless, be absolutely worthless for the purpose in view. A good engine oil would, for example, be quite unfitted for use as a spindle oil, and though several times as high in price, might be the cause of such considerable waste of power on light mill machinery that the mill owner, as has already been seen, might find it to his interest to decline using it, even if it were offered him as a gift. The heavy oils are the most costly, and, in this case, the better oil is therefore also the cheaper in the market.

(4.) The cost of using a lubricant which is not well adapted to the work is so great that unguents should always be tested, and their adaptability to the special case determined, by a correct system of chemical and physical tests, and by trial upon a good testing-machine, if possible, under the exact conditions of the intended use.

The determination of the quality of any lubricant is an easy task; but the identification of the real conditions of use, as proposed, may sometimes be difficult. The difficulty arises, however, not from faults of method of test or uncertainty of results, but from defects of design or construction, or sometimes of management, of the machinery upon which it is proposed to use the oil. Where journals are kept in good order, and are properly proportioned, no difficulty need ever arise in the attempt to find the best possible lubricant for them. As a rule, there is no excuse for a condition of machinery which gives rise to such uncertainties. As a rule, in all successfully conducted departments of business, such uncertainties do not exist; they do not arise with sufficient frequency to invalidate the above rules. Testing-machines are now made in sufficient variety of form and of ample range of application, and of such satisfactory accuracy, that there is no longer necessity of accepting the risks, and of meeting the enormous expense involved in the application of lubricants of unknown quality to valuable machinery.

(5.) Where lubricants of the precise quality desired are not found in the market, it is advisable to secure the right grade by mixing. This can always be done by making a series of mixtures of good oils, such that, at the one side, the gravity and other qualities shall be too high, and, on the other side, too low, for the special application had in view, and thus working out—after determining by trial the law of variation—the mixture most perfectly suited to the purpose. The writer has often been called upon thus to determine

the best of a series of mixtures for a cylinder oil, for example, or for an engine or a spindle oil. By this method he has sometimes improved the quality of an oil for a special kind of work more than one hundred per cent. Satisfactory results can almost invariably be attained by careful and skillful work.

(This paper received discussion jointly with that of Mr. J. T. Henthorn, which follows it, entitled The Power Required to Overcome the Frictional Resistances of Engine and Shafting in Mills, and its Cost.)

CLXXIV.

ON THE POWER REQUIRED TO OVERCOME THE FRICTIONAL RESISTANCES OF ENGINE AND SHAFTING IN MILLS, AND ITS COST.

BY JOHN T. HENTHORN, PROVIDENCE, R. I.

As a matter of interest bearing upon the question of the frictional resistance of engine and shafting in mills, the writer has been prompted to enlarge and revise some material which has been previously published,* and to present it to the Society in the hope that it may lead to an investigation and discussion of this subject which forms such an important factor in the running expenses of our mills.

This subject has not, perhaps, received that due attention from those most directly interested which it demands as a matter of economy in the proportion of parts for the work to be done in the first instance, in the distribution of power to the various machines in the most economical methods, in the exercise of diligent oversight in its adjustment and alignment, and in the selection of proper lubricants. All of these factors have a direct influence upon the cost of running, and may be increased or decreased, according to the judgment used in the designing and subsequent care bestowed in its management. In the past few years a fashion has been inaugurated to run very light shafting at high velocities, say 350 to 400 revolutions per minute, for the main lines, and in many cases higher. This is very well where provision is made to take the thrust of the heavy belts by placing a hanger near the pulley—a plan which is made feasible by introducing stringers running parallel with the shaft. But when applied to the ordinary construction of mills where the pulleys are liable to run under tight belts and to be placed some distance from bearings with bays from 8 feet to 9 feet centers, it would seem that the advantages of less revolving weight are more than offset by the increased cost of running caused by the extra friction from shafts springing while doing their work.

It is true that self-adjusting boxes in the hangers which are now so universally used in the equipment of mills, alleviate in a meas-

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* *Cotton, Wool and Iron*, issue of Jan. 19, 1884.

ure the trouble which would be experienced if boxes having fixed bearings were employed. In fact, this adjustability contributes in a large measure to the running of light shafting with any degree of success. But still, with all this, there exists a defect which cannot be overcome unless the shafting is stiff enough to withstand, without deflection, the strain of the set screws in the pulleys, and to sustain the weight and tension of belts.

If light shafting is to be employed, special provision is necessary to keep it in place so as not to be deflected by belts, by providing bearings where they will sustain the thrust. One may be found necessary near each pulley.

This matter of running belts as tight as a fiddle-string seems to the writer to be entirely uncalled for, as it only results in extra pressure upon bearings and a shorter lifetime of belt. It is, however, hard to avoid it in the management of any mill. In some cases, as a result of defective design, it is a necessity in order to do the work, while in others it is merely the result of a lack of judgment on the part of those who do the repairs. From whatever the cause, the result is the same upon the bearings, and if light shafting is employed, upon that also to a marked degree.

This state of things suggests the importance of providing ample light single belts to do the work, secured under a light tension, revolving upon pulleys of as large diameter as the case will admit, so as to make the velocity of the belt the predominating factor in its transmitting qualities, rather than its tension. These pulleys may be of as light weight as would be consistent with the work to be done. And finally there should be provided a shaft of sufficient diameter to insure its running practically in line, without being deflected by the load applied, either from weight of pulley or tension of belt. The writer has known cases where couplings were more to blame in causing a shaft to run out of true than any other cause, and all such cases have their effect upon the cost of running. It is therefore not safe to assume an increased cost as due to any one particular cause without a full consideration of *all* facts.

To carry out this plan of shafting will no doubt result in a slight increase of first cost for the shafting and pulleys, but after the plant is once running the friction including the engine for a Print Mill should not exceed 19 per cent. of the full power.

Referring to the table annexed upon that subject, it will be seen that out of the fifty-five examples of a miscellaneous character cited, there are but 7 below 20% ; 20 from 20% to 25% ; 15 from 25% to

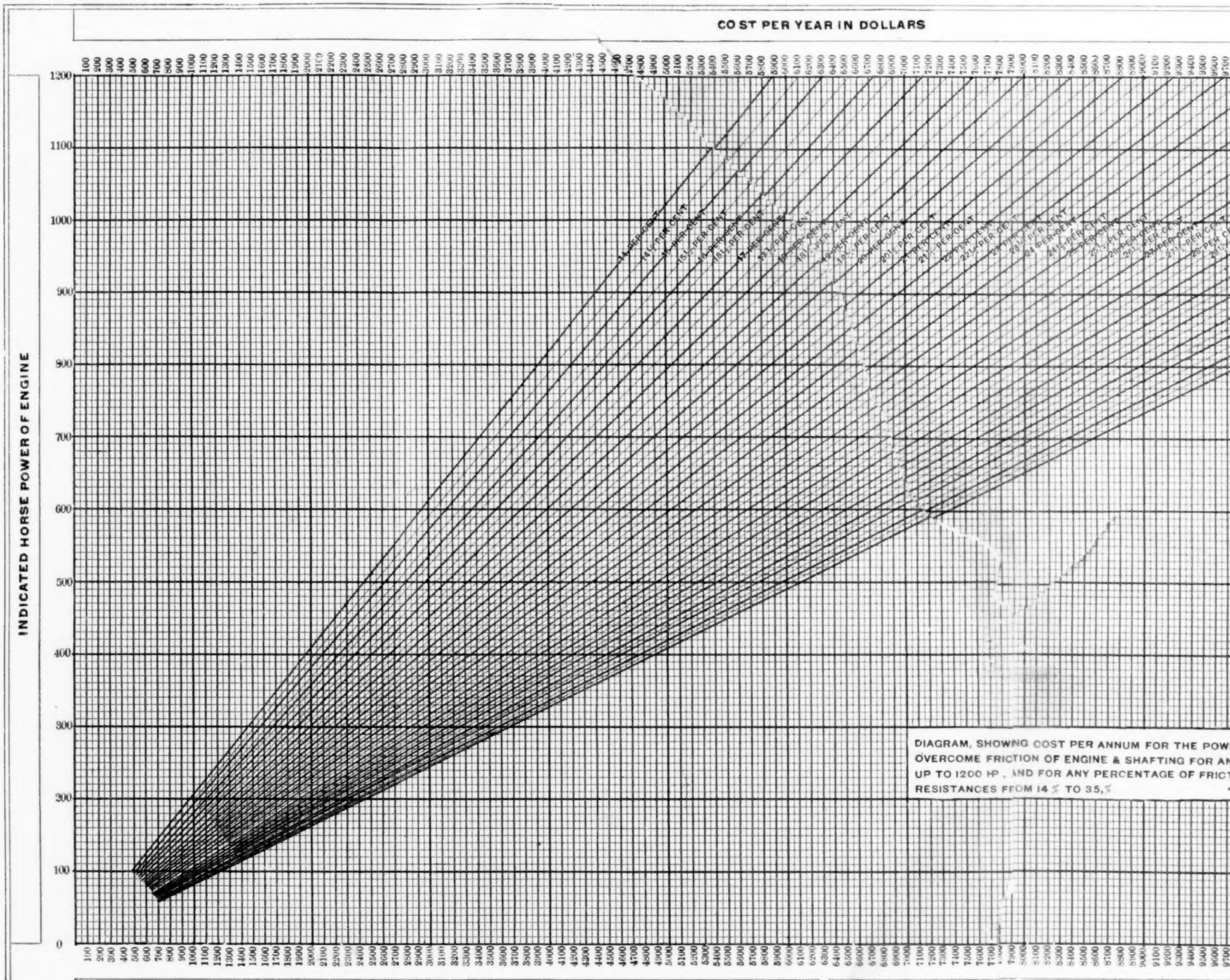
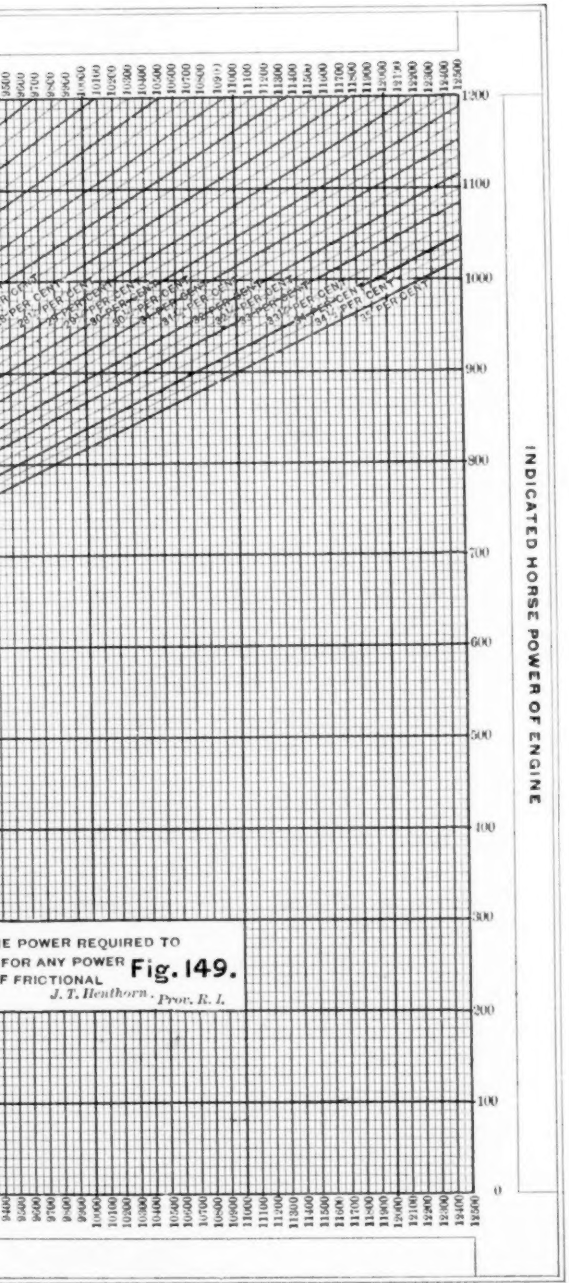

COST PER YEAR IN DOLLARS
INDICATED HORSE POWER OF ENGINE
DIAGRAM, SHOWING COST PER ANNUM FOR THE POWE
OVERCOME FRICTION OF ENGINE & SHAFTING FOR AN
UP TO 1200 HP, AND FOR ANY PERCENTAGE OF FRICT
RESISTANCES FROM 14 % TO 35 %
COST PER YEAR IN DOLLARS.

Fig. 149.



E POWER REQUIRED TO
FOR ANY POWER
F FRICTIONAL
Fig. 149.
J. T. Henthorn. Prov. R. I.

30%; 11 from 30% to 35%, and 2 above 35%; while the average of the whole number 55 is 25.9%.

In machine shops, where there is probably more shafting in proportion to the work done than in any other branch of manufacturing, the percentage of power required to overcome friction will exceed any given in the table, ranging from 45 to 50%. This is brought about by the excessive number of counters employed for driving tools of very light power while in operation.

As would naturally be suspected, the smaller the mill, the more power is required to drive engine and shafting per unit of power of the full load from the fact that a less advantageous condition of things exists regarding the proportion and general arrangement of the shafting, many factors of which remain a constant even through quite a range and increase of power. Therefore the same degree of economy may not be expected in mills of small capacity which it is reasonable to look for in larger ones.

To illustrate the cost for power, for running the engine and shafting, to overcome friction, the writer has prepared the accompanying diagram (Fig. 149), in which the diagonal lines represent the different percentages of the full indicated power required to overcome the frictional resistance of engine and shafting. The horizontal lines represent the indicated horse-power that the engine is developing when on its regular duty, and the ordinates show the cost in dollars per annum for those elements, based upon $35 per annum as the minimum cost value of a horse-power.

To use this diagram, supposing it be found that 25 per cent. of the full power is required for driving any engine and shafting and the full load is 860 horse-power; by following the diagonal line representing the 25 per cent. friction until it comes to the horizontal line representing 860 horse-power, the ordinate at this intersection is found to be $7,550 per annum, as the cost of overcoming the frictional resistances. Now as a comparison, by following the same line of reasoning, and assuming, as has been before stated, that it is entirely practical to run a Print Mill under proper conditions with an expenditure of 19 per cent. of the full power for friction, it will be found that for this power 860 horse-power and 19 per cent. we have $5,730, or a difference of $1,820, which is certainly worth considering, and it would pay in such a case to employ a man to devote a portion of his time to keep posted as to the running condition of the shafting, and to hold him responsible for the care of this part of our machinery.

TABLE I.

Table showing No. of spindles per loom, indicated horse-power required per 1,000 spindles, with all preparative machinery, and the per centum of full load required to overcome friction of engine and shafting in various mills.

No. of Mill.	Date.	Goods Manufactured.		Engine.		No. of mule spindles.	No. of frame spindles.	Total No. of spindles.	No. of looms.	No. of spindles per loom.	H. P. per loom with all preparative machinery	H. P. per 1,000 spindles with all preparative machinery.	H. P. required for engine and machinery or full load.	H. P. required for engine and shafting.	Per centum of full load required for engine and shafting.
		Material.	Style.	Single or pair.	Size.										
1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	14.	15.	16
1	Ap'l, 1873.	Cotton.	Prints.	Pair.	23″ × 60″	35,928	......	35,928	784	45.82	.540	11.7	423.6	70.25	16.5
2	Ap'l, 1873.	Woolen.	Heavy.	Single.	26″ × 48″	......	......	......	......	......	......	......	225.6	82.9	16.7
3	May, 1883.	....	Electric Lighting.	Single.	18″ × 48″	......	......	......	......	......	......	......	130.7	22.4	17.1
4	June, 1876.	Cotton.	Braiding, etc.	Single.	30″ × 60″	......	......	......	......	......	......	......	298.7	52.4	17.5
5	Feb., 1883.	Cotton.	Shirting.	Pair.	36″ × 72″	27,648	23,520	51,168	1,072	47.73	.878	18.3	941.5	173.5	18.4
6	Jan., 1879.	Cotton.	Prints.	Pair.	30″ × 72″	53,712	......	53,712	1,300	41.31	.719	17.4	935.5	177.6	18.9
3	Dec., 1883.	....	Electric Lighting.	Single.	18″ × 48″	......	......	......	......	......	......	......	189.7	37.0	19.5
7	May, 1873.	Iron.	....	Single.	32″ × 72″	......	......	......	......	......	......	......	150.5	31.2	20.7
8	Jan., 1874.	....	....	Pair.	18″ × 48″	9,984	7,232	17,216	342	50.33	.821	16.3	281.1	58.3	20.7
9	Mar., 1869.	Cotton.	Prints.	Pair.	30″ × 48″	......	......	......	......	......	......	......	406.5	86.8	21.3
10	Mar., 1871.	Cotton.	Prints.	Pair.	34″ × 60″	54,784	......	54,784	1,242	44.10	.611	13.8	759.0	163.1	21.4
11	May, 1872.	....	....	Single.	12″ × 24″	......	......	......	......	......	......	......	46.7	10.2	21.9
12	Dec., 1883.	Cotton.	Prints.	Pair.	32″ × 72″	34,080	20,128	54,208	1,050	51.62	.923	17.8	969.6	214.3	22.1
13	Mar., 1881.	Cotton.	Fine.	Single.	34″ × 72″	22,264	10,752	33,016	576	57.31	.799	13.9	460.3	106.0	23.0
7	May, 1873.	Iron.	....	Single.	32″ × 72″	......	......	......	......	......	......	......	221.2	51.3	23.1
14	Aug., 1877.	Cotton.	Prints.	Pair.	28″ × 60″	......	26,656	26,656	800	33.32	.725	21.7	580.7	134.4	23.1
15	Nov., 1873.	Cotton.	....	Pair.	23″ × 60″	......	......	......	......	......	......	......	558.0	129.8	23.2
16	Jan., 1869.	Cotton.	Prints.	Pair.	32″ × 54″	......	......	......	......	......	......	......	530.5	124.3	23.4
17	Mar., 1873.	Cotton.	Shirtings.	Single.	26″ × 48″	6,912	6,400	13,312	284	46.87	.924	19.7	262.5	60.4	23.6
17	Ap'l, 1869.	Cotton.	Shirtings.	Single.	26″ × 48″	6,912	6,400	13,312	284	46.87	.798	17.0	226.8	53.9	23.7
18	Feb., 1883.	Cotton.	Heavy Sheeting.	Pair.	44″ × 120″	52,472	42,688	95,160	2,156	44.13	.688	15.6	1,485.2	352.2	23.7
9	Mar. 1871	Cotton.	Prints.	Pair.	30″ × 48″	35,328	......	35,328	840	42.05	.457	10.8	384.0	91.8	23.9

19	Ap'l, 1885.	Worsted.	Light.	Single.	23″ × 48″	....	....	....	....	....	....	....	260.6	63.9	24.5
20	Jan. 1885.	Cotton.	....	Single.	20″ × 48″	....	....	....	....	....	....	....	193.7	47.5	24.5
21	May, 1877.	Worsted.	Heavy.	Single.	23″ × 48″	....	....	....	....	....	....	....	204.8	50.4	24.6
22	May, 1876.	Cotton.	....	Single.	20″ × 60″	....	....	....	....	....	....	....	196.6	48.6	24.7
23	Dec., 1873.	Cotton.	Prints.	Pair.	28″ × 60″	53,520	19,008	42,528	1,008	42.19	.566	13.4	570.7	142.6	24.9
24	Mar., 1873.	Cotton.	Light Sheeting.	Pair.	18″ × 48″	10,726	10,944	21,664	452	47.92	.700	14.6	316.8	80.4	25.3
17	Ap'l, 1871.	Cotton.	Shirtings.	Single.	26″ × 48″	6,912	6,400	13,312	284	46.87	933	19.9	265 0	68.0	25.6
25	Nov., 1878.	Cotton.	Heavy.	Pair.	28″ × 60″	20,224	17,584	37,808	806	46.90	.753	16.0	607.3	155.6	25.6
26	Aug., 1883.	Worsted.	Fine.	Pair.	30″ × 72″	....	....	....	....	....	....	....	1,033.7	265.5	25.6
27	June, 1873.	Cotton.	Heavy Goods.	....	23″ × 60″	11,808	8,384	20,192	313	64.51	.849	13.1	266.0	70.3	26.4
28	May, 1877.	Cotton.	Prints.	Pair.	20″ × 60″	33,480	1,440	34,920	706	49.46	.669	13.5	472.4	111.5	26.6
29	Mar., 1871.	Cotton.	....	Single.	16″ × 42″	....	....	....	....	....	....	....	78.5	21.0	26.7
30	Ap'l, 1868.	Cotton.	....	Pair.	26″ × 48″	....	....	....	....	....	....	....	608.1	163.3	26.8
31	Aug., 1883.	Worsted.	Heavy.	Single.	30″ × 60″	....	....	....	....	....	....	....	526.4	145.7	27.6
32	Nov., 1867.	Cotton.	....	Single.	20″ × 48″	....	....	....	....	....	....	....	136.9	38.8	28.3
33	July, 1878.	Cotton.	....	Single.	20″ × 48″	....	....	....	....	....	....	....	241.6	70.1	29.0
31	1880.	Worsted.	Heavy.	Single.	30″ × 60″	....	....	....	....	....	....	....	421.2	122.4	29.0
34	....	Cotton.	....	....	....	....	....	....	....	....	....	....	142.0	41.4	29.1
35	Feb., 1879.	Cotton.	Twine.	Single.	23″ × 42″	....	....	....	....	....	....	....	121.5	35 8	29.4
36	May, 1881.	Cotton.	Prints and Fancys.	Pair.	28″ × 60″	22,680	18,944	41,624	964	43.17	.655	15.1	632.3	189.1	29.9
27	July, 1873.	Cotton.	....	Single.	23″ × 60″	....	....	....	....	....	....	....	222.7	66.9	30.0
37	Ap'l, 1872.	....	....	Single.	14″ × 42″	....	....	....	....	....	....	....	67.9	22.5	30.0
38	....	Woolen.	....	Single.	14″ × 48″	....	....	....	....	....	....	....	77.8	23.5	30.2
39	Oct., 1867.	Cotton.	....	Pair.	26″ × 48″	....	....	....	....	....	....	....	163.7	51.0	31.1
40	Mar., 1879.	Woolen.	Heavy.	Single.	26″ × 60″	....	....	....	....	....	....	....	220.2	69.3	31.4
41	May, 1876.	Cotton.	Prints.	Single.	16″ × 42″	8,000	....	8,000	124	64.51	.898	13.9	111.4	35.5	31.8
42	Jan., 1869.	Cotton.	....	Single.	34″ × 60″	....	....	....	....	....	....	....	284.7	113.9	31.8
43	Mar., 1867.	Cotton.	....	Single.	23″ × 48″	....	....	....	....	....	....	....	142.8	46.5	32.5
44	Jan., 1874.	Cotton.	....	Single.	34″ × 72″	....	....	....	....	....	....	....	299.4	99.0	33.0
45	Ap'l, 1873.	Cotton.	Light Sheeting.	Pair.	28″ × 60″	....	....	....	....	....	....	....	466.9	155.7	33.3
46	Jan., 1878.	Cotton.	Heavy.	Single.	20″ × 60″	7,896	8,696	16,592	384	43.20	.767	17.7	294.6	102.1	34.6
27	Aug., 1872.	Cotton.	....	....	....	....	....	....	....	....	....	....	241.9	93.6	38.6
47	Feb., 1867.	Cotton.	....	Single.	20″ × 48″	....	....	....	....	....	....	....	104.2	40.9	39.2
45	July, 1876.	Cotton.	Light Sheeting.	Pair.	28″ × 60″	22,752	21,120	43,872	1,000	43.87	.699	15.9	699.6	....	....
24	Jan., 1877.	Cotton.	Light Sheeting.	Pair.	20″ × 60″	10,720	10,944	21,664	452	47.92	.734	15.3	331.9	....	....
24	May, 1877.	Cotton.	Light Sheeting.	Pair.	20″ × 60″	10,720	10,944	21,664	452	47.92	.691	14.4	312.5	....	....
48	Ap'l, 1879.	Cotton.	Prints.	Pair.	20″ × 48″	8,544	6,480	15,024	334	44.98	.879	19.5	293.7	....	....
49	Sept., 1883.	Cotton.	Heavy Sheeting.	Pair.	30″ × 72″	34,152	11,568	45,720	979	46.70	.852	18.2	834.1	....	....
50	Dec., 1883.	Cotton.	Prints.	Pair.	{28″ & 30″ × 48″	20,736	14,736	35,472	724	48.99	.835	17.0	605.2	....	....
														Av'ge.	25.9

In the equipment of mills, engineers should above all things avoid any desire to economize in the strength of the first movers or jack shafts, where large belts are to be taken care of.

Many cases come up where the shafts have been put in and have failed from their springing while doing their work, especially when the pulley is central between the bays. Such failures invariably show at their fracture a portion of the section as smooth as though finished with a tool, where the process of disintegration working in from the surface gradually wore away the fiber of the iron as the defect developed; while the remaining portion would clearly show the changes which the iron had undergone by crystallization from the vibration to which it had been subjected.

An instance may be cited, showing the effect of a continued vibration, where a piece of first-class hammered iron 11″ diameter, was used for a time as a porter-bar in building up shafts, during the process of forging under a steam hammer. As the forging was suspended in the crane, each blow of the hammer caused a vibration throughout the whole piece, which finally resulted in the dropping off of a portion of the porter-bar behind the crane chain, showing a fracture of a clear, but decidedly of a crystalline nature.

In connection with this subject is appended a diagram which has been prepared by the writer and which he has found very convenient for reference, to show at a glance the size of shaft required for a given number of horse-power when running at a certain speed. (Fig. 150.)

This diagram was prepared from the results of observation regarding the transmitting powers of good hammered iron shafts of known diameter and speeds, ranging in their limits from the maximum and minimum powers and speeds as given in diagram. From this it was possible to determine that for a good hammered iron shaft it is perfectly safe to use the following formula for designing work where D=Diameter of shaft in inches of good hammered iron.

R=Revolutions of shaft per minute.

HP=horse-power transmitted.

$$\sqrt[3]{\frac{56 \times \mathrm{HP}}{R}} = D \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad 1.$$

$$\frac{D^3 \times R}{56} = \mathrm{HP} \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad 2.$$

$$\frac{56 \times \mathrm{HP}}{D^3} = R \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad 3.$$

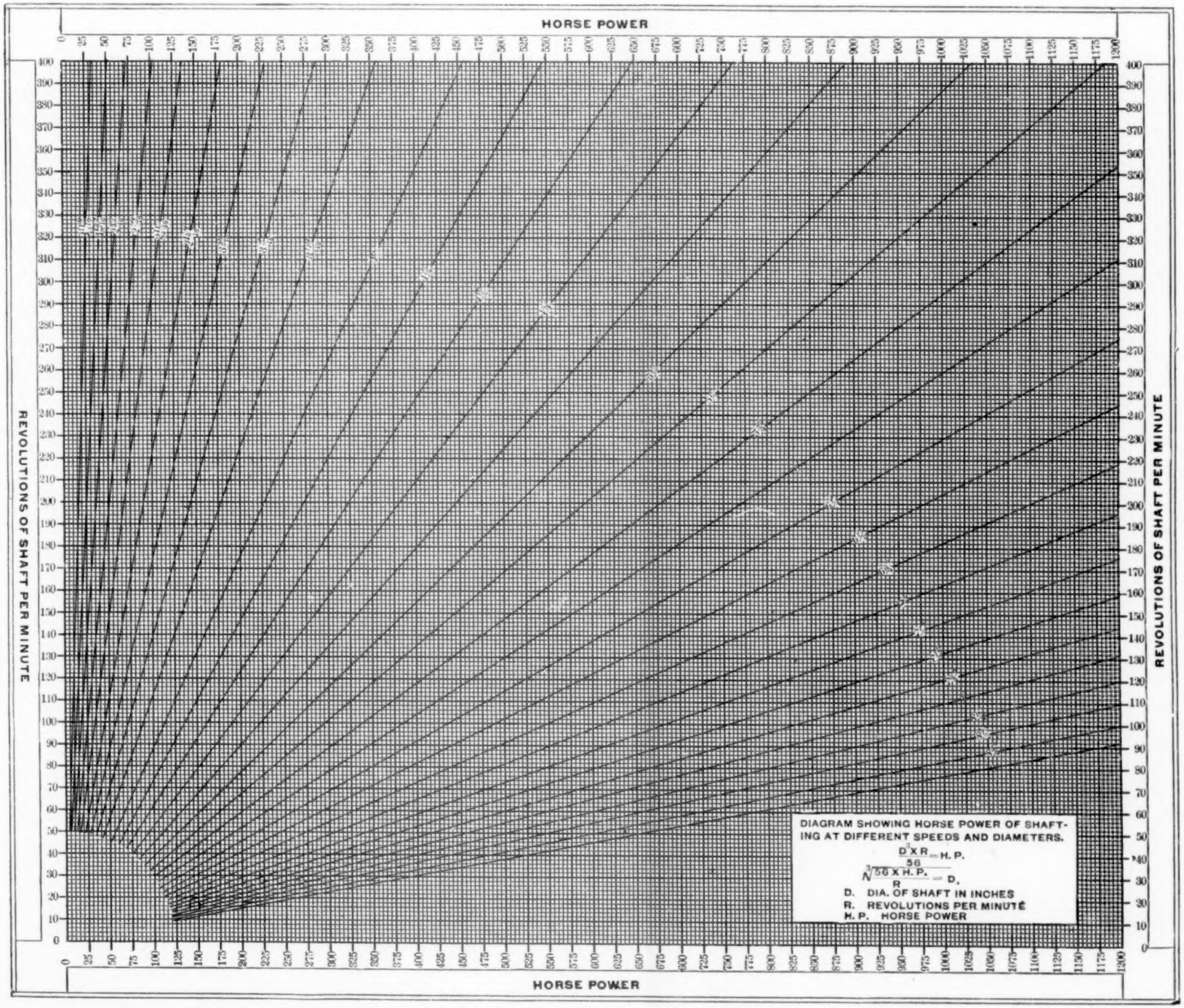

HORSE POWER
REVOLUTIONS OF SHAFT PER MINUTE
DIAGRAM SHOWING HORSE POWER OF SHAFTING AT DIFFERENT SPEEDS AND DIAMETERS.
$\frac{D^3 \times R}{56} = H.P.$
$\sqrt[3]{\frac{56 \times H.P.}{R}} = D.$
D. DIA. OF SHAFT IN INCHES
R. REVOLUTIONS PER MINUTE
H. P. HORSE POWER
REVOLUTIONS OF SHAFT PER MINUTE
HORSE POWER

FIG. 150.

To use this diagram, supposing that it is desired to drive 900 horse-power at 304 revolutions per minute; following the horizontal line representing revolutions per minute and the ordinate, for power, it is found that the nearest diagonal line is one representing $5\frac{1}{2}''$ diameter. Or supposing there is a shaft $7\frac{1}{4}''$ in diameter and its speed is 170 revolutions per minute, and it is required to find the horse-power which it will safely transmit. Running along on the horizontal line of 170 revolutions per minute until the diagonal line is reached, representing $7\frac{1}{4}''$ diameter, at this intersection an ordinate is found representing the horse-power which in this case is 1,155.

The question naturally arises, what proportion of this frictional resistance is due to working the engine itself.

It is one that is governed by size, style, general proportion of several details, and arrangement of parts, and in a measure, it may be said, by speed. This question can only be answered in a general way by citing a few cases of a particular class that have come up when running under certain conditions. Experiments were made with a 14″ by 60″ engine, running without belt and having a belt-wheel 16 feet in diameter and 21″ face, weighing about 18,000 lbs., while running at different speeds up to 62 revolutions per minute. This engine had been recently furnished with a new cylinder, and would consequently require a little more power to overcome friction than one would expect had it been in operation for a time. The results are shown on diagram Fig. 150 A by the heavy full line *A*. The continuous light line *B* shows an assumed increase in proportion to speed, and the dotted line *C* an assumed increase in proportion to that required for the maximum and minimum speeds. The average of the average pressures in cylinder was 2.3 lbs., corresponding to 7 per cent. of the rated power of the engine.

For a 30″ by 72″ engine running 52 revolutions per minute, and a pulley 25 feet in diameter and 64″ face, weighing about 56,000 lbs., with two belts, and corresponding jack pulley about 8 feet in diameter and a short piece of jack-shaft about 11 feet long working in two bearings, the average pressure was found to be 2.2 lbs. per square inch of piston corresponding to 6.6 per cent. of the rated power of the engine. In a class of high-duty pumping engines the friction has been found to vary from 8 to 9 per cent. of the indicated horse-power for good examples. This not only includes the friction of engine, but also that of pumps and water entering and passing through the valves to the delivery main.

Upon a consideration of the matter, the writer believes that $5\frac{3}{4}$ per cent. of the rated power fairly represents the amount necessary to drive the engine unloaded, and an additional $1\frac{1}{4}$ per cent. as the amount due to the load, making 7 per cent. as the amount to be de-

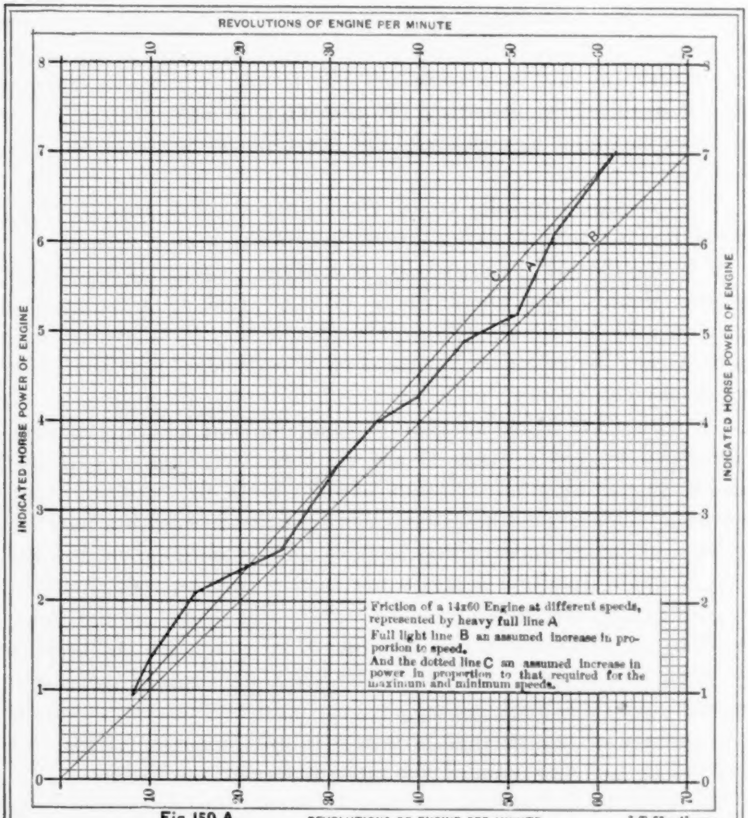


Fig. 150 A.

ducted from the total frictional resistances, to arrive at that required for shafting only.

To determine the quantity and cost of oil used in the engine-room for general lubrication of valve gear, pins, main bearings, etc., and also the amount used to lubricate cylinders of engines, inquiries were made among those interested with a result given in Table II. From this it will be seen that the total quantity of oil used for all purposes in the engine-room, per indicated horse-power, for a year of 309 days of 10 hours each, determined from the total

cost, column 20 and power column 6, is 42.4 cents, while the cost for general lubrication about engine-room (excepting cylinder) per indicated horse-power determined from average of cases given is 22.5 cents. For the cylinder lubrication the average cost per indicated horse-power is 23.9 cents, and 1.37 cents per square foot of cylinder surface traversed by piston during one minute of time, each item being for a year of 309 days of 10 hours each.

The mean of these two figures 22.5 and 23.9 is somewhat larger than that determined by the totals, from the fact of a smaller number of cases being used which have these facts in detail.

The probabilities are that one-half of the cost of lubricants is required for the engine cylinder and the remainder for general lubrication when run to the best advantage.

Touching the question of economy in maintaining this last item, the writer would say, by all means provide drip-pans under cranks, main bearings, eccentrics, slides, etc., into which all oil may be allowed to run freely through and from these parts. This excess should be collected and afterward strained through a series of three sieves, of three degrees of fineness, made in the shape of pans one fitting the other and having fine wire-gauze bottoms, upon which is placed a layer of cotton-waste which may be renewed occasionally.

There is no practical difficulty in carrying out this plan, as the oil may be used over and over again upon heavy main bearings in places where the temperature of the room is quite high (say 100° F.) without the slightest risk or inconvenience thereby, while the plan will materially reduce the running expenses for oil for this part of the machinery.

Drips from shafting in mill, may be treated in the same way to good advantage.

To arrive definitely at the quantity and cost of oil used in cotton mills for all purposes, including that for engine-room, letters of inquiry were addressed to a few leading manufacturers covering the following items:

Period of time covered by return in hours.

Total No. gallons oil used during that period for all purposes.

Actual cost of same.

Lbs. of manufactured product during period.

Class of goods.

No. of spindles in mill.

No. of looms in mill.

TABLE

TABLE SHOWING THE TOTAL COST FOR OIL USED IN ENGINE-ROOM FOR ALL

No. of Mill.	Size of Engine. Diameter of Cylinder in inches.	Size of Engine. Stroke in feet.	Single or Pair of Engines.	Rev. of Engine per minute.	Ind. horse-power developed.	Period covered by returns in hours.	Oil used for general lubrication. Gallons Oil used during period.	Cost per Gallon.	Cost for period given. Col. 8 × Col. 9.	Cost per year of 309 days of 10 hours each, or 3,090 hours. Col. 10 × 3,090. / Col. 7.	Cost per year of 309 days of 10 hours each per ind. horse-power. Col. 11 / Col. 6.	Cylinder surface traversed by piston exposed per minute. D × 3.1416 × 2 × S × R. D = dia. cyl. in feet. S = stroke in feet. R = rev. per min.
1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.
											Cents.	Sq. ft.
1	24″	6′	Compound.									4,296.20
	36″	6′	Single.	57.	500.	836	37.5	50 cts.	$ 18.75	$ 69.30	13.86	6,440.54
2	30″	6′	Pair.	48.	900.	3,282	244.	50 cts.	122.	114.86	12.76	9,047.80
	23″	5′	Single.	75.								4,640.25
3	28″	4′	Single.	100.	750.	1,700	156	40 cts.	62.40	113.42	15.12	5,857.60
4	24″	3′	Pair.	105.	700.	300	15	55 cts.	8.25	84.97	12.14	7,914.06
5	16″	4′	Single.	53.	60.	300	3	55 cts.	1.65	16.99	28.32	1,772.74
6	16″	4′	Single.	80.	120.	300	2	55 cts.	1.15	10.84	9.87	2,675.84
7	22″	4′	Single.	80.	300.	300	3	55 cts.	1.65	16.99	5.66	3,680.00
8	16″	3½	Single.	80.	80.	300	6	55 cts.	3.30	33.99	4.24	2,341.36
9	20″	4′	Pair.	80.	417.	300	12	55 cts.	6.60	67.98	16.30	6,693.12
10	16″	4′	Single.	80.	60.	300	4	55 cts.	2.20	22.66	37.76	2,675.84
11	12″	3′	Single.	60.	45.	300	1	55 cts.	.55	5.66	12.57	1,128.60
							24.5	$1.12				
12	23″	4′	Single.	83.	260.	3,503.	23.	40 cts.	36.64	82.32	12.43	4,108.16
13	14″	5′	Single.	62.	50.	165.	1	40 cts.	.40	7.49	14.98	2,272.92
14	26″	5′	Pair.	54.	711.	2,740.	Grease.					7,346.16
15	30″	6′	Pair.	52.5	913.	792	Grease.		139.14	542.88	59.46	9,896.04
16	32″	7′	Single.	37.	281.							4,338.25
	20⅜″	4′	Single.	60.	180.	792.	Grease.		80.06	335.76	72.83	2,509.92
17	28″	5′	Pair.	49⅜	698.	1,620.						
Totals and Average.					7025.						22.5	

Speed of main lines of shafting.

To these inquiries, 31 replies were received, which are incorporated in Table III. just as furnished. For each case the cost was determined for one uniform period of time, as a year of 309 days of 10 hours each; also per spindle in cents; and per loom in dollars; upon the above basis. The cost for oil per pound of manufactured product, in cents, was determined for the period covered by return only.

From a summation of this table is determined the following facts covering the returns from the different mills, all of which were on cotton goods.

NO. II.

PURPOSES, PER INDICATED HORSE-POWER PER YEAR OF 309 DAYS OF 10 HOURS EACH.

No. of Mill.	OIL FOR LUBRICATING ENGINE CYLINDER. Gallons Oil used during period.	Cost per Gallon.	Cost per period given. Col. 14 × Col. 15.	Cost per year of 309 days of 10 hours each, or 3,090 hours. Col. 16 × 3,090 / Col. 7.	Cost per year of 309 days of 10 hours each, per ind. horse-power. Col. 17. / Col. 6.	Cost per year of 309 days of 10 hours each, per sq. ft. cylinder surface traversed by piston exposed during one minute. Col. No. 17. / D × 3.1416 × S × 2 × R or Col. No. 17 / Col. No. 13 D=dia. cyl. in feet. S=stroke in ft. R=rev. per min.	Total cost for Oil, for all purposes, per year of 309 days of 10 hours each. Col. 17 × Col. 11.	Total cost per ind. horse-power per year of 309 days, of 10 hours each. Col. 20 / Col. 6.	REMARKS.
	14.	15.	16.	17.	18.	19.	20.	21.	
					Cents.	Cents.		Cents.	
1	44	65 cts.	$ 28.60	$105 71	21.16	.984	$175.01	35.00*	
2	175¼	65 cts.	113.91	107.24	11.91	1.185	222.10	24.67*	
3	162	85 cts.	137.70	250.29	33.37	2.384	363.71	48.49	
4	15	65 cts.	9.75	100.42	14.34	1.268	185.39	26.48	
5	2	65 cts.	1.30	13.39	22.31	.755	30.38	50.63	
6	4	65 cts.	2.60	26.78	22.31	1.008	37.62	31.35	
7	6	65 cts.	3.90	40.17	13.39	1.091	57.16	19.05	
8	4	65 cts.	2.60	26.78	33.47	1.143	60.77	75.96	
9	8	65 cts.	5.20	53.56	12.84	.800	121.54	29.14	
10	3	65 cts.	1.95	20.08	33.46	.750	42.74	71.23	
11	2	65 cts.	1.35	13.90	30.88	1.230	19.56	43.46	
12	104	65 cts.	67.60	50.63	22.93	1.451	91.95	35.36	
13	.875	75 cts.	.66	12.36	24.72	.543	19.85	39.70	
14	192.	93 cts.	178.56	201.36	28.32	2.741			
15	100	65 cts.	65.00	253.59	27.77	2.562	796.47	87.23†	
16	55	65 cts.	35.75	139.47	30.25	2.036	475.23	103.30	
17	145	75 cts.	also includes that used on cross head slides.				280.61	40.20*	
					23.9	1.37	2981.09	42.43	

Pounds of manufactured product during periods reported... 51,203,153
Cost of oil for all purposes for periods given...............$21,731.73
Cost of oil per POUND OF MANUFACTURED PRODUCT............ .04244 cents.
Total number of spindles in the mills..................... 1,723,804
Total number of looms in the mills........................ 43,353
Total cost for lubricants for all purposes, in the different mills
for a year of 309 days of 10 hours each...................$49,422.60
Cost per year of 309 days of 10 hours each PER SPINDLE.... 2.8670 cents.
Cost per year of 309 days of 10 hours each PER LOOM........$1.1400

* Oil from cranks, slides, main bearings, etc., strained and used over again.

† Includes oil for lubricating, jack shaft, 7″ dia., about 60′ long.

TABLE NO. III.

TABLE SHOWING COST OF LUBRICANTS USED FOR ALL PURPOSES IN DIFFERENT MILLS PER SPINDLE, LOOM AND POUND OF MANUFACTURED PRODUCT.

No. of Mill.	Period covered by return. Date.	Period covered by return. H'rs.	No. of lbs. of Manufactured Product during Period.	Class of Goods Manufactured.	Average No. of Yarn.	Speed of Main Lines of Shafting.	Total No. of Spindles.	Total No. of Looms.	Gallons Oil and lbs. of Grease used in whole Mill for all purposes, including Engine-room during period.	Average cost of lubricant per Gallon or Pound.	Total cost for Period Given in Dollars. Col. 10 × Col. 11.	Cost per year of 309 days of 10 hours each or for 3,090 hours in Dollars. $\frac{3,090 \times \text{Col. 12}}{\text{Col. 3.}}$	Cost per year of 309 days of 10 hours each or for 3,090 hours. Per Spindle in cents. $\frac{\text{Col. 13}}{\text{Col. 8.}}$	Cost per year of 309 days of 10 hours each or for 3,090 hours. Per Loom in Dollars. $\frac{\text{Col. 13}}{\text{Col. 9.}}$	Cost per pound of Manufactured Product in cts. $\frac{\text{Col. 12}}{\text{Col. 4.}}$
1	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	14.	15.	16.
										Cts.			Cents.		Cts.
1	....	1,716	705,401	....	....	....	46,600	1,015	25.10	57.7	$1,449.50	$2,610.11	5 6010	$2.5715	.2054
2	Sept. 27 to Dec. 27, 1884.	630	553,109	Print cloth	17.5	....	53,000	1,336	Oil 992 gal. Grease, 126 lbs.	25.6 15.	273.03	1,339.14	2.5266	1.0023	.0493
3	....	1,560	3,698,711	Sheetings	....	300 Rev.	88,000	1,805	3,250	21.	682.50	1,351.87	1.5362	.7489	.0184
4	For year 1884.	3,282	2,210,051	Sheetings and Silesias	....	310	53,824	1,238	3,783	46.3	1,754.02	1,651.49	3.0681	1.3339	.0793
5	July 1 to Dec. 31, 1884.	1,620	822,868	Twills and Sheetings	....	315	41,040	933	1,399	40.5	567.78	1,082.98	2.6388	1.1607	.0690
6	....	700	764,491	Print cloth	....	....	66,432	1,605	1,000	....	478.95	1,947.31	2.9312	1.2132	.0626
7	....	230	552,681	Print cloth	....	200	156 672	4,462	910	29.2	265.76	3,570.42	2.2789	.8001	.0480
8	....	1,500	1,175,316	Flannels and Sheetings	....	310	38,816	916	1,999	18.2	364.68	751.24	1.9353	.8201	.0310
9	For year 1883.	3,000	12,769,239	Sheetings, Shirtings and Drills	....	250 to 300	119,528	3,652	11,084	25.6	2,847.00	2,847.00	2.3818	.7795	.0222
10	4 weeks ending Mar. 28, 1885.	240	580,256	Sheetings, Shirtings and Fancys	....	....	110,000	2,850	600	30.	180.00	2,317.50	2.1068	.8131	.0310
11	....	66	20,575	Print cloth	....	320	26,624	630	61	15.1	9.25	433.06	1.6265	.6873	.0449
12	....	1,700	4,000,000	Sheetings, Drills, Bags and Duck	....	220 to 350	50,000	1,400	4,500	13	585.00	1,063.32	2.1266	.7595	.0146
13	Ending Nov. 29, 1884.	1,533	*2,251,026	Variety average 8⅛ yards per lb.	....	150 to 200	90,540	1,808	3,066	40.	1,227.63	2,474.47	2.7330	1.3686	.0545
14	....	240	2,00,000	Sheetings	28½ × 36	240 to 350	55,000	1,225	450	51.1	230.00	2,961.25	5.3840	2.4173	.1150
15	....	1,500	1,637,923	Wide Sheetings and Satteens	....	....	56,000	1,125	....	....	970.77	1,999.78	3.5710	1.7775	.0592
16	....	60	40,000	Print cloth	....	90 to 250	34,560	920	56	50.	28.00	1,442.00	4.1724	1.5673	.0700
17	....	1,510	1,500,000	Jeans, Shirtings and Drills			50,000	1,400	2,500	50.	750.00	1,534.76	2.0695	1.0962	.0500
18	Ending Aug. 2, 1884.	691	948,365	Lt. & H'vy Dr'ls, Sat'ens & Sh'etings	....	155 to 276	56,112	1,385	1,251 lbs. Grease. 1,292.5 gal. Oil.	....	566.70	2,534.15	4.5162	1.8297	.0597
18	Ending Jan. 30, 1885.	699	935,798	Lt. & H'vy Dr'ls, Sat'ens & Sh'etings	32½	290	56,112	1,385	1,707 lbs. grease. 1,134 gal. Oil.	....	570.56	2,522.21	4.4949	1.8210	.0609
19	....	300	60,339	Print cloth 64 × 64	....	270 to 406	16,608	350	251	23.1	58.00	597.40	3.5970	1.7068	.0961
20	....	1,540	2,576,700	Drills and Sheetings	32⅛	215	85,000	1,852	2,153	36.2	780.07	1,565.20	1.8414	.8451	.0302
21	....	300	181,053	Print cloth, 64 × 64	Wps. 29 Fill. 37	250	41,072	1,026	500	22.0	110.00	1,133.00	2.7585	1.1042	.0607
22	Oct. 1883, to Oct. 1884.	2,872	3,328,778	Print cloth	12.5	300	79,000	1,902	6,850	25.1	1,725.29	1,856.24	2.3496	.9759	.0518
23	Ending May 2, 1885.	2,678	3,038,633	Sheetings and Shirtings	16	150	35,720	1,212	1,865	24.	447.60	516.45	1.4458	.4261	.0147
24	....	1,694	583,000	Medium Sheetings	25.5	110 to 460	16,000	472	1,100	13.6	150.00	273.61	1.7100	.5796	.0257
25	Ending April 25, 1885	3,064	3,024,989	Colored goods, Ginghams	....	168 to 242	51,600	2,001	6,249	30.7	1,922.85	1,939.16	3.7578	.9690	.0635
26	....	264	39,673	Print cloth, 64 × 64	....	198 to 300	9,952	264	58	23.4	13.59	159.06	1.5982	.0025	.0342
27	....	264	139,828	Print cloth, 64 × 64	....	150 to 260	41,184	1,011	239.5	45.	107.88	1,262.68	3 0659	1.2489	.0771
28	6 months ending Ap. 25, 1885.	1,510	931,497	Fine Sheetings	....	....	37,504	800	1,450	35.2	510.55	1,044.76	2.7856	1.3059	.0548
29	....	60	8,000	45 to 50 yarn	....	287	15,584	328	25.5	....	11.15	574.22	3.6843	1.7506	.1393
30	....	3,130	1,924,853	Sheetings and Satteens	....	....	45,720	1,045	3,927	....	2,093.62	2,066.84	4.5206	1.9778	.1087
Total and Average		40,303	51,203,153	....	....	....	1,723,804	43,353	....	....	21,731.73	49,422.60	2.8670	1.1400	.04244

* Includes 775.354 lbs. yarn sold. Shafting has 2,455 hangers.

DISCUSSION.

Mr. Barrus.—The following table may be found of interest in connection with the experiments tabulated in Mr. Henthorn's paper, as well as in connection with Professor Thurston's paper. The results were obtained on some tests in my own professional practice.

MEMORANDA REGARDING POWER CONSUMED IN VARIOUS NEW ENGLAND COTTON MILLS.

1.	2.	3.	4.	5.	6.	7.	8.
Number of Mill.	Kind of Goods manufactured.	Kind of Engine, whether single cylinder or pair of cylinders.	Number of Spindles.	Average indicated power developed with machinery at work.	Indicated power developed with shafting and loose pulleys running.	Number of Spindles per horse-power.	Per cent. friction (column 6) to total power.
1.	Cambrics.	Single.	17,868.	314.	78.	5.69.	24.7.
2.	Cambrics.	Single.	29,904.	422.	89.	7.09.	21.1.
3.	Prints.	Pair.	43,712.	690.	153.	6.33.	22.1.
4.	Prints.	Pair.	34,080.	621.	125.	5.49.	20.1.
5.	She'tings.	Single.	23,584.	362.	85.	6.51.	23.5.
6.	She'tings.	Single.	15,584.	276.	71.	5.64.	25.7.
7.	Prints.	*Pair.	32,480.	618.	111.	5.25.	18.0.
8.	She'tings.	Pair.	56,224.	773.	163.	7.27.	21.1.

Mr. Walker.—It seems to me from my experience and the best authority I know of, that the coefficient 56 in this formula on the sixth page of the paper is entirely too small, even for good hammered iron shafts as stated by Mr. Henthorn.

Trautwine gives on page 182, *tenth thousand*,

$$D = \sqrt[3]{\frac{HP}{Rev} \times 125}.$$

William Sellers & Co., page 341, *Machine Tools*, gives same formula as above.

The coefficients in following table, which is the one which I use, give practically the same results as the above formula.

* One cylinder idle when friction test was made.

SHAFTING WILL TRANSMIT WITH SAFETY, BEARINGS SAY 8 TO 10 FEET CENTERS.

Dia. of Shaft, in Inches.	Horse-power in one rev.	Dia. of Shaft, in Inches.	Horse-power in one rev.	Dia. of Shaft, in Inches.	Horse-power in one rev.
$\frac{15}{16}$	.008	$2\frac{15}{16}$	.216	$5\frac{15}{16}$	1.728
$1\frac{3}{16}$	.0156	$3\frac{3}{16}$	.272	$6\frac{7}{16}$	2.195
$1\frac{7}{16}$	.027	$3\frac{7}{16}$	.343	$6\frac{15}{16}$	2.744
$1\frac{11}{16}$	.043	$3\frac{11}{16}$	.424	$7\frac{7}{16}$	3.368
$1\frac{15}{16}$	.064	$3\frac{15}{16}$	.512	$7\frac{15}{16}$	4.096
$2\frac{3}{16}$	.091	$4\frac{7}{16}$	.728	$8\frac{7}{16}$	4.912
$2\frac{7}{16}$	.125	$4\frac{15}{16}$	1.000	$8\frac{15}{16}$	5.824
$2\frac{11}{16}$	.166	$5\frac{7}{16}$	1.328	$9\frac{7}{16}$	6.848

My experience has shown that the above formula and coefficients are not too large when the transverse strain (or belt tension) is considered. I think Mr. Henthorn's formula would transmit the horse-power so far as torsional strength is concerned, but would be inclined to doubt the formula for the necessary transverse strain with bearings, say 8 to 10 feet centers.

To compare these formulæ I put the following question. What diameter shaft will be necessary to transmit 300 horse-power, the shaft to make 150 revolutions? According to Mr. Henthorn we have

$$\sqrt[3]{\frac{56 \times 300}{150}} = 4.82'' \text{ Dia.}$$

And according to Trautwine, Sellers & Co., we have

$$\sqrt[3]{\frac{300}{150} \times 125} = 6.29'' \text{ Dia.}$$

The difference of 1.47″ in diameter is considerable. I know of no manufacturer who would be willing to guarantee good work on the formula advanced by Mr. Henthorn. Transverse strain (or belt tension) is one of the worst elements the manufacturer of shafting has to contend with, and must be provided for otherwise than by putting a bearing at every pulley.

Mr. C. E. Emery.—I consider the papers which have just been read of great value, and that we should encourage a collection of data from actual experiments and their presentation to the Society in such form that comparisons can be made. It will be noticed that Professor Thurston in his paper found it of interest to make extracts from that of Mr. Henthorn in illustration of features brought out in his own paper.

I wish to call attention to one point in relation to the friction of

steam engines which is not generally known. The friction of engines determined from indicator diagrams with the load thrown off, known as friction diagrams, is frequently higher than the friction of the engine and load (the latter meaning the additional friction on the bearings of the engine caused by the external load). This results from the fact that all the stuffing-boxes about an engine are set up tight enough to balance the full steam pressure, and they consequently produce much more friction when the engine is running light with a very low pressure than when the engine is loaded. It will be seen therefore that when taking friction diagrams the stuffing-boxes should always be slackened off. In fact they should be no tighter than is required for the pressure in making all experiments on friction, such as that of shafting or of special machines. I have proved the inaccuracy of the customary methods of ascertaining the friction of an engine by means of friction diagrams, by the use of dynamometers transmitting the whole power of the engine.

Mr. Bancroft.—I would like to call the attention of the Society to the statement on page six of Mr. Henthorn's paper in regard to providing light single belts to do the work. In a number of experiments that my firm have been conducting lately in regard to the friction of transmitting the power by belting, we find that the friction is scarcely dependent at all upon the work done, but depends entirely on the original friction due to the tension of the belts, and the only case in which the work done affects the friction is where the character of the belt is such that the tension increases in doing work. Hence I consider that this suggestion of Mr. Henthorn's to diminish the tension by increasing the diameter of the pulleys is exceedingly valuable.

Mr. Babcock.—Allow me to state a fact in regard to tight belts. In tests which were made at the American Institute some years ago between two engines, the results were made to show directly the opposite of what they should have shown, from the fact that one belt was a great deal tighter than the other. When one engine was tested the load at first was so great that the belt slipped, whereupon it was tightened up as tight as it could be made with screw clamps. The load was subsequently reduced, and the other engine was run with its belt in its normal condition. The first engine showed a considerable percentage of saving on indicated horse-power, but on dynamometer power taken from the driven shaft it fell behind. This was ascertained afterward to be due

to excessive friction on the driving shaft resulting from this very tight belt, and not to any fault of the engine itself.

Mr. Hawkins.—The Mason Machine Works of Taunton, Mass., have thought it worth their while for a number of years to keep a man to attend to their belting through the whole establishment. Of course, that would be impracticable in small institutions, if confined to that labor alone; but the above-mentioned firm find that it pays to keep a man to do that exclusively. They will not allow a belt to be repaired or shortened or a new one put on by any one but this man. In a large machine shop, where the power required to run shafting is so great in proportion to the work actually done, a little addition of tension on the belts throughout the establishment would make it a very serious loss. I think that that is a subject that requires to be emphasized more in connection with friction and lubrication than has been heretofore done.

Mr. Bancroft.—Our experience has been exactly the same as has been referred to by the previous speaker. We have found it to our advantage to have one man to look after all the belts.

Mr. C. E. Emery.—I would like to ask the question whether or not statements heretofore published are confirmed by the results of practice, to the effect that a double belt will transmit the same power as a single belt of greater width, without extra friction, provided both are of the same tension.

Mr. Bancroft.—So far as our experiments have gone they show that the friction on the journal is proportionate to the tension, irrespective of the character of the belt or the diameter of the pulleys, excepting so far as the friction is affected by speed. The friction of journals is usually affected slightly by the speed of rubbing surfaces; but I mean taking any one given speed in revolutions per minute and change the diameter of the pulley, the width of the belt, or the thickness of the belt, and the variation in friction with a constant tension has been exceedingly slight. But the most curious thing that has been developed yet is the fact that with a flexible belt and a belt-stuffing which is somewhat adhesive in its nature, we have found that the sum of the tensions on the two sides of the belt changes considerably as the work is increased. For example, if the sum of the tensions at the beginning was 100 pounds, we have had instances where the sum of the tensions in doing work was 500 pounds. That was an extreme case. But invariably the sum of the tensions is increased. I am in hopes of getting that in shape so as to present it at some

future meeting of the Society. It is contrary to what has hitherto been advanced in the text-books, and has been regarded with doubt by a good many persons to whom I have mentioned it; but I am satisfied that it is possible to demonstrate the fact.

Mr. Thompson.—I have had some experience in increasing the transmitting power of belts by running one belt on top of another. I have no exact data as to the power saved or as to the difference in power between that and a double belt; but it seemed to do the work so much more easily, putting one belt over the other, and it did not seem to produce so much displacement in the molecules of the leather.

Mr. C. E. Emery.—Something somewhat in confirmation of the remarks of Mr. Bancroft occurred quite recently during the test of a dynamo at our boiler station. We were experimenting to determine the relative cost of electricity and gas, and, as a larger dynamo was to be used temporarily only, it was not thought necessary to purchase the wider belt required, but an old belt was used which was thoroughly saturated with oil. The belt would hardly run the dynamo at all, with the light load due to an open circuit at starting, although all the oil was wiped off that was possible, but as the load was increased the belt slipped less and less, and finally ran the dynamo at full capacity without increasing its tension.

Mr. Bancroft.—We have found too, by actual count, that the slip on such a belt as Mr. Emery describes may be as high as twenty per cent., but if that slip is allowed to continue until the pulleys become hot then a change takes place and the slip rising a little higher than that, the belt will come off almost immediately. It seems to be a question of the temperature at which the belt-stuffing becomes fluid.

The President.—It would seem from Mr. Emery's statement that the belt did not slip.

Mr. Emery.—With a light load the slipping was very great, and we had to put up boards to guide the belt, but, as more work was put on, the belt ran better and better. The belt was six inches wide and drove the dynamo when furnishing current for 260 to 270 incandescent electric lamps.

Mr. Sweet.—I assume that in such a dynamo-belt running at a high velocity, the oil in the belt went to the outside of it by centrifugal force after it got going. There is a limit to which the

oil can be kept on the inside of a belt as well as a limit at which a belt can be run.

Mr. Babcock.—I should like to ask Mr. Emery if he found that result repeated, or was it only the first time trying.

Mr. Emery.—We were making a series of progressive experiments, varying the number of lights and determining the power. We got the belt to operate satisfactorily after a while.

Mr. Hawkins.—I experimented a good many years ago with some very high-speed vertical-belts running wood-working machinery for the manufacture of spools. The centrifugal force of the belt was an element, and a very great one; so great that, in a vertical belt four inches wide, running over a four-inch diameter pulley at the bottom, the four-inch pulley running at 5,000 turns per minute, we could distinctly see that the belt touched the pulley at the bottom only, say, about one-half of an inch of its circumference. So long as it was kept up at that speed, it would run cool; but, if we attempted to run the same spindle at one-half the number of turns per minute, it would heat up immediately. The belt would become tight from relaxation of the centrifugal force, and the same belt when standing still would be under so great a tension as to resound like a fiddle-string.

Mr. Bancroft.—In some of our experiments we found precisely the same results that Mr. Emery speaks of. After the belt had been running there was a very great deal of slip, so that we would have to stop occasionally and clean off the surface of the belt; but as soon as the stuffing became sufficiently absorbed and the surface of the belt got into proper condition, the slipping ceased. But the twenty per cent. slip that I spoke of comes in as the load is gradually increased beyond what the belt is really capable of performing.

CLXXV.

APPARATUS USED IN TESTING MATERIALS.

BY GUS. C. HENNING, M. E., NEW YORK.

SEVERAL years' work at testing iron and steel had made it evident to the writer that all auxiliary appliances in common use to measure elongations of metals when subject to stress, are so extremely crude and unreliable that it was desirable to obtain better apparatus.

So many points had to be considered to make such apparatus applicable in every instance, that it appeared desirable to use one kind for short specimens and another for long ones. Each had to fulfill totally different requirements, and besides being equally accurate throughout its range of work was to be so constructed that the apparatus could easily and at any time be checked by the observer to prove its accuracy.

This latter point became so highly desirable by failure of old devices to give identical results for repetition of identical stresses that it appeared necessary to prove the correctness of apparatus employed before placing any confidence in results obtained.

These devices were of necessity to be of such design as would make them convenient and practicable to be used in daily work under surrounding disturbing influences, and at the same time would not be liable to derangement from slight causes.

Two years' use of one, and nine months' almost daily use of the other, have demonstrated their practicability; the annexed tables of repeated measurements will demonstrate their accuracy.

The design of this apparatus is laid before this society for the purpose of obtaining opinions and criticisms in order to be able to improve it still further, and to construct thoroughly reliable and accurate devices, to assist in the interpretation of results when testing materials.

The first apparatus, which was designed in collaboration with Mr. C. A. Marshall, C. E. Engineer of Tests, Cambria Iron Co., Johnstown, Pa., shown in Figs. 151, 152 and 153, in elevation, bottom view and half side view, is a "double self-centering electric-contact Micrometer." This apparatus consists essentially of two carrying frames, *A* and *B*, surrounding the test piece, *s*, symmetrically, which

are free to move with any change of length in the direction of such change, which latter is measured by means of a pair of finely divided micrometer screws and heads, *m*, carried by the one frame, opposite a pair of contact plugs, *g*, carried in a similar manner by the other frame. To secure a perfectly symmetrical adjustment of this

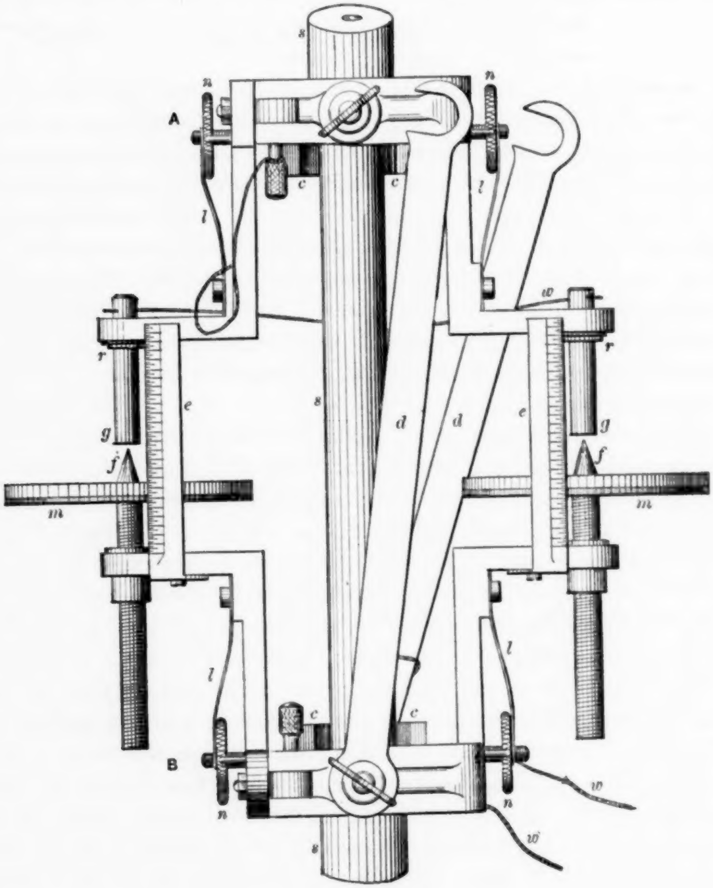


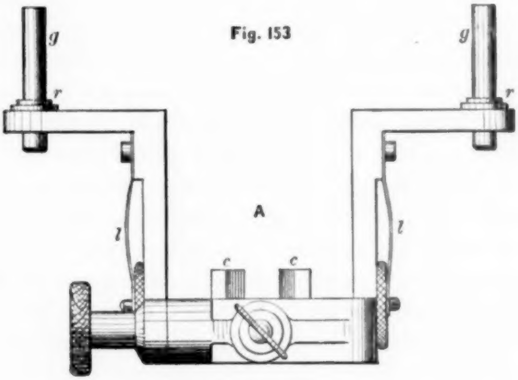
Fig. 151

apparatus with reference to the axis of the test piece, each frame is provided with a pair of spring-cushioned platens, *c l*, and a pair of centering screws, *h*, placed on opposite sides of the frames and exactly midway between the micrometers; the vertical distance between the pairs of screws in the two frames is fixed by means of the side bars, *d*, which swing around the shoulders on the frames, and are

made of any desired length, six, eight, ten, or more inches as seems desirable for the test.

To facilitate the application of this apparatus, one side of each frame is hinged, but is fitted so nicely that there is no looseness or play in the joints of the hinge, *p*, and the frame when closed is practically solid.

Fig. 153



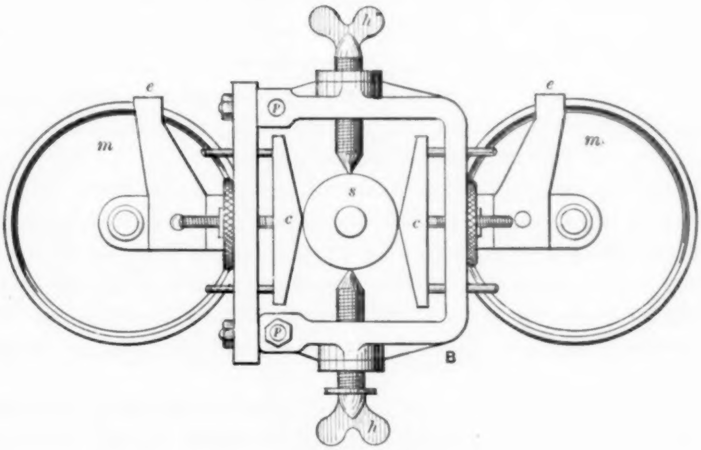


Fig. 152

Vertical scales, *e*, are provided to measure the amount of elongation when greater than the pitch of the micrometer screw. Each of the frames is connected with an electric bell, a circuit, *w w*, being established by bringing the point, *f*, of micrometer in contact with contact plug, which is insulated electrically by a gutta percha bushing, *r*, inserted in the upper frame. One wire is attached to the ends

of insulated contact plugs which are also connected to each other. The other wire of the circuit is connected to the lower frame.

To be able to adjust the apparatus to any specimen, each spring platen is provided with a stud bolt passing through the frame and spring and carrying a nut, *n*, between the two, so that each may act as a bearing for the nut when it is turned to increase or decrease the space between the opposite platens; to obtain a uniform adjustment these platens are adjusted so as to stand the same distance from the axis of the instrument. When a test piece is inserted, these platens, having been properly adjusted, will be forced outward symmetrically, as the springs are of equal strength and will therefore carry the frames with them. As the edges of these platens are parallel and bear directly against the surfaces of the test piece, they will cause the frames to stand parallel to it, and therefore as the micrometers are placed parallel in the frames, they will stand parallel to the axis of test piece. To insure proper adjustment in a plane normal to that of micrometers, the centering screws are brought to bear against the surface of the test piece after the side bars have been thrown over the shoulders on frame, and the points of micrometer have been placed under the centers of contact plugs. Now the centering screws are forced into the test piece slightly until they have taken a secure hold to prevent slip; after this the side-bars are removed gently and having attached the electric wires the apparatus is ready to measure the elongation in the measured distance adopted.

It will be noticed that the plane of contact lies midway between the centering screws, and this is one of the features of this design, as thus any flexure occurring in the gauged length does not introduce errors of measurements, as both screws and contact plugs will swing about the centering screws, and the lateral motion of the one will be precisely equal to that of the other, and the point of contact will not shift.

To measure elongations in six inches, the contact plugs are withdrawn from their insulating bushes and others exactly one inch shorter are put in their place, while the micrometers are lowered one inch, thus leaving the plane of contact on the medial plane of the apparatus. To measure elongations in greater lengths, longer plugs are inserted and the micrometers raised a proper amount.

The apparatus as shown is equally applicable to all shapes, from the size of a fine wire up to a piece measuring 1½ inches in either or all directions.

The essential features of this apparatus are,

1. Its perfectly symmetrical construction.
2. Equally easy application to all shapes.
3. Certainty of symmetrical adjustment about any test piece.
4. Its exact adjustment to a definite length.
5. The large size of micrometer heads, giving large subdivisions and avoiding entirely the errors due to the momentum of the hand, and also requiring the least possible force to turn the heads, which greatly facilitates ready and accurate manipulation.
6. Its accuracy of construction, having been built by the Brown & Sharpe M'f'g. Co., of Providence, R. I.

Below are tables Nos. I., II. and III. which give the elongations of a bar of steel, observations having been repeated eight times in rapid succession under loads increasing by 1,000 lbs. at a time from 0 to 22,000 lbs. on the beam of the testing machine; these elongations are the average readings of the two micrometers, and 344 observations were taken in 2 hours and 40 minutes, which is about as rapid work as can be done under favorable conditions; had any attempt been made to obtain precisely identical results by taking more time, there is but little doubt that even the slight errors found would have been eliminated.

TABLE NO. I.

Pickled Round No. 682. March 29, 1885.

READINGS OF MICROMETERS OF ELECTRIC-CONTACT, SELF-CENTERING MICROMETERS; DESIGNED BY GUS. C. HENNING, M. E.

	1st.			2d.			3d.			4th.			5th.			6th.			7th.			8th.		
Loads applied.	L. Screw.	Average Reading.	R. Screw.	L. Screw.	Average Reading.	R. Screw.	L. Screw.	Average Reading.	R. Screw.	L. Screw.	Average Reading.	R. Screw	L. Screw.	Average Reading.	R. Screw.	L. Screw.	Average Reading.	R. Screw.	L. Screw.	Average Reading.	R. Screw.	L. Screw.	Average Reading.	R. Screw.
0	147	187.75	228½	136	188.50	241	137	189.25	241½	139¾	188.12	236½	140½	188.12	236¼	139	188.75	238½	140	188.50	237	150	187.75	225½
1,000	155	192.75	230½	147½	193.50	239½	145	193.75	242½	143½	193.00	242½	150¼	193.12	236	153½	193.75	234	159¾	193.80	227¼	173	193.50	214
2,000	157¾	198.00	238¼	152½	198.50	244½	149	198.37	247¾	146	197.87	249¾	151	198.37	245¾	150¼	198.50	246¼	150	198.62	247¼	160½	198.50	236½
3,000	168	203.50	239	160¾	203.37	246	157¾	203.62	249½	155	202.75	0½	158	203.00	248	157¾	203.25	248¾	157¾	203.62	249½	162	203.25	244½
4,000	197	208.00	239	167¾	208.50	249¼	166	209.00	2	163½	208.25	3	166¼	208.12	250	166	208.62	1¼	164¼	208.50	2¼	169¾	208.50	247¼
5,000	186	213.50	241	174¾	213.87	3	174	214.00	4	171¼	213.12	5	175	213.50	2	174¼	2.3.87	3½	173	213.75	4½	176¾	213.62	0½
6,000	193	218.12	243¼	182	218.62	5¼	181	219.25	7½	177½	218.00	8½	182½	218.75	5	181¾	218.87	6	180	218.75	7½	183¾	219.00	4¼
7,000	201	223.62	246¼	189	223.87	8¾	188¼	224.62	11	185	223.25	11½	190	224.00	8	188½	223.87	9¼	188	224.25	10½	190¾	224.12	7½
8,000	208	229.00	250	195¾	229.00	12¼	194	229.25	14½	192	228.62	15¼	195	228.25	11½	195	228.87	12¾	195	229.75	14½	196	228.62	11¼
9,000	213¾	234.37	5	201½	234.25	17	200½	234.25		197¾	233.50	19¼	202½	234.25	16	201½	234.37	17¼	200	234.75	19½	203	234.62	16¼
10,000	219	239.25	9½	207¼	239.62	21	205¾	239.87	24	207½	238.25	24	209	239.50	20	208	239.50	21	206½	240.00	23½	208¾	239.50	20¼

11,000	224¾	244.62	14½	213	244.50	26	212	244.75	27½	209	244.50	30	214	244.37	24¾	213	244.25	25½	211	244.75	28½	213	244.12	25¼
12,000	231	249.75	18½	218¾	249.62	30½	217½	249.87	32¼	216	249.12	32¼	219½	249.25	29½	218	249.75	31½	216	249.62	33¼	219	249.37	29¾
13,000	236¼	254.00	21¾	221¼	254.62	35	223½	254.87	36¼	222½	255.00	37½	225¼	254.62	34	223½	254.12	35¾	222½	254.50	36½	224¼	254.50	34¾
14,000	241½	259.75	28	231	260.00	39	229½	260.00	40½	228	259.62	41¼	230¾	259.50	38¼	229½	259.87	40¼	228	259.50	41	230	259.50	39
15,000	247¼	255.37	33½	237	265.87	43¾	234¼	264.62	45	233¾	264.87	46	237½	265.50	43½	234¾	264.87	45	234	265.00	46	235	264.50	44
16,000	2¼	270.25	38¼	242	270.12	48¼	240¼	270.12	50	240	270.25	50½	241¾	270.00	48¼	240	270.12	50¼	238¾	270.00	51¼	240½	269.87	49¼
17,000	7¾	276.00	44¼	247¾	275.87	54	245½	275.25	55	245	275.25	55½	247	275.25	53½	246	275.50	55	244¼	275.12	56	246	275.25	54½
18,000	13	280.25	47½	3	280.87	58¾	1¼	280.75	60¼	0	280.25	60½	1¾	280.62	59½	0	280.50	61	249½	280.37	61¼	0¼	280.00	59¼
19,000	18	286.25	54½	8¾	286.37	64	6¼	286.25	66¼	5	285.62	66¼	7¾	286.25	64¾	5½	285.87	66¼	4½	285.75	67	6	285.50	65
20,000	22¾	291.37	60	14	291.50	69	11½	291.37	71¼	10¼	290.75	71¼	11¼	290.87	70½	10	291.00	72	9	290.75	72½	11	290.75	70½
21,000	27	296.87	66¾	19½	296.75	74	17	296.50	76	16	296.25	76½	16¾	296.12	75½	16½	296.75	77	15	296.25	77½	16	295.75	75½
22,000	29½	301.75	74	24¾	302.00	79¼	22	301.25	80½	21¼	301.12	81	22½	301.37	80¼	22	301.75	81½	21¼	301.75	82¼	22	300.87	79¾
2,000	176	199.75	223½	173¾	199.37	225	179¾	199.62	219½	183	198.50	214	186½	198.75	211	190¾	198.75	206¾	198	198.00	198	196	198.25	200½
0	136	188.75	241	137	189.25	241½	138	189.00	240	140½	188.37	236½	139	188.75	238½	140	188.50	237	150	187.75	225½	161½	188.25	215

TABLE III

Elongations for 1,000 lbs. intervals from 0–22,000 lbs. load.

	in.	in.	in.	in.	in.	in.	in.	in.
0–1,000	.000500	.000500	.000450	.000488	.000500	.000500	.000500	.000575
1–2,000	525	500	462	487	525	475	512	500
2–3	550	487	525	478	463	475	500	475
3–4	450	513	538	550	512	537	488	525
4–5	550	537	500	497	538	525	525	512
5–6	462	475	525	488	525	500	500	538
6–7	550	515	537	525	525	500	550	512
7–8	538	513	463	537	425	500	550	456
8–9	537	525	500	488	600	550	500	600
9–10,000	488	537	562	475	525	513	525	488
10–11	537	488	488	625	487	465	475	462
11–12	513	512	512	462	488	550	487	525
12–13	425	500	500	588	537	437	488	513
13–14	575	538	513	462	488	575	500	500
14–15	562	587	462	525	600	500	550	500
15–16	488	425	550	538	450	525	500	537
16–17	575	575	513	500	525	538	512	638
17–18	425	500	550	500	537	500	525	475
18–19	600	550	550	537	563	537	538	550
19–20,000	512	513	512	512	462	513	500	525
20–21	550	525	513	550	525	575	550	500
21–22	488	525	475	487	525	500	550	512

Elongations in 5,000 lbs. intervals.

									Average.	E.
2–7,000	.02562	.02537	.02625	.02538	.02563	.02535	.02563	.02560	2560.4 =	31,245,300
7–12,000	2613	2615	2525	2587	2525	2588	2537	2525	2564.4 =	31,196,400
12–17,000	2625	2625	2538	2613	2600	2575	2550	2588	2589.2 =	30,916,900
17–22,000	2575	2613	2600	2587	2612	2625	2663	2562	2593.4 =	30,847,500

Average E = 31,047,400

Difference in E between 0.0256″ and 0.0257″ = 121,600 lbs.
" " " Interval 2–7,000 lbs. and 17–22,000 lbs. load = 395,900 lbs.

Elongations between 2,000 lbs. and 22,000 lbs. load

								Average.	E.
in.	in.	in.	in.	in.	in.	in.	in.		
.010375	.010350	.010288	.010325	.010300	.010325	.010313	.010237	.0103377	= 30,954,600

Highest Value of E taking Elongation between 2,000 and 22,000 lbs load = 31,259,100
Lowest " " " " " " " " = 30,843,300

Average Elongation for 1,000 lbs. load increments.

								Average in.
.000518	.000515	.000509	.000514	.000515	.000514	.000515	.000514	.00051425

TABLE II.

Observations of Elongation of Pickled Round, C. I. Co., Lab. No. 682. Area of Test Piece = .5001 sq. in. March 25, 1885.

GUS. C. HENNING.

Time of Eight Series of Observation, 2 hrs. 40 min.

No Rests between Series.

MEAN READINGS OF DOUBLE MICROMETER.

Loads.	1st Series.	2d Series.	3d Series.	4th Series.	5th Series.	6th Series.	7th Series.	8th Series.
	in.	in.	in.	in.	in.	in.	in.	in.
0	.018775	.018850	.018925	.018812	.018812	.018875	.018850	.018775
1000	19275	19350	19375	19300	19312	19375	19350	19350
2	19800	19850	19837	19987	19837	19850	19862	19850
3	20350	20337	20362	20275	20300	20325	20362	20325
4	20800	20850	20900	20825	20812	20862	20850	20850
5	21350	21387	21400	21312	21350	21387	21375	21362
6	21812	21862	21925	21800	21875	21887	21875	21900
7	22362	22387	22462	22325	22400	22387	22425	22412
8	22900	22900	22925	22862	22825	22887	22975	22862
9	23437	23425	23425	23350	23425	23437	25475	23462
10000	23925	23962	23987	23825	23950	23950	24000	23950
11	24462	24450	24475	24450	24437	24425	24475	24412
12	24975	24962	24987	24912	24925	24975	24962	24937
13	25400	25462	25487	25500	25462	25412	25450	25450
14	25975	26000	26000	25962	25950	25987	25950	25950
15	26537	26587	26462	26487	26550	26487	26500	26450
16	27025	27012	27012	27025	27000	27012	27000	26987
17	27600	27587	27525	27525	27525	27550	27512	27525
18	28025	28087	28075	28025	28062	28050	28037	28000
19	28625	28637	28625	28562	28625	28587	28575	28550
20000	29137	29150	29137	29075	29087	29100	29075	29075
21	29687	29675	29650	29625	29612	29675	29625	29575
22	30175	30206	30125	30112	30137	30175	30175	30087
Dropped to								
2000	19975	19837	19962	19850	19875	19875	19800	19825
0	18875	18925	18900	18837	18875	18850	18775	18825

Elongations from 0 lbs. to 22,000 lbs. load.

.011400	.011350	.011200	.011300	.011325	.011300	.011325	.011312

Fig. 155 shows the elastic curves of Tables I., II. and III. of three observations.

The second apparatus is what is frequently but inappropriately called a "modulus apparatus," because the modulus of elasticity is determined by interpretation of observations taken therewith, or by deductions therefrom.

Fig. 154 gives the general construction of this apparatus in detail. It consists principally of a graduated circle with a concentric

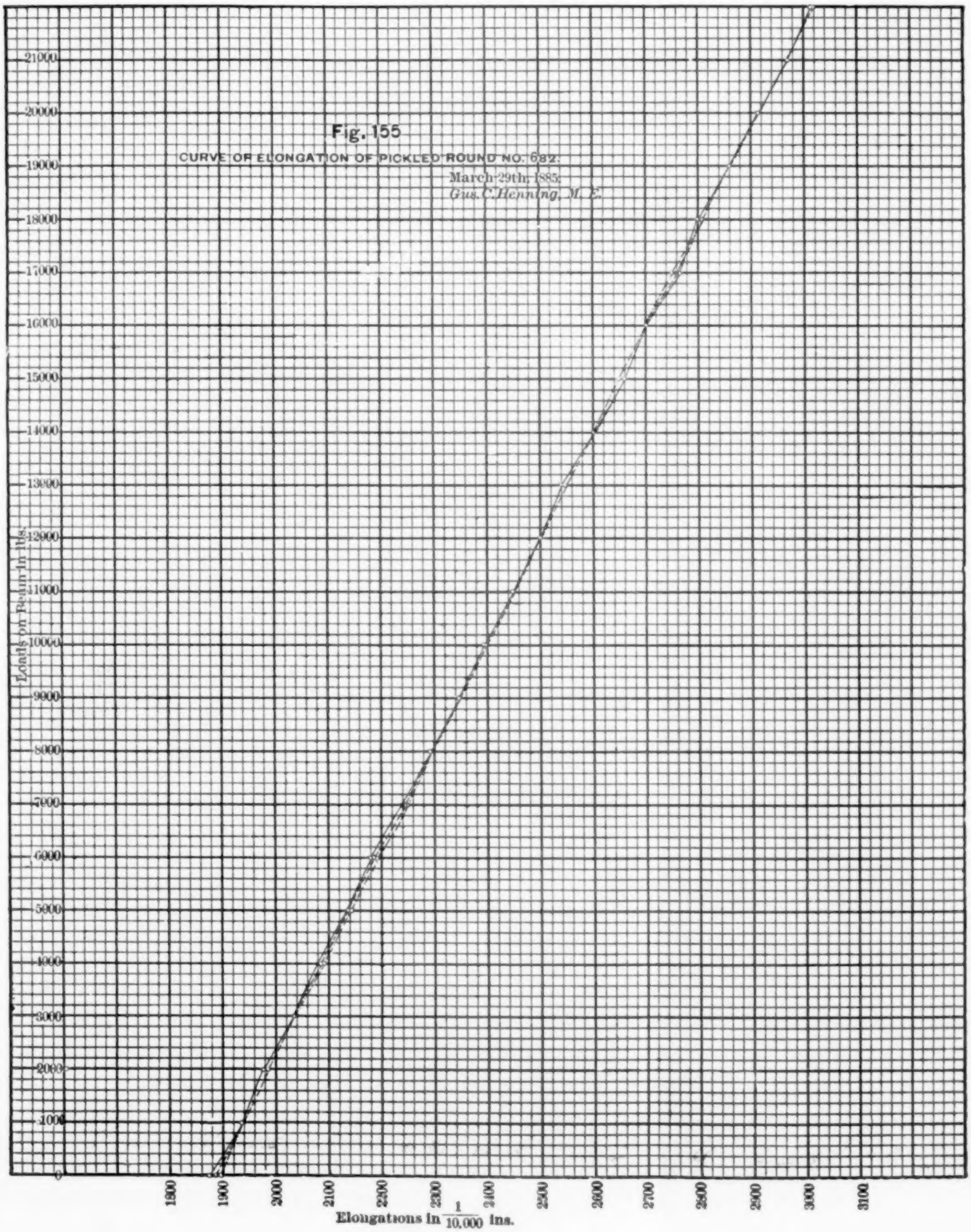


Fig. 155
CURVE OF ELONGATION OF PICKLED ROUND NO. 682.
March 29th, 1885.
Gus. C. Henning, M. E.
Loads on Beam in lbs.
21000
20000
19000
18000
17000
16000
15000
14000
13000
12000
11000
10000
9000
8000
7000
6000
5000
4000
3000
2000
1000
0
1800
1900
2000
2100
2200
2300
2400
2500
2600
2700
2800
2900
3000
3100
Elongations in $\frac{1}{10,000}$ ins.

hub, free to revolve over a vernier in a horizontal plane when strained by a tape attached to the hub and an attaching post.

The frame A, so constructed as to be easily attached to any rod, carries the vernier bracket B by a steel stud-pin normal to the plane of rotation and concentric with vernier V. This bracket has a lug through which a light rod passes carrying a lens L opposite the vernier and which can be readily adjusted for focus. The up-

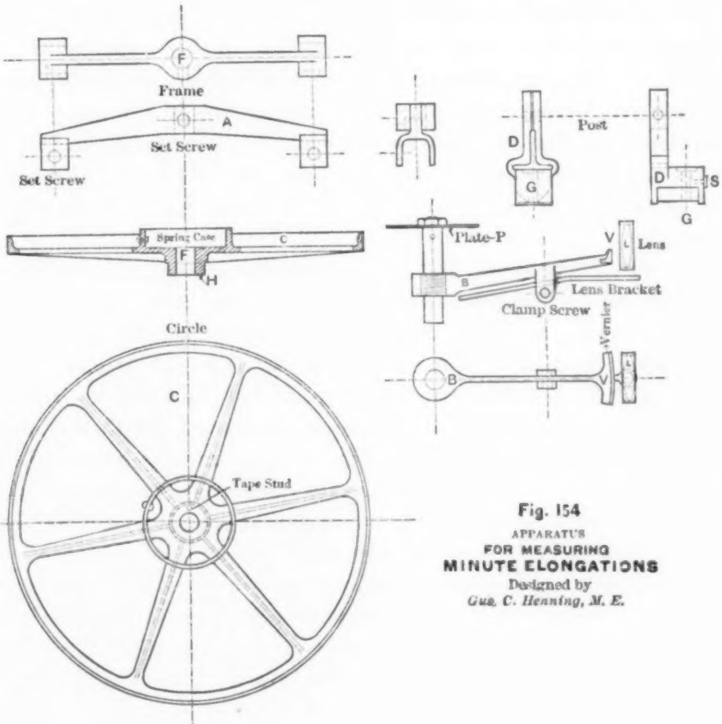


Fig. 154
APPARATUS
FOR MEASURING
MINUTE ELONGATIONS
Designed by
Gus. C. Henning, M. E.

per part of stud-pin is also the axis of graduated circle C, which latter is provided with a spring case containing a clock spring, one stud of which is attached to spring case, while the other is attached to stud-pin. By means of this spring the apparatus returns to the original reading upon removal of load on bar to be tested.

The gauge rod which carries the frame is secured to definite points on the bar by means of center punches pressed into the punch marks on the bar and held firmly by means of springs straddling

the bar. As one end of the gauge rod must be free to move with the elongation of the bar under varying loads, the free end slides in a slot in the post *D*, held in place by a spring and center punch *G*. The upper end of this post is split and provided with a thumb screw to fix the free end of the pulling tape which is secured to the hub below the circle.

Thus having made two center punch marks, spaced a certain distance apart, the center punches in the end of the gauge rod and post are dropped into these points and the springs placed so as to press on the heads of the center punches and on opposite sides of the bar. The tape having been previously fastened, the apparatus is ready to work, and upon a load being applied to the bar and stretching it, the free end of the gauge rod will slide in the post, thus causing the circle to revolve over the vernier. Upon release of stress the spring will cause the circle to return to its original position.

It will be noticed that the apparatus is balanced, and as the motion is in a horizontal plane the disturbing effect of tremor and jars is entirely avoided; also, as the center of gravity is always in the same position and the direction of strain never changes, the friction will be constant and uniform for all positions of circle. In this construction the weight of the apparatus has no effect whatever upon the observations, as it will have in all arrangements of lever apparatus such as those designed by Bouscaren, Paine, and others, which can only be applied in a horizontal position, as their action would be totally different in any other.

This apparatus may be applied and used in a vertical position with equally reliable results, as the relative positions of weights is constant, and gravity can have no effect upon the motion of any part.

The hub *H* is supposed to measure exactly $\frac{1}{10}$ of the diameter of the circle in order to increase motion tenfold to facilitate observations, but as there may be errors of workmanship a provision ought to be made, as in every instrument of precision, to determine the values of observations taken; this should be done by apparatus which is to be used for test purposes only and known to be correct. For this purpose the post *D* is provided with a fine micrometer screw attached by means of the screw *S*, in such a manner that any motion of the gauge rod may be readily measured by the micrometer and every point of the scale checked by repeated readings on the circle for all positions. In this apparatus I have thus determined the value of scale readings for each revolution of micrometer;

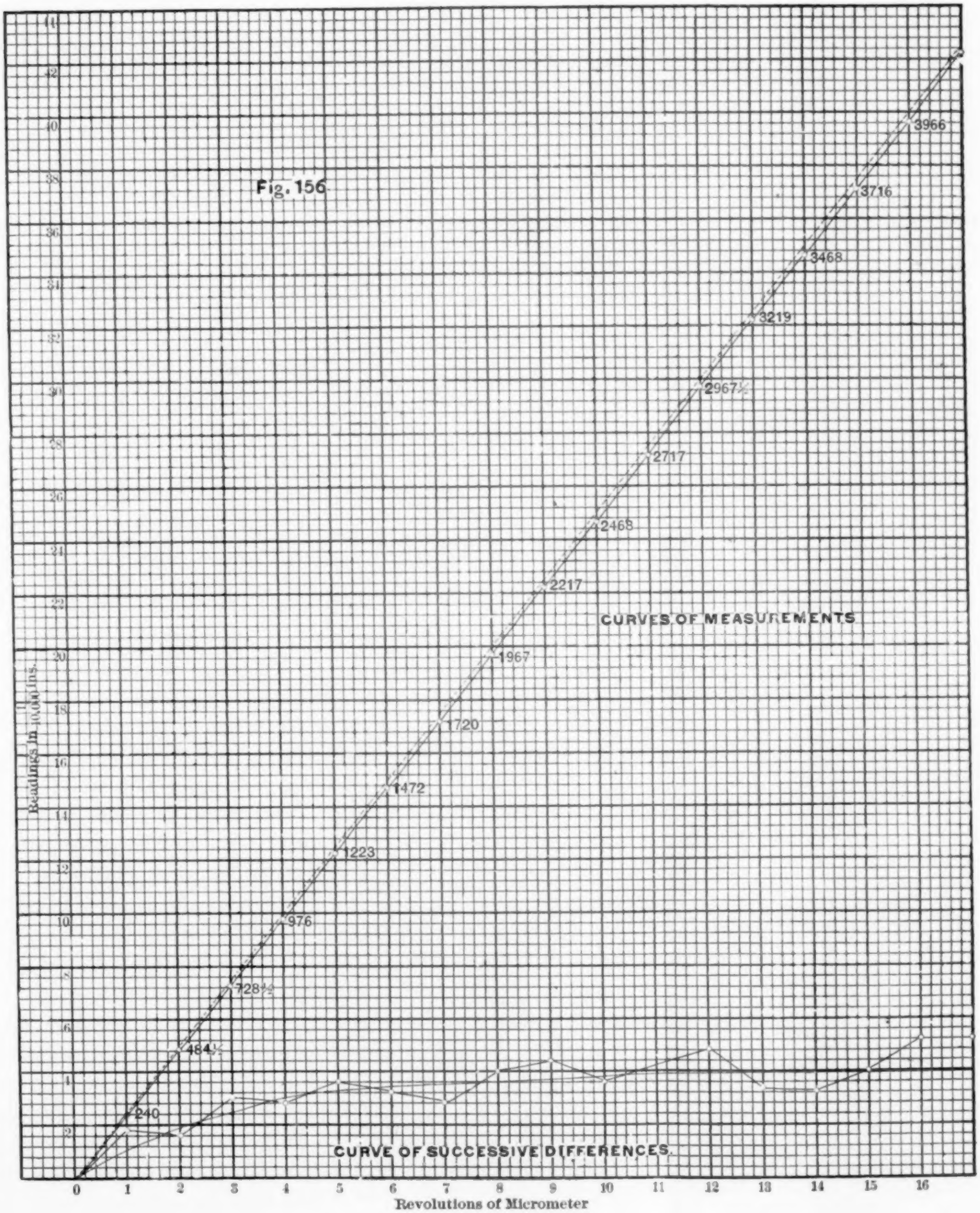


Fig. 156
CURVES OF MEASUREMENTS
CURVE OF SUCCESSIVE DIFFERENCES.
Readings in 10,000 ins.
240
484½
728½
976
1223
1472
1720
1967
2217
2468
2717
2967½
3219
3468
3716
3966
0
1
2
3
4
5
6
7
8
9
10
11
12
13
14
15
16
Revolutions of Micrometer

the annexed Table No. IV. gives the repeated observations of one such apparatus which reads to the $\frac{1}{500}$ part of an inch, while the checking micrometer reads to the $\frac{1}{5000}$ part of an inch.

TABLE IV.

Revol's.	Readings on Circle.				Average.
0	0	0	0	0	0
1	240	240	240	240	240
2	484	484	486	484	484½
3	728	730	728	728	726½
4	976	976	976	976	976
5	1220	1224	1224	1224	1223
6	1472	1472	1472	1472	1472
7	1720	1720	1720	1720	1720
8	1964	1968	1968	1968	1967
9	2216	2216	2220	2216	2217
10	2468	2468	2468	2468	2468
11	2716	2716	2718	2718	2717
12	2966	2968	2968	2968	2967½
13	3220	3218	3220	3220	3219½
14	3466	3470	3468	3468	3468
15	3716	3716	3716	3716	3716
16	3968	3966	3964	3966	3966
17	4218	4220	4220	4218	4219
18	4472	4472	4472	4472	4472
19	4720	4720	4720	4720	4720
20	4972	4968	4968	4968	4969

5 in. on arc = 5,034 in. by micrometer.

These four series of measurements are plotted in Fig. 156, which also shows a mean curve of successive differences for equal intervals of arc. These measurements show plainly that in this particular instrument the graduated arc is not concentric with hub, but is somewhat eccentric, and therefore the mere observations of stretch of bars would be erroneous if no correction be applied. By interpolation, the value of every point of the arc is found; and having taken a number of observations, the values of the observed readings are obtained by referring to this interpolation. As long as the error of any apparatus can be and is readily determined, it is quite unnecessary to have the apparatus itself strictly accurate; in fact, it is so difficult a matter to secure perfect accuracy that it is far preferable to take an apparatus as substantially accurate as is practicable, and then carefully to determine all errors, which can be checked and verified at any time.

Measurements of every description are purely relative, not abso-

lute, and therefore it is not necessary to have perfect apparatus, provided the equation of error is determinable and known. For instance; it would be impossible to obtain perfect yard measures, as there are no two perfectly alike, and the best vary differently for different conditions. It is therefore the usual plan to use a comparatively accurate measuring apparatus, the equation of error of which has been carefully determined.

Another apparatus of this kind which the Brown & Sharpe Manufacturing Co., of Providence, R. I., have constructed for the writer, has an arc which is divided into $\frac{1}{40}$ inches, giving observations of elongation to the $\frac{1}{10000}$ part of an inch.

Such apparatus can be used for compression by simply attaching the pulling tape on the opposite side of the hub to that which would be used for tension, and the action of the apparatus will be precisely the same.

When it is desirable to measure considerable changes of length, from one to three inches, a little bracket or table is attached to the post which carries and guides the forward end of the gauge rod. A complete revolution of the graduated circle, which is $10\frac{3}{16}$ inches in diameter, corresponds to a change of length of 3.2 inches.

It may be urged that the tension on the tape will cause it to elongate and thus introduce errors in results; as the difference in tensions on the tape is less than one half pound it is readily seen that the elongation due to such load on a tape which has a sectional area of $.375 \times .005 = .001675$ sq. in., in 15 inches of length, will be equal to less than $\frac{7,500,000}{30,000,000 \times 1,675} = 0.000134$ in., which is a variation from the average correct result of less than the smallest division of vernier.

It may also be urged that the motion of the gauge rod will lessen the obliquity of the tape to the direction of motion and thereby introduce another error; the effect of this change of direction of tape must be considered, as the tape is not entirely parallel to the gauge rod, but has a deflection of $\frac{1}{8}$ in. in twelve inches on either side of the rod. With a possible motion of the rod of three inches this will change the direction of tape from $\frac{1}{8}$ in 12 to $\frac{1}{8}$ in 15 in. and the error will be directly in proportion to the length of tape in these two positions. In the first case, the length of tape will be $= \sqrt{(\frac{1}{8})^2 + (12)^2} = 12.0000101$ inches, in the second case the tape will be $= \sqrt{(\frac{1}{8})^2 + (15)^2} = 15.0000081$ inches; and the difference of

tape in two positions or error due to same = 0.0000020 in., which is by far less than the smallest division on vernier.

If the tape deviated from the direction of the rod as much as ¼ in. then this would produce an error of 0.000479 in. for three inches motion of rod or thirty inches motion of circle and correspondingly less for less motion.

Moreover, when using this apparatus for the determination of the modulus of elasticity, for which purpose this apparatus was especially designed, the motion of the circle will be less than two inches for a gauged length of twenty feet, under loads of 25,000 lbs. per square inch, and the consequent error introduced for differences of direction of tape will be infinitesimal.

Even this error, small as it is, can be reduced by using a longer tape between the hub of circle and post.

One point must be borne in mind in using this apparatus if correct results are essential; that is, the bar or rod or section to be tested must lie in a plane during the entire test; should the weight of bar produce any distortions or variations, this or any other apparatus can never give correct results.

One particular feature of this apparatus is its simplicity, and small number of moving parts, which are but two: the circle and the tape.

DISCUSSION.

Mr. Kent.—I had the pleasure of seeing this instrument about two weeks ago, and while I had no opportunity of making a thorough test of it, it impressed me very favorably as being probably the most accurate instrument which has yet been designed for the purpose. But Mr. Henning's paper might be understood by some as giving the impression that Mr. Henning was the inventor of the whole apparatus. He has only invented certain improvements of an apparatus which has already been in use for about ten years; and it may be interesting to state the history of the getting up of that apparatus. In 1875 I was employed by Professor Thurston in making tests of materials. It was necessary to get some better method than we had of measuring elongations. I applied to Professor Mayer to recommend some method of measuring, and he suggested a micrometer screw with an astronomical level attached to it, such as is used in Saxton's comparator, and such an apparatus was made which measured ten-

thousandths of an inch. We found that that worked very satisfactorily in transverse tests, but not in tensile. Professor Mayer was then experimenting on electric contact as a means of measuring distances, and he had me make some experiments to determine what the error of that method was. We took a very fine micrometer screw, made in Paris, and fastened it to a glass plate and took a small battery, and applied a weak current of electricity, making and breaking it to ring a bell. With that very fine screw, and with a weak current, I found that the error of contact was something less than $\frac{1}{100000}$ of an inch. We then took the same micrometer which we had used with the contact level, applied the electric current, and used it to measure elongations. That was the first application of the electric contact to the measuring of elongations, but we found then the error of the bending of the specimen made the apparatus worthless; and at the first operation several people at once suggested the way of remedying the error by using two micrometers, one on each side. Scarcely any one can get the credit of that invention, because half a dozen, I suppose, suggested it at one time. But I think I was the first to use the micrometer screw with a contact level to determine elongations, and also the first to use electric contact for the same purpose. A sketch of the first apparatus with two screws will be found in my paper on "Strength of Materials," Van Nostrand's Science Series, No. 41, published in 1879. On page 5 the paper mentions the essential features of this new apparatus —1. Its perfectly symmetrical construction. 2. Equally easy application to all shapes: both these points were in the first instrument. 3. Certainty of symmetrical adjustment about any test piece. 4. Its exact adjustment to a definite length. The third and fourth are, I believe, the improvements of Mr. Henning, and for these he should get credit, and he has made, probably, the most perfect apparatus in use for the purpose.

Mr. Bond.—I would like to ask Mr. Kent if there would be any objection to using a microscope passing over a finely graduated scale? Would the shock of the breaking of the specimen have any tendency to injure the microscope? I should think that this would be the simplest method that could be adopted for the purpose.

Mr. Kent.—We found that after repeated trials it was generally useless to adopt this apparatus for elongations much beyond the elastic limit. We then had to take a simple compass and scale.

Within the elastic limit of the test piece this apparatus is entirely satisfactory. The use of the microscope would, no doubt, be equally accurate. But the convenience of operation of the micrometer with electric contact is vastly superior to that of the microscope.

CLXXVI.

THE POLAR PLANIMETER.

BY CHARLES E. EMERY, PH.D., NEW YORK.

It is safe to say that the principles of the Polar Planimeter are not generally understood. The term "Planimeter" was first applied to an instrument invented by a Mr. Oppenkoffer in 1827. This instrument was improved by a Swiss engineer, M. Welty, in 1849, and in 1854 the now well known Polar Planimeter was invented by Prof. J. Amsler of Schaffhausen, Switzerland. In a circular issued in English about the year 1863, relating to the Amsler instrument, it is stated that "a Polar Planimeter will not work with accuracy and precision if it is not constructed with the greatest care and a true knowledge of its mathematical principles; therefore no counterfeit of the instrument is possible;" and in another paragraph the information is volunteered that the number engraved on the arm (which is to be added to the reading of the instrument when the pole is located within the figure to be measured) represents the area of a circle with the polar arm as radius. The latter statement is positively incorrect, and in connection with that quoted above may have been intended to discourage and possibly prevent the manufacture of the instrument in England and elsewhere. From time to time during the last twenty years discussions of the principles of the instrument have appeared in technical works, some of them, it is said, employing in the demonstration the Calculus of Variations, and all, so far as the writer has seen, treating the subject in a general way as a mathematical problem or puzzle, but without formulating practical rules to enable the instrument to be properly constructed or adjusted when out of order.

The writer has recently had occasion to investigate the principles of the instrument in order to adapt it for a special use in connection with his business, and proposes in this paper to discuss the principles by both elementary and general methods, and to present the results in detail in convenient form for ready reference.*

*** On account of some difficulties experienced with one of the original Amsler instruments and the discovery of the erroneous statement above referred to, the writer decided to make an original investigation of the subject without reference to other discussions. Upon completing the investigation the**

The following brief statement of the extremely simple general principles and governing proportions of the planimeter will be of interest to those who do not desire to study the demonstration.

An arm carrying in bearings a revolving roller will be operative as a Planimeter, to show the area of any figure of which the perimeter is traced by one end of the arm, provided the other end be guided to move back and forth in the same line, either straight or curved.

First Condition.—*When the angle of main arm is changed less than* 360° *and the tracer is moved backward to the original position.* In this case the axis of the roller should be parallel to a line joining the centers at the ends of main arm, when the only proportions that affect the result are the length of the main arm and the diameter of the recording wheel. If N = length of main arm from tracer to hinge, D = diameter of recording wheel, and J = number of divisions on the wheel, each representing a unit of area—

difficulty with the Amsler instrument was traced to an error in the engraving opposite the divisions on the arm. Some literature on the subject was then collected and examined. Mr. J. C. Hoadley, M. E., loaned the work of Prof. G. A. Hirn, entitled "Theorie Analytique Élémentaire du Planimétre Amsler," published in Paris in 1875, which elaborately discusses the instrument with main and polar arms of equal length, and gives a constant area which is correct for that special proportion and that only. Prof. J. Burkitt Webb, who had made a special study of the subject, called attention to several demonstrations, including one of his own. The demonstration of Prof. C. Cullmann, in his work on Graphical Statics, published in Zurich in 1875, is very complete for all proportions. Professor Webb in his own discussion points out that if a line drawn across the face of the recording wheel be extended past the pole, the perpendicular distance of this line from the pole will be the radius of a circle, of which the circumference represents the distance moved by the recording wheel when the tracer is moved through a complete circumference. All of the demonstrations were designed more to prove the general accuracy of the principles of the Planimeter than to popularize the knowledge of the same and develop rules for the construction and adjustment of the instrument, for which purpose it is thought the investigation herein given is best adapted.

Other articles on the Planimeter are to be found in Vol. VIII., Cosmos, Paris, 1856; Reports of the British Association, 1872 (by F. J. Bramwell); and in Williamson's Integral Calculus, London, 1880. Prof. Webb reports also that there is a book on the subject written by Chr. Nehls, a Hamburg engineer, and, during the meeting, has been kind enough to refer the writer to a demonstration given in Prof. Barnard's report on the Paris Exhibition of 1867 (Vol. III. of Reports, page 623), to another in Spon's Dictionary, and still another in the Journal of the Associated Engineering Societies, published last October in St. Louis. Articles on the subject in a pamphlet on Mathematical Drawing Instruments, by W. F. Stanley, 1878, and in Buff & Berger Hand-Book, were also referred to. The demonstration in Spon's Dictionary is limited like that of Prof. Hirn. That of Prof. Barnard appears to cover the principal features.

(*a*) $$ND = \frac{J}{\pi};$$

or when $J = 10$, as is customary,

(*b*) $$D = \frac{3.183}{N}.$$

(*c*) $$N = \frac{3.183}{D}.$$

SECOND CONDITION.— *When one end of the main arm is guided by a polar arm and the fixed point or pole is located within the figure to be measured.* In this case it is essential that the tracer, the hinge and the axis of the wheel be constructed in the same straight line, and there is to be added to the area indicated a quantity constant for the particular proportions of the instrument. If N = length of main arm, from tracer to hinge, as before, R = the distance of wheel from hinge, P the length of the polar arm, and A_2 the area to be added.

(*d*) $$A_2 = \pi (P^2 + N^2 \pm 2NR).$$

The minus sign, before the last term in parenthesis, applies when the tracer and wheel are both constructed on the same side of hinge, and the plus sign when they are on opposite sides.

To change an instrument, with adjustable main arm from one system of units to another.—Simply measure the wheel by the desired scale of units and calculate the length of arm frcm above formula, which will give the result in units of the new scale. The calculated length of arm may be multiplied or divided by multiples of 10 to keep within the capacity of the instrument, and the error thus introduced corrected by shifting the decimal points in the results. With a non-adjustable arm, multiply the reading by the square of the ratio of the new scale to that for which the instrument is adjusted.

TO ADJUST AN INSTRUMENT.—Simply put it in good mechanical condition so that the joints move freely and without shake, and

(*a*) When the pole is without the figure: Adjust the length of arm by calculation or by trial of a known area, so that the reading shows the correct area.

(*b*) When the pole falls within the figure : With length of main arm as before, adjust the position of the wheel with relation to the hinge, or change the constant, by calculation or trial, so that the difference of readings plus the constant will show the correct area.

A number of radius slips of metal of different lengths, may be used to guide the tracer in an exact circle for the purpose of testing

the instrument, or an exact rectangle can be cut in a piece of metal or drawing paper and used to guide the tracer around a known area.

When the main arm of planimeter does not make a complete revolution about the pole the operative principles of the instrument are as simple as those of a child's counting frame. It is therefore thought better to take up this special case first, and thus gradually develop a more general demonstration.

In Figure 197, let *a b* represent the main arm of a polar planimeter, *b c* the polar or guide arm, and *d* the recording wheel. Evidently, then, *a* is the tracing point and *c* the pole, or fixed point about which the instrument is revolved. First suppose the radius arm *b c* removed and the point *b* guided to move in a straight line,

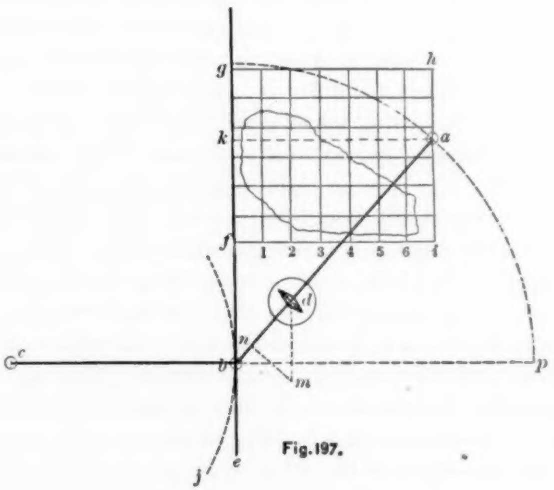


Fig. 197.

e g, which we will term the line of reference. This could be called the axis of ordinates or axis of *Y*, but, as will be seen hereafter, it may be run in any direction, and for temporary convenience only is represented as a vertical line. When the axis of the wheel *d* coincides with the line of reference *g e*, and movement is made in that line, evidently the recording wheel *d* will slip and not be revolved at all. If, however, the arm *a b* be deflected to any angle from *e g*, there must be, independent of the slip, a revolving movement of the recording wheel for every movement of the tracing point, *a*. If the arm *a b* be swung back and forth, about either of its ends as a center, evidently the revolving movement of the recording wheel *d* in one direction will be exactly neutralized by the reverse move-

ment, as the arm is brought back to the same position as before. So likewise (still supposing the point b to be guided to move in the straight line $g\,e$) the movement of the tracer a at right angles to $g\,e$ for a given distance in one direction, for instance, from f to i, will produce a certain movement of the wheel which will be exactly neutralized by a return movement, also at right angles to the line $g\,e$, through an equal distance, for instance $g\,h$, for the simple reason that the arm $a\,b$ is moved through the same angle in each direction.

If, therefore, the tracer be started at the point i and moved in the direction of the hands of a watch around the rectangle $i\,f\,g\,h$, the revolving movement of the wheel d, when tracer is moved to the left from i to f, will be neutralized during the equal movement $g\,h$, from left to right; during the movement $f\,g$ the recording wheel will not be revolved at all, and the whole effective revolving movement of the recording wheel d will take place during the movement of the tracer from h to i. During the latter movement the center of the recording wheel d will move in a line $d\,m$, parallel to $e\,g$, and the wheel d be revolved a portion of its circumference, measured by the line $m\,n$, at right angles to the axis of the wheel. For, evidently, the result would be the same as if d slid in the line of its axis to n, thereby not revolving at all, and then were rolled out at right angles to that line, from n to m. Any doubt on this point can be overcome by imagining the wheel d to be toothed, and the paper on which it rests to be an elongated rack with the teeth arranged, for the nonce, parallel to the line $a\,b$. But the line $n\,m$, representing the movement of the circumference of the wheel, is the sine of the angle $n\,d\,m$, with $d\,m$ as radius, and from similar triangles the movement of the wheel is proportioned to $a\,k$, the sine of the angle $k\,b\,a$, with the arm $a\,b$ as radius. But $a\,k$ also represents the departure of the tracer from the line of reference $g\,e$, so the rate at which the recording wheel d is revolved is proportioned to the distance of the tracer a from the line of reference; for instance, the distance $g\,h$ or $f\,i$, in this case, and also, evidently, to the distance the tracer is slid parallel to the line of reference, viz., $h\,i$ in this case. The total revolving movement of the wheel is therefore proportioned to the product of the two, or to the area, and the recording wheel d can be graduated so as to indicate the area, as hereinafter explained. Under conditions stated, the wheel should be graduated to show positive values when its top is moved to the right. If then the tracer, starting at i (seven units in this case from the line of reference), be moved to five, then up that line to

$g\,h$, and along $g\,h$ and $h\,i$ to the place of beginning, the wheel will first subtract five times $h\,i$, then add seven times $h\,i$, leaving a record of two times $h\,i$ as the area of the rectangle inclosed, or, if the vertical movement be reduced the area will be proportionately reduced. To the left of the line of reference $g\,e$ the upward movement will be positive, so an area lying on both sides of $g\,e$ will all be positive. The records are independent of the size of the rectangles, so any irregular area may be conceived of as broken up into small rectangles, each of which may be measured separately or all together by tracing the outline.

To find the proper proportions of the instrument, when operated as above, we use the following notation:

A = the actual area of the figure.

A_1 = the indicated area of the figure.

M = the movement of translation of the center of recording wheel, as distinguished from the revolving movement.

N = length of the main arm, $a\,b$.

R = distance of recording wheel from center, b.

D = diameter of recording wheel.

J = the number of divisions of circumference of recording wheel, each division to represent one unit of area.

Q = the actual revolving movement of the circumference of the recording wheel, d.

θ = the angle $k\,b\,a = n\,d\,m$ = the angle between the axis of recording wheel and the direction of the movement, M.

Then from the above we have:

$$Q = M \sin \theta \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

The actual area A = the departure from the neutral line, or $N \sin \theta$ multiplied by M, so

$$A = M\,N \sin \theta \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Substituting value $M \sin \theta$ from (1), we have in general,

$$A = N\,Q \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

The number of square units in the area must equal the number of linear units indicated by revolving wheel, $= Q$, divided by the length of one unit, or $\frac{\pi D}{J}$, hence

$$A = \frac{J\,Q}{\pi D} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

Equation (1) is true for all values of $\sin \theta$. When $\theta = 90°$,

$\sin\theta = 1$, $Q = M$, and $A = M N$. Making this special value of $A = A_{90}$ we have

$$A_{90} = M N = \frac{J Q}{\pi D} = \frac{J M}{\pi D} \quad . \quad . \quad . \quad . \quad (5)$$

$$N = \frac{J}{\pi D} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

$$D = \frac{J}{\pi N} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

When $J = 10$, as is customary,

$$N = \frac{3.183}{D} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

$$D = \frac{3.183}{N} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

For instance, for a main arm five inches long, the diameter of the recording wheel should be 0.6366 inch.

With above proportions and under conditions stated $A = A_1$. In general hereafter we make

$$A_1 = M N \sin\theta = N Q \quad . \quad . \quad . \quad . \quad (10)$$

until A_1 and A are proved equal under other conditions.

The above demonstration applies only when the point b is guided in the straight line $g\,e$, called the line of reference, which it will now be seen can be run in any direction, as it can be proved in the same way as before by constructing small rectangles in the figure to be measured, with faces parallel and at right angles to the assumed line of reference. The instrument for purposes already discussed will, however, be equally correct if the point a be guided in a straight line and the point b used as a tracer. This can also be demonstrated in the same way as before. The next step is to show that the end opposite the tracing point may be guided in any line, broken or curved, so long as the direct and return paths are identical. This follows from the fact that if the tracer a be guided in any straight line and the point b be moved back and forth in any line, for instance, the arc $b\,j$, with the center at c, the combined slipping and rotary movement of the wheel when moved in one direction will be exactly neutralized by contrary movements in the other, so, on returning both ends of the arm to the same point through the same paths, the reading of the wheel d would not be altered. Neither of the end points would enclose an area, but simply run on a line. So when one end, for instance b, traverses a

line, for instance the arc produced by the guide arm $c\,b$ with center at the pole c, and the other end traces an area, the latter only will change the position of the recording wheel upon returning the arm to the original position.

The above demonstrations apply only when the tracing arm $a\,b$ is obliged to move angularly back and forth in order to trace the area measured, without causing the arm $a\,b$ to make a full revolution around the point b, or the guide arm $b\,c$, to make a full revolution around the pole c. Under these limited conditions, all planimeters are correct which have the right relation of wheel diameter to length of arm, wherever the wheel may be placed, either in the line $a\,b$, or in the same produced, or parallel thereto, so long as the

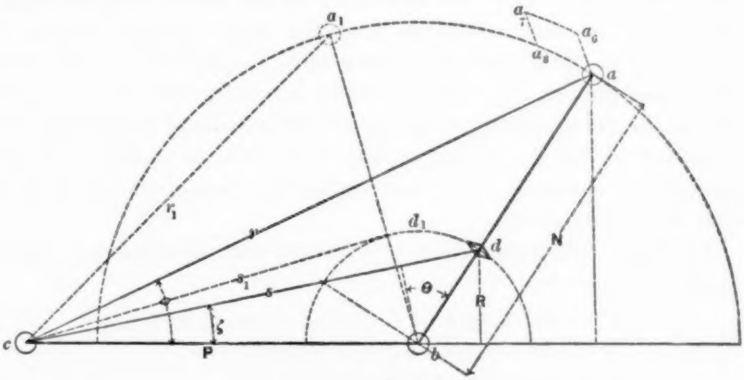


Fig. 198.

position of the wheel is maintained constant with relation to $a\,b$. This can be proved by simply running the line of reference through the axis of the wheel and proceeding as before.

If the point b be guided in the line of reference and one side of a figure with length $= f\,i$, be brought to that line, since the area is shown only by the movement at the extreme end, say in the line $h\,i$, as before explained, the average height of the irregular figure from the starting point i will be shown after tracing the figure by moving the tracer a from i toward h until the reading of the recording wheel is the same as at starting. This is the principle of the Coffin Averaging Machine, used chiefly for ascertaining the mean pressure of Indicator Diagrams.

Next suppose a planimeter guided by a polar arm to make a complete revolution about the pole c, which will then be located within the figure to be measured. In this case the tracer a may be guided

over the outline of any irregular figure within the limits of the instrument, which are fixed by the lengths of the arms and the adjustment possible by changing the angle $c\,b\,a$ from 0° to 180°.

Referring to Fig. 198, by inspection of any elementary figure, such as a, a_6, a_7, a_8, inclosed by arcs concentric with b and c respectively, it will be seen that the record produced by the direct movement of the tracer about b is neutralized on the return movement to the place of beginning or to any point equidistant from the pole c. The final records are therefore those produced by revolution about c, and if it can be shown that the area of every circle about c (within the capacity of the instrument) may be ascertained correctly with the instrument by tracing its circumference, the area of every sector will be shown by tracing its arc, and as every irregular figure may be conceived of as made up of an infinite number of sectors, with equal arcs but varying radii, the instrument will measure these sectors by simply tracing the perimeter of the figure. When the pole is without the figure, the sectors of larger radii will be added on the direct movement, and those of shorter radii subtracted on the return movement, so that this demonstration includes that previously given.

In Figure 198 note letters of reference and notation previously employed; also let

P = the length of the polar or guide arm $c\,b$.
s = the distance $c\,d$ in a straight line.
r = distance $c\,a$ in a straight line.
ϑ = the angle $b\,c\,d$.
φ = the angle $b\,c\,a$.

In general

$$A = \pi\, r^2. \qquad (11)$$

$$M = 2\,\pi\, s. \qquad (12)$$

Hence from (10)

$$A_1 = M\,N \sin\theta = 2\,\pi\,N\,s \sin\theta \qquad (13)$$

From the general polar equation of a circle, with c as the pole, $c\,b$ as the axis of reference and φ and ϑ as the angles fixing the directions of the radii vectores r and s respectively, we have

$$r^2 - 2\,P\,r\cos\varphi = N^2 - P^2. \qquad (14)$$

$$s^2 - 2\,P\,s\cos\vartheta = R^2 - P^2. \qquad (15)$$

In the triangle $c\,b\,d$, from the geometrical formula relating to a side opposite an acute angle, ϑ, we have,

$$R^2 = P^2 + s^2 - 2\,s\,(s - R\sin\theta). \qquad (16)$$

By letting fall perpendiculars from a and d, on $c\,b$ produced, we have by similar triangles

$$r \cos \varphi - P : s \cos \zeta - P :: N : R. \quad . \quad (17)$$

From (17) we have

$$\cos \varphi = \frac{P\,R - P\,N + N\,s \cos \zeta}{R\,r}. \quad . \quad . \quad (18)$$

From (15) we have

$$2\,P\,s \cos \zeta = s^2 - R^2 + P^2. \quad . \quad . \quad . \quad . \quad (19)$$

From (16) we have

$$s^2 = P^2 - R^2 + 2\,R\,s \sin \theta. \quad . \quad . \quad . \quad . \quad (20)$$

hence from (14),

$$r^2 = 2\,P\,r \cos \varphi + N^2 - P^2$$

From (18),

$$= 2\,P^2 - \frac{2\,P^2 N}{R} + \frac{2\,P\,N\,s \cos \zeta}{R} + N^2 - P^2$$

From (19),

$$= 2\,P^2 - \frac{2\,P^2 N}{R} + \frac{N}{R}\,s^2 - N\,R + \frac{P^2 N}{R} + N^2 - P^2$$

From (20),

$$= 2\,P^2 - \frac{2\,P^2 N}{R} + \frac{P^2 N}{R} - N\,R + 2\,N\,s \sin \theta - N\,R + \frac{P^2 N}{R}$$
$$+ N^2 - P^2$$

$$= 2\,N\,s \sin \theta + P^2 + N^2 - 2\,N\,R \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (21)$$

Hence from (11),

$$A = \pi\,r^2 = 2\,\pi\,N\,s \sin \theta + \pi\,(P^2 + N^2 - 2\,N\,R)$$

Compare (13),

$$= A_1 + \pi\,(P^2 + N^2 - 2\,N\,R) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (22)$$

which is the general equation of the planimeter for every possible proportion, when the pole c lies within the area to be measured and a is not entirely revolved about b. The latter qualification is rendered necessary for the reason that, so far, it has been assumed that the angle $c\,b\,a$ is changed only to vary the distance $c\,a = r$, and the modification in result due to a complete revolution of a about b has not yet been investigated.

In Fig. 198 draw s_1 tangent to the circle described by R; also draw the line $b\ d_1\ a_1$ at right angles to such tangent, and connect $c\ a_1 = r_1$. From the right angled triangles $c\ d_1\ b$ and $c\ d_1\ a_1$ we have

$$P^2 - R^2 = r_1^2 - (N-R)^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (23)$$

Hence—

$$r_1^2 = P^2 + N^2 - 2\ N\ R \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (24)$$

Comprising (24) and (22) it will be seen that the quantity to be added to the indicated area equals $\pi\ r_1^2$, the area of the circle generated by r when s is at right angles to the axis of the recording wheel at the bearing point of the wheel on the paper. When the recording wheel d is placed on the other side of the hinge b, in the line $a\ b$ extended, R becomes minus, which changes $-\ 2\ N\ R$ to $+\ 2\ N\ R$; so when all lengths are considered positive, as in the introduction, the area

$$A_2 = \pi\ r_1^2 = \pi\ (P^2 + N^2 \pm 2\ N\ R) \quad . \quad . \quad . \quad . \quad (25)$$

represents the correct number to be engraved on the arm of a polar planimeter, which number, as explained by the maker, is to be added when the pole is within the area to be measured. The statement of the manufacturers that this number should equal $\pi\ P^2$ is very erroneous. The correct constant will be found on trial to equal 0 when $N = R = P$; to equal P only when $R = \frac{N}{2}$, as in the universal planimeter hereinafter discussed, and it will always be greater than P when N is greater than $2\ R$. The constant may become negative when, as is improbable in practice, N is made less than R, and P, also of reduced comparative length.

It will be seen that in the first demonstration we had a *line* of reference; so in the above case, with the pole within the figure we have a *circle* of reference with radius r_1. All areas exterior to this circle are plus, and all interior, minus, and are so recorded by the instrument, so that the algebraic sum of the indicated and constant areas gives the correct area.

In general it may be stated that with instruments as ordinarily graduated all areas traced in the direction of the motion of the hands of a watch will be positive and those traced in the reverse direction, negative. Any included area may, therefore, be rejected by simply running in to it on a definite line, circuiting it backward and coming out to main boundary on the original line of entrance.

Finally, we will examine the modification in result when the

tracer makes a full revolution about b. We will suppose the point b to be fixed, as all movements about c have already been discussed. Under such conditions the only movement of the surface of the wheel d, in contact with the paper, will be in a circle with $b\,d = R$ as radius.

Hence, $M = Q = 2\,\pi\,R$ . . . . (26)

(From 10),

$$A_1 = N\,Q = 2\,\pi\,N\,R \quad \ldots \ldots \ldots \quad (27)$$

$$A = \pi\,N^2 \quad \ldots \ldots \ldots \quad (28)$$

When $A = A_1$ equating (27) and (28),

$$R = \frac{N}{2} \quad \ldots \ldots \ldots \quad (29)$$

Hence when one end of the arm $a\,b$ makes a complete revolution about the other, the only correct position for the recording wheel d_1 is at the center of the arm. Such a revolution of the arm cannot be made with instruments as ordinarily constructed, and is not desirable, but is necessarily discussed in fixing the proportions of a universal instrument. In all cases, when stops are provided so that the angle $c\,b\,a$ can be varied only from 0° to 180°, or through any less range, the general equation (22) and that for the constant (25) are applicable.

APPENDIX II.

A special investigation of the particular case when $R = N = P$ is interesting from the simplicity of the demonstration. Refer to Fig. 199, and use the notation previously given.

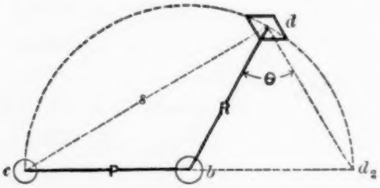


Fig. 199.

The wheel d (which also forms the tracing point) may be revolved about b to vary the distance $d\ c = s$ the radius vector or variable radius of revolution about the pole c. The momentary direction of motion of d about c is at right angles to s, or in the line of $d_2 d$, since the triangle $c\ d\ d_2$ is always inscribed in a semicircle; the axis of the recording wheel is in the line of the radius $b\ d = R$, so $\theta =$ the angle between these lines, and we have by inspection—

$$s = 2\ (R \sin \theta) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (30)$$

$$M = 2\ \pi\ s = 4\ \pi\ R \sin \theta \quad . \quad . \quad . \quad . \quad . \quad (31)$$

From (10),

$$A_1 = M\ N \sin \theta = 4\ \pi\ R^2 \sin^2 \theta \quad . \quad . \quad (32)$$

$$A = \pi\ s^2 = 4\ \pi\ R^2 \sin^2 \theta \quad . \quad . \quad . \quad . \quad (33)$$

Hence,

$$A_1 = A \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (34)$$

showing that this form of the instrument, with the limitations before named (viz., that $a\ b$ is stopped from making a complete revolution about b), gives correct results when a complete revolution is made about the pole, c. Moreover, it is to be observed that no quantity is to be added to the indicated area to produce the actual area, or, in this case, $\pi\ r_1 = 0$, as previously stated.

An independent investigation, by a different method, of the particular case where the instrument is completely universal, that is when a can be completely revolved about b or c, may be of interest.

Referring to Fig. 198, let fall on s a perpendicular from b, and designate the upper and lower segments of s_1 by g and f respectively. We then have for the special case:

$$N = 2\ R \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (35)$$

$$s = f + g \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (36)$$

$$g = R \sin \theta \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (37)$$

By Geometry,

$$R^2 - g^2 = P^2 - f^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (38)$$

and since s bisects N,

$$r^2 + P^2 = 2\,s^2 + 2\,R^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad (39)$$

From (38),

$$f^2 - g^2 = P^2 - R^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (40)$$

From (39) and (36),

$$(f + g)^2 = s^2 = \frac{r^2}{2} + \frac{P^2}{2} - R^2 \quad . \quad . \quad . \quad . \quad (41)$$

$$f + g = \pm\, s \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (42)$$

From (40) and (42),

$$f - g = \pm\, \frac{P^2 - R^2}{s} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (43)$$

$$g = \pm\, \frac{s^2 - P^2 + R^2}{2\,s} \quad . \quad . \quad . \quad . \quad . \quad (44)$$

From (37),

$$\sin \theta = \frac{g}{R} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (45)$$

From (10),

$$A_1 = M\,N \sin \theta = 2\,\pi\,s\,N \sin \theta.$$

From (45) and (35),

$$= 4\,\pi\,s\,g.$$

From (44),

$$= \pm\, 2\,\pi\,(s^2 - P^2 + R^2).$$

From (41),

$$= \pm\, 2\,\pi \left(\frac{r^2}{2} + \frac{P^2}{2} - R^2 - P^2 + R^2\right)$$

$$= \pm\, \pi\,(r^2 - P^2) \quad . \quad . \quad . \quad . \quad . \quad (46)$$

$$A = \pi\,r^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (47)$$

From (46) and (47),

$$A = A_1 + \pi\,P^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (48)$$

Therefore in this special case $r_1 = P$, as previously stated.

CLXXVII.

TECHNICAL TRAINING AT THE WORCESTER FREE INSTITUTE.

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In presenting an outline of the plan and operation of the Worcester County Free Institute of Industrial Science, especially in its relation to the training of the Mechanical Engineers, it is proposed, First, to refer briefly to the origin of the Institute. Second, to give its course of study and practice, by means of a chart showing the amount and distribution of the time devoted by the student to each study. This chart includes a scale diagram showing graphically the relative time assigned in the whole course to each branch or department, and is followed by diagram analyses of the several departments showing the course more in detail. Third, to add some explanation in reference to special features of the school training.

The name of this Institute at once attracts attention. While in some respects it is appropriate and descriptive of the character of the school, in other respects it is liable to be misunderstood. The adjective "Free" simply means that by a provision of the founder the Institute offers free tuition to residents of Worcester County, in which the school is located. Students from other localities pay tuition at the rate of one hundred and fifty dollars per year.

The adjective "Industrial" means that the training of the school is to be directed toward the promotion of the industries of the commonwealth and country, through the development and application of science to its manufacturing and engineering interests. It does not mean that students are to devote their time mainly to manual labor while in the school or after graduation. It is true, however, that the school originated in a plan and purpose that it should teach the practical applications of science, and that as a means to such an end there should be a department of practice in the Institute. So strong was the purpose to emphasize this feature that it was originally suggested that the name include the word practical, making the proposed name, The Worcester County Free

Institute of Practical Industrial Science. The fact that all industrial science is practical may have led to the omission of this adjective. This emphasis of the practical and industrial side, or of the adjectives of the name, tended to diminish the prominence of the word which they qualify, and gave to some the impression that the objects of the school could be attained by giving students mostly shop work and little science. But in the minds of those to whom the objects of the Institute were entrusted, the necessity of a thorough scientific course was clearly seen, while at the same time the determination that this should be accompanied by actual practice (which for mechanical engineers should be in a real working machine-shop), and that the whole aim of the school should be to prepare young men for practical efficiency in their chosen departments, was firm and fixed. The only problem was to administer the school in such a manner that these two elements should be blended in due proportions and in proper relations. The first dreams of the possibility of such an institution in Worcester were entertained by men who were unfamiliar with schools either of science or of any other department of higher learning. John Boynton desired to give one hundred thousand dollars to found a school free to youth of Worcester County, where studies could be pursued which were not usually taught in the public schools, and which should fit the youth for the practical duties of life. Ichabod Washburn had long cherished a desire to found and endow a machine-shop where young men could be taught in the principles as well as in the practice of mechanics, and could gain by an easier and shorter road, what he, as one of the pioneers in the wire manufacture, had struggled so many years to secure in the work of building up a business, involving the construction and use of complicated machines and delicate mechanical and chemical processes.

It was in the plans and ideas of these two men that the school took root. They were unacquainted with each other, unlike each other, and the union of their ideas and plans resulted in the establishment of an institution, which was independent in its origin, and could not fail to be unique in some of its features. While it was necessary and desirable that its course of study should include the subjects taught in other technical schools, yet the origin of this Institute and the special provisions made for actual practice and for the application of science in connection with its acquirement in the school made it imperative that special methods should be adopted and that the work of the school should be such as ulti-

mately to satisfy the demands of the leading practical men, in whom the school awakened much interest and hope.

With this brief introduction the interval may be passed over of nearly seventeen years since the school first opened, and an effort will be made to state clearly just what is now done in the Institute for a student who takes its course in mechanical engineering.

This department is selected because it is the one in which the gentlemen of this Society are primarily interested. It may be said here that the same general plan is pursued in all the departments, the practice in each being appropriate to that department, but thorough and practical.

The course in mechanical engineering extends over three and a half years. The other courses occupy three years. The preliminary half year for mechanical engineers is devoted largely to practical training in the shop, and was prefixed to the regular course in order to give more practice in actual shop work than the practice time of the three years could secure without impairing the efficiency of the general course or crowding out the more advanced work of the shop course.

To spare you the tedious repetition of the students' hour plan, a chart has been prepared, which shows at a glance the course of study and the distribution of the time allotted to each subject. For each recitation of one hour, there are reckoned two hours of preparation. Some lectures, and exercises in drawing, laboratory work and practice, do not require preparation.

Following the chart are pages giving diagram analyses of the departments of study and practice, sufficiently in detail to show their character and scope.

It will be seen from figures at the bottom of the chart (total hours per week) that the number of hours which the student is occupied is large, ranging from fifty-nine to sixty-eight hours per week. This is not all or nearly all study or close mental work.

Ten hours per week is practice: this practice always begins at 1 o'clock P. M. and closes the following day at 12 M. By this arrangement of the time the student having begun his practice for the week can use a machine without removing his work or being interrupted until the week's practice is completed, when the work is laid aside to be resumed the following week. The shop is thus full of students from Monday noon to Saturday noon of every school-week. This ten hours of work gives change of occupation and relief from study. Then there are from six to ten hours per week

devoted to drawing, and several hours of laboratory work, all of which is mainly an exercise of the physical or perceptive faculties, so that the time devoted to purely mental effort is reduced to five or six hours per day, for a student who faithfully devotes all the time assigned for the preparation of lessons. Some work as much as this, some get along with less, and some undoubtedly give more than the allotted time to the work.

No effects indicating over-work in general have been observed; on the other hand it seems that the discipline of steady and continued application is one of the good elements in the training.

The Institute requires hard work and close application, and thereby secures a class of students who propose to make their way by work, while those who want an easy time can seek elsewhere an easy road to knowledge. It is true, however, that there are evils arising from an over-crowded course, which should be guarded against, and it is possible that at some points it might be better to give a little more time for the work required. The department of practice occupies the largest area in the time diagram for the course. While this fact clearly accords with the purpose to make instruction in practice a prominent feature, it should not be inferred that its influence is detrimental to the thoroughness of the instruction in other branches. Of the 2,376 hours of practice, 800 hours are made in the Apprentice half year preceding the regular school course of three years, and 336 hours is summer practice made outside of term time, leaving but 1,240 hours which are taken up by practice in regular term time. This is much less than the time given to the study of mathematics or language during the same period, and only about one-sixth of the students' working time during the term. Moreover much of the work put down as practice, helps rather than hinders the theoretical studies. This is especially true in the latter part of the course, when the engineering problems which arise in designing, draughting and constructing machinery, are a direct stimulus to proficiency in applied mechanics and physics; to the acquirement of ability to use knowledge with good judgment, and a help to the persistent pursuit and study of the profession in years to come. In a word, the course in practice as arranged and carried out does not in any degree, all things considered, diminish the efficiency of the school training. From a study of the working of the system, for sixteen years, I believe that a higher standard of true attainment, and a better quality of mental discipline is secured than would have been secured without the shop department.

Bearing in mind then that the student has (outside of and unimpaired by the practice) a course of mental training in the Institute for three full years, let us inquire what are some of the results of his work in practice? As I have endeavored to show that much of the practice is helpful indirectly to the other departments, it is equally true that the school training is essential to the best results of the instruction in practice. The favorable influences under which the student works, the relation of his work to his mental training, and the systematic distribution of his practice through the course, must be taken into account in considering the claims which experience enables us to make in regard to the practical attainments of graduates. In regard to skill, it may be said that the graduate has learned a trade. Estimated from the most practical standpoint, that of his value to his employer, he is the equal in skill of the young man who has served an apprenticeship of three years, and this without counting his other attainments. He can put aside all pretensions to education, and taking his place in a machine shop, earn fair wages, say from $1 to $2 per day.

Because he *can* do this, it does not follow that he will. There has sometimes been an attempt to show that the result of the training of the Institute is to produce *mechanics*, because it has a shop and teaches students a trade, and that its graduates are inferior to those of other technical schools in other attainments, though everywhere, as far as I am aware, they are acknowledged superior in practical skill with machines and tools.

The work of this department of the Institute in training mechanical engineers, proceeds upon the assumption that a knowledge of machine-shop practice is the best foundation for success in mechanical engineering. The draughtsman, the designer, the inventor, the engineer, must go to the shop for the perfection, embodiment and execution of his designs, inventions and projects. To be ignorant of the possibilities and practices of the shop, is a life-long drawback and disadvantage. Therefore the first point at which the department of practice in the Institute aimed, the attainment which it insisted upon as necessary, and the object which it has accomplished, has been practical skill in machine-shop work. This attainment has been reached by a plan and means which were peculiar (in this country at least) to this Institute. The donation and endowment of a machine shop, to be a department of the School, and along with the shop the conditions which made it necessary that it should be a shop for real business, the provision of a super-

intendent, with fixed duties, to administer the shop in the interests at once of education and of sound practical training and the efficient support of men of liberal views as well as eminent attainments in manufacturing, engineering and professional pursuits, constituted an element in the plan of a technical school which was entirely new. Such a plan would not have been developed as the outgrowth of a school. The plan was the result of the perception on the part of practical men of the need of intelligent skill, obtained in connection with instruction. The Washburn machine shop has, in accordance with this original plan been a business shop. It has sought to produce the best machines of their kind, and to do thorough work in all departments. An examination of the last column in diagram No. 1, will show the range of work done. It will be seen that contracts for work involving engineering ability have formed a part of the business. In all this work, the standard of production has been just the same as that of any shop striving to build up its reputation and win success.

It has looked wholly to those best fitted by practical experience, to judge of the quality of its products. The shop does not depend upon the students to carry on the work. The students do not go from the school to help fill the orders of the shop. The students go to the shop to see how business is done, what standards must be reached, and to take a responsible share in the work.

The shop does not try to see how good a machine it can build with boys. It builds a machine to suit the rigid requirements of the purchaser and teaches the boys what these requirements are, giving them every opportunity and incentive to reach the highest attainments in their work.

The fact that a proper standard must be reached in every piece of work, that this is inevitable, and that not to reach such a standard constitutes a failure on the student's part, and causes his work to be rejected, puts a value upon the work, and develops in the student a feeling of responsibility and enthusiasm which is a great factor in his progress.

But the work of this department will be misunderstood, unless it is remembered that this administration of the shop on business principles is adopted not as an end, but as the best means of securing the objects of the department, viz., educating and training the students in the practical mechanics. There seems to have been much misconception in regard to this department of the Institute. It has been claimed that it degraded the training to make the shop a

money-making institution; that it is a mistake to combine education and business; that business cannot be successfully carried on with boys; that it has been tried and proved a failure; that it is absurd for a school to sell anything. These notions evidently arise from a misconception of the plan and relation of the machine shop to the school. It is easy to fall into the error of thinking that a school has undertaken, by getting a room and some tools, to accomplish the feat of instructing students and at the same time making machines which are sold at a large profit, because made by student labor which is not paid for. Nothing could be more impracticable than this, and nothing could be further removed from the plan and operation of the department of practice in the Institute. To be able to form an unprejudiced judgment of the Institute plan, follow me in your thought to a right point of view. Suppose yourself engaged in making a plan for a technical school having a department of mechanical engineering. First, we will suppose you have arranged the course of study. You can easily include what experience has shown to be best. When this plan is carried out you will have provided for the theoretical training. Now you believe that the engineer should have some mechanical practice; that he shall not be destitute of practical knowledge of machine-shop methods and mechanical skill.

You have a few hours per week of the students' time to devote to this practice. Let me suppose that there are two ways open to you, either of which you may choose, and that your choice is governed wholly by the consideration of the best interests of the student. The first plan is to appropriate a room and purchase machines and tools, which the students may be taught to use during their practice time, in learning the elements and processes of machine-shop work.

The second plan comes to your notice in this manner. It happens, for example, that your school is located close to the shops of the Messrs. Sellers, and they say to you, We have done business a good many years, for the sake of business, for the advancement of the interests of manufacturing and engineering, for reputation, and for the various emoluments open to a successful career. Now we are willing to retain our foremen and best workmen, to teach your engineering students during their practice time, and we will see that they are instructed in all the work of our business from the rudiments upward as fast as they can learn, and will conduct the business, not for the sake of making money, but for the sake of

having a good place for your students to work, where they can learn in the most practical manner, and be best fitted to fill such positions as are waiting them in the practice of their profession. Now which plan will you choose? If you reject Messrs. Sellers' offer, on what ground will you do it? Will you say, We should like to have our students familiar with the use of your tools and processes, we know they represent advanced attainments, but we do not like to have their work sold, and unless you will give up business, and only make pieces to be laid aside as samples or thrown away, we fear your practice would not do for students, and that the sale of your perfected and useful machines, would in some way render the practice unfit to be associated with a school.

If you accept Messrs. Sellers' proposition, you simply decide to send your students to their shops for practice, and this practice causes no more interference with the school work than would the time devoted to a well conducted gymnasium, or to any other department of school training. Whatever your final decision, you would undoubtedly be led to consider the nature and value of the two kinds of practice offered, and I think would see that practice in Messrs. Sellers' shops, while it contained every valuable element that is found in any shop-practice, also included many valuable features which are supplied by the business element, and which would be lacking without it.

In short, there is the same demand for a real business shop for instruction and practice for engineers as there is for a real human limb or a real hospital with actual patients, for a medical student, or a working laboratory for a chemist. The shop is needed to supplement the school work; to remedy defects which have been recognized in the school training. If it is a real shop bringing to the students real experience combined with good practical instruction, it will accomplish its object. The farther it is removed from this, the more will it be subject to the very influences which tend to perpetuate the defects which it ought to remedy.

There is a certain reality, vitality and life in real work which, to a student who has once felt it, makes the work of fitting useless pieces seem trivial and uninstructive.

The tendency of modern methods of teaching—especially in scientific studies, is toward the plan which the real business shop is able to carry out most fully: viz., combining the analysis and the theory with the practical illustration and application. This is a subject upon which the engineers of the country ought to have

definite opinions, and as schools are becoming indispensable for the training of engineers the demand should be strong that such training be based upon right principles, and should include the best methods. While a school may not be able to secure a shop like Messrs. Sellers', it can settle the question whether the shop shall *first*, be a place where business is done, in order that there may be something practical for the students to learn, or whether it shall be a place fitted with tools where only their use and the processes of shop practice are taught.

The Washburn machine shop is giving instruction in practice to 101 students each week. The size of the shop and the amount of the funds for its maintenance, limit the number which can be admitted to the Department of Mechanical Engineering. For several years the number has been kept within this limit by competitive examination for entrance.

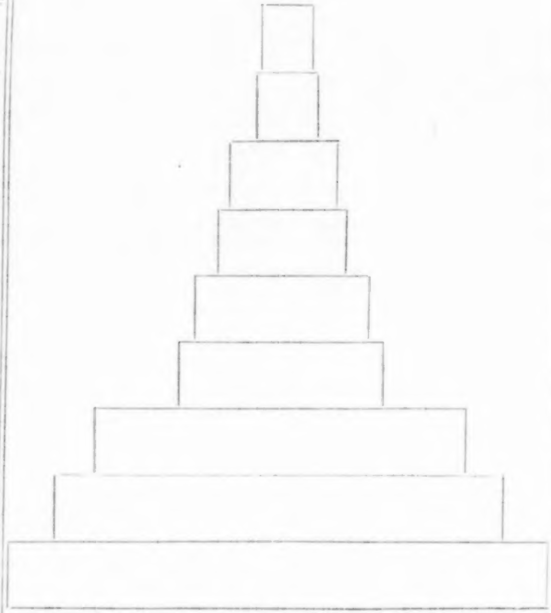
Practice in the departments of civil engineering and chemistry begins at the middle of Junior year. The nature and scope of the practice in these departments are shown by Diagrams No. 8 and No. 9.

The graphical diagram shows the liberal allowance of time given to language, as well as to mathematics and drawing. The means relied upon for making the three years' course most effective are, thoroughness of instruction in all departments; small divisions in recitation; the undivided devotion of the student to his school duties; long terms, or school years; and practical illustrations and applications of principles taught, by the solution of problems and by laboratory and shop work.

CHART.

SUBJECTS:	Hours per week devoted to study and instruction. App'tce half y'r.	Junior 1st half.	Junior 2d half.	Middle 1st half.	Middle 2d half.	Senior 1st half.	Senior 2d half.	Totals. (hours).
Mineralogy, Metallurgy and Geology.					3	3	3	180
Thesis Work....							10	200
Physics.........		6	6	3	4	6		500
Chemistry.......		6	8	3	3	6		520
Applied Mathematics.....						15	21	720
Drawing	10	6	6	8	8	6		880
Pure Mathematics		21	21	21	18			1620
Language.......	18	12	12	12	12	15	15	1920
Practice.	40	10	10	10	10	10	10	*2376
Totals per week.	68	61	63	57	58	61	59	

Time diagram for course.



SCALE.
1 lineal inch = 800 hours.

* This total includes 336 hours of summer practice.

Practice for students in the department of Mechanical Engineering.

Diagram No. 1.

		PROCESSES, TOOLS AND MACHINES.
Woodwork, Apprentice Class.	Bench Work.	Laying out work with knife and pencil, the use of planes, hand saws, chisels, squares and gauges.
	Turning and Sawing.	Use of power lathes and the tools for turning hard and soft wood, use and care of circular saws and scroll saws.
	Shaping, Boring, Mortising and Tenoning.	Cylinder, "Daniels," "buzz," planers, shaping and moulding machines, horizontal and upright boring machines, mortising and tenoning machines.
	Pattern making, Moulding and Casting.	Plain, square work, built-up work, turning, chucking and core box-making, core-making, tempering sand for moulds and cores, making cores and moulds, mixing, melting and pouring metal.
General Machine Shop Work. (Junior and Middle Classes.)	Bench Work.	Filing, chipping, tapping, reaming, scraping, and fitting plane surfaces and finishing with oil, stone and emery cloth.
	Lathe Work.	Drilling and countersinking, filing, polishing, hand-tooling and squaring up. Proper speeds for cutting metals, turning to exact size and calipering, cutting threads, squaring up and finishing nuts, chucking straight holes, reaming, inside boring, boring with boring-bars and fitting bearings.
	Drilling, Milling and Planing.	Drilling with speed lathe, upright and traverse drillers; use of Universal milling machine, milling nuts, bolts, head- and studs, cutting splines, fluting taps and reamers, and cutting gears. Instruction in use of planer, planing surfaces and bevels.
	Machine Screw Making.	Making machine bolts with revolving head screw machine, cutting up stock, making screws and studs and tapping nuts.
	Tool Making.	The correct forms of turning tools and the principles of grinding them; making taps, dies, reamers, twist drills, countersinks, counter bores, mills, milling machine, cutters, mandrels, boring bars, chuck drills, and centers, forging and tempering steel.
	Management of Steam.	Care of boilers and engine, including the work of firing, the care and control of the steam pressure and the water supply: also the care and manipulation of the steam pump and injectors. The practice in the steam department is under the constant oversight of the engineer.
Construction of special machines. (Senior class.)		A part of senior practice consists in draughting, setting-up and putting in operation, some machine of sufficient complexity to allow all the members of the class to take a responsible part in the work. This work differs from the general practice in this respect, viz.:—that it is especially the work of the senior class and not liable to be partly the work of journeymen. For list of machines constructed see next column.
Work in Draughting-Room. (Senior class.)		Making working drawings, blue prints and tracings for use in shop. Designing tools and machines.

PRODUCTS OF WASHBURN MACHINE-SHOP.

(Average annual sales for fourteen consecutive years, $15,374.51.)

Samples of frame-work, mortised and tenoned and dove-tailed, (2 weeks.)

Boxes, simple carved brackets, book-shelves, foot-rests and cabinets.

Patterns and core-boxes for casting the following pieces: plain platen, ribbed platen, flange coupling with recess and core, cylinder, cored cylinder with chambered core, slide rest with dove-tailed gib moulded with loose pieces, tool post with square core, oil cup cover with balanced core, two groove pulleys, one cored, moulded in three parts, white metal castings from each of these patterns.

Cabinet work, wall-cabinet, book-cases and writing desks.

Machinists' tools, engine and speed lathes, with hardened steel bearings.

Revolving head and screw machines, and hardened steel mandrels.

Chuck drills, bench centers, and lathe tools.

Emery grinding machinery of every description.

Hydraulic elevators, presses, accumulators, and general hydraulic machinery.

Drawing models, improved adjustable drawing tables, drawing boards and T squares.

All iron and wooden apparatus for chemical laboratories, lamp stands, concentric ring stands, test-tube racks.

Apparatus for physical laboratories, including the Willis system of apparatus for the use of lecturers and experimenters in mechanical philosophy, complete working machines illustrating the movements of the link and valve.

C. H. Morgan's machine, showing the correct forms for cams and their movements.

Twenty-five H. P. Corliss engine.

Ten H. P. upright, reversible engine.

Forty H. P. Buckeye engine.

Thirty H. P., high speed, straight line engine, 26″ engine lathe, 16 feet bed.

Diagram No. 2.

- Language.
  - English.
    - Grammar. — Syntax, composition, study of English and American authors, composition and oral reports on topics suggested by the works read.
    - Critical Reading. — One or more of Shakespeare's plays. Written themes relating to the play.
    - Essays and Debates. — Occurring at intervals through senior year.
    - Civil Government. — Constitution and government of the United States, with some study of the governments of England and France.
  - German.
    - Grammar. — Comfort's first book.
    - Reading. — German prose and drama.
    - Translation. — Written and oral both from German to English and English to German.

Diagram No. 3.

- Pure Mathematics.
  - Algebra.
  - Geometry.
    - Plane. — Demonstrations. Original work in solution of problems.
    - Solid. — Demonstrations. Solution of problems.
  - Trigonometry.
    - Plane. — Demonstration, transformations and application of the thirty-seven most useful formulæ.
    - Spherical. — Solution of spherical triangles.
  - General Geometry. — Thorough discussion of conic sections, constructions of curves, problems in foci, etc.
  - Calculus.
    - Differential: Whole of Bowser.
    - Integral: Whole of Bowser.

Diagram No. 4.

- **Applied Mathematics.**
  - **Theoretical Mechanics.**
    - Analytical statics and hydrostatics, with solution of numerous problems.
    - Dynamics and hydrodynamics, also with problems.
      - Including in general the contents of Bowser's Analytical Mechanics.
  - **Applied Mechanics.**
    - **Strength and Stability of Materials and structures.**
      - Strength and Deflection of beams, stresses in trusses, strength of axles and pillars, bursting strength of pipes, boilers and thick cylinders. Lectures on boiler construction.
        - Graphical Methods, (reciprocal diagram,) method of section.
    - **Kinematics.**
      - Rolling curves, link work, gearing, length of belts, motions and velocity of reciprocating parts of steam engines, etc.
    - **Dynamics.**
      - Conservation and transformation of energy. Work and efficiency of machines and motors. Theory of turbine and other water wheels and hydraulic motors. Tractive power of locomotive, effect and efficiency of brakes, energy of fly-wheels, and reciprocating parts of engine.
  - **Thermodynamics.**
    - **Air.**
      - General laws. Equations of principal curves from theory. Cycles. Application to problems in work and efficiency of an engine.
    - **Steam.**
      - Properties, expansion curves, empirical formulæ, action and work in steam engines.

Diagram No. 5.

Drawing.	Freehand.	Outline.	Blackboard work, plane projections, outlines and curves. Pencil drawings from geometrical curves and flat designs.
		Shaded.	Crayon drawings from models and casts; sketches of models finished in water colors.
		Perspective.	Sketches from real objects, mainly geometrical models or machines.
		Sketching from nature.	Two hours per week for a part of middle year, devoted to outdoor sketching of landscape, etc.
		Plane Projection.	Models and simple machines finished in water-colors, with construction of shadows, or finished with shade lines or line shading.
	Mechanical.	Isometric and Perspective.	Principles of Isometric and application to a simple drawing from model or working drawing. Instruction in laying out and executing a perspective drawing with shadows, etc.
		Machine drawing and Design.	Preparation of working drawings, working from specifications, designs of parts of machines, calculations of speeds and stresses. Gearing, lectures on steam engine construction.
		Thesis Drawing.	A finished drawing accompanying each thesis and retained by the Institute.

Diagram No. 6.

Chemistry.	Inorganic.	Non-Metals.	Two lectures per week for first half year.
		Metals.	Two lectures per week for second half junior year. Laboratory work two hours per week, second half of junior year.
	Analysis.	Qualitative.	Laboratory work and lectures two hours per week for second half junior and first half of middle year.
	Organic.	20 lectures first half senior year.	
	Metallurgy of Iron.	20 lectures first half senior year.	

Diagram No. 7.

Physics.	General Physics.	A course of recitations, lectures and experiments with mathemetical demonstrations and problems. This course ends with the middle of the second year.
	Applied Physics.	A course of lectures following the course in general physics and extending through one year. This course includes the applications of physics in the use of steam, gas, fuel, and electricity, and steam, hot-air and gas engines, heating, ventilation, telegraphy and the various recent developments of electrical science.
	Laboratory Work.	Practice (in small divisions) in the actual use of the testing machine in determining the tensile, crushing and cross-breaking strength of wood and metals. Transmission and brake dynamometers, steam engine indicator, dynamo-electric machines and various apparatus for accurate measurements and for determination of efficiency of steam boilers, electric motors, etc.

Diagram No. 8.

Practice in Civil Engineering.	Juniors.	Study of works on plane and topographical surveying, have practice in field-work with compass, transit and level, in making triangular and topographical surveys, establish street and lot lines and make the plots of their surveys.
	Middles.	Study works on railroad curves and earthwork, run preliminary lines for a railroad, locate and cross section a railroad line, make maps and profiles of the lines and estimate the cost of constructing the located road.
	Seniors.	Study works on stereotomy and civil engineering, calculate the stresses for given loads upon roofs, highway bridges and railroad bridges, calculate the dimensions of the members and make plans for pile bridges, arches, abutments, piers and other structures.

Diagram No. 9.

Special Work in Chemistry.	Juniors.	Lectures and recitations on the metals, methods of qualitative separation, laboratory practice, qualitative analysis.
	Middles.	Chemical philosophy, 60 exercises, 180 hours. Lectures and recitations on qualitative methods. Laboratory practice, qualitative analysis.
	Seniors.	Abstracting articles from English, German and French chemical journals, 30 exercises, 90 hours. Organic chemistry, lectures and recitations, 60 exercises, 180 hours. Lectures and recitations on commercial analysis. Laboratory practice, special qualitative methods on adulterations. Thesis work.

DISCUSSION.

President Holloway.—The old, and ever recurring question is, What shall we do with our boys? and it is only equaled in the difficulty of its proper solution by the other question—How shall we proceed to make at least a portion of them mechanical engineers? This latter question is one which treats of their education in technical as well as practical knowledge, and in regard to the best mode of procedure, the ablest and best engineers have long disagreed. I do not imagine that Prof. Alden intended by his paper to do more than illustrate how they seek to combine teaching and training, or theory and practice, at the particular institute to which he refers, in preparing a young man to become a mechanical engineer if he can, or a machinist, if he cannot. Not being familiar with the course pursued at other and similar institutions, I cannot say whether the course prescribed in this paper is as good or better than others, but as we have representatives here from like institutions, it will no doubt appear from the discussion which will follow, and I will ask Prof. Webb to state his views in regard to it.

Prof. Webb.—I have no intention of giving my views as to the "Best method of teaching Mechanical Engineering" at this meeting, but will state something of the history of the subject and the object of bringing it up for discussion here. During the three years or more that I was in Europe, some four years ago, I spent considerable time visiting various technical schools purposely to see how the courses of instruction are shaped and carried out, and since my return, in connection with the American Association for the Advancement of Science, I have taken pains to secure for this subject the discussion which it merits. It was inaugurated last year at the Philadelphia meeting, and much more interest was taken in it than had been anticipated; although not reported, it got out enough to excite considerable favorable comment, and because the discussion was so interesting, we appointed a committee, of which Prof. Alden is a member, to solicit papers and views for presentation at our meeting at Ann Arbor next August. The object in bringing it before this society was to induce a discussion here, and inasmuch as many of you are also members of the American Association we can, without any committee or other formal process, have this discussion represented there as giving the views of the mechanical engineers of the country.

In talking with a number of practical men on this subject I have found that the general opinion is by no means one of entire confidence in machine-shop practice for the development of a mechanical engineer.

At the Worcester Institute they believe in the necessity of actual business in the shop; that, in order to make shop practice worth anything, a student must go into a shop which is engaged in making things for the market. The opposite to this is the Russian system, where, instead of taking a standard fixed by the market, we adapt both the design and quality of the things made to our purposes of instruction. Upon this point there is some difference of opinion, and upon it turns the whole organization of some of the mechanical schools of the present day. The Stevens Institute works one way; they make their course in the shop to subserve the instruction they wish to give; at the Worcester school they do it the other way. I would like to call attention to the division of this subject which I proposed at the Philadelphia meeting of the American Association and which was simultaneously made at the Social Science Association by General Walker. There are really three kinds of schools. First we have the *School of Mechanical Engineering*, where the object is to produce the engineer who must know all of the higher parts of the business. Next we have the *School for Superintendents*, who must be able to direct and control the workman and must know intimately all the processes; he does not need to know the higher parts for which he can depend upon the engineer. Then we have a third kind of school which is for younger students who have not yet chosen their business or profession, and the majority of whom will never engage in mechanical pursuits,—in fact, for all boys who are in public schools. That is called the *Manual Training School*, and it can easily be combined with or made a part of the regular public school. We all know that the French people are the clearest-cut in their language and ideas of any, and therefore two French schools have been chosen as examples. The *École Centrale*, founded in 1829, of which the celebrated M. Tresca, honorary member of this Society, is a professor, is emphatically a school for engineers. It is the hardest working school that I know of; the students are required to be there at half-past eight in the morning, and if they are not, they cannot get in without a written application, and they stay there until four in the afternoon with half an hour for lunch, besides which

the amount of study necessary at home leaves no time for the distractions so common in American schools. The École d'Arts et Métiers at Châlons sur Marne (there are similar schools at Aix and Angers) is for the instruction of superintendents. In the École Centrale there is no shop practice, but here seven hours per day is devoted to it, and instruction in mathematics, in drawing and in language is given as an accompaniment, which necessitates some evening recitations. Young men come out of these schools either as blacksmiths, moulders, pattern-makers or machinists,—mostly the latter—and are qualified for positions in the draughting-room or as foremen. Some schools, where they make things for the market, consider that the money derived from their sale is a great argument in favor of that system; now this school at Châlons gets 400,000 francs from the government every year, and sells what it makes, but the selling is entirely incidental. They sell the things they make to get rid of them, and without falling into the error of supposing that student labor can be made to pay, and they get but 25,000 francs back from what they sell. The difference between these two schools will be better appreciated when it is added that the graduate of the school at Châlons is not prepared to enter the École Centrale.

Now, here we have in France examples of the first two kinds of schools, and this country probably furnishes the best examples of the third, the manual training school—there being a very prominent and successful one at St. Louis in connection with Washington University. The object of these schools is to cultivate the hand and eye, teach the use of tools, give healthy and interesting exercise, and show the student the applications of the principles laid down in his books;—it is a revulsion from the excessive confinement and study of text-books that we have had in our public schools. But we want to consider here the first two kinds of schools, and the broad distinction between them should be kept in mind, the *School of Mechanical Engineering* and the *School for Superintendents.*

There is one other point of distinction which I wish to emphasize, namely, between *mechanical laboratory practice* and *shop practice.* In a school of mechanical engineering, if we admit that the student must know something about the manual exercises in the shop, and even that he should acquire some manual dexterity, that is merely shop practice, but, besides that, he must have mechanical laboratory practice,—he must learn, for instance, how to test a steam boiler. These are essentially different things; one

carried to the extreme will give you the machinist, the other is absolutely necessary for the mechanical engineer. This distinction should be kept in mind in considering all courses of instruction that may come under your notice.

With these words, as an introduction, I leave to you the discussion of the "Best method of teaching Mechanical Engineering;" what we want is to get the opinion of the practical engineers of the country, and I do not fear but that it will be on the right side.

Prof. Thurston.—To give point and clearness to the discussion on this interesting and valuable paper, I would suggest that what we would specially like to know would be the answers from the different members to the following questions:

1. What constitutes a correct system of education for the Mechanical Engineer?

2. Comparing the system of "*Construction*" practiced at Worcester with the so-called *Russian system* or "*Instruction*" system in use elsewhere, which is the best as a course of manual training for the engineer?

3. What constitutes a correct course of instruction for the "Manual Training School," pure and simple, as distinguished from the School of Engineering?

4. Which of the two courses above referred to is best for the latter school of instruction?

5. What constitutes the difference between the "Mechanical Engineer" and the "Mechanic?" What between the so-called engineer and the shop-man?

6. To what extent is it advisable to carry on the two courses of instruction of the school of mechanical engineering, and of the manual training school, in the same institution, and in the same shops?

7. Sketch out a course of instruction that would seem to be practicable for each case, indicating number of hours, per week and per annum, and total length of course considered practicable and advisable, explaining at length and in detail.

Mr. Kent.—Prof. Webb says he had no doubt that the verdict of the mechanical engineers on this subject will be the right one. I have no doubt that that verdict will be both right and wrong. The whole subject of the education of mechanical engineers is comparatively a new one. It has not yet got down to a foundation upon which to build exact opinions.

I think that the radical and fundamental criticism to which the paper is liable, is that the author makes a mistake of calling the Worcester school a school of mechanical engineering at all. He speaks in the paper of the training of mechanical engineers, and says that the course of mechanical engineering extends over three and a half years. Looking at the diagram on page 10 you find a graphic illustration. There are 2,376 hours out of about 9,000 devoted to shop practice. Mineralogy, metallurgy and geology are at the top of the diagram—they are useful things for mechanical engineers to know, but they are not a necessary part of mechanical engineering. Physics is only what a man ought to know before he is ready to study mechanical engineering. Chemistry—there is none of it necessarily in mechanical engineering, although it is well for a mechanical engineer to know something of it. Applied mathematics—probably about half of that is mechanical engineering. Drawing—not over half of that is really mechanical engineering, as free-hand drawing is included in the course. Pure mathematics is not mechanical engineering, but what is necessary before a man can begin to study mechanical engineering. It is a preparatory study. Language is certainly not mechanical engineering, although it would be well for mechanical engineers to understand the English language so that they could write an intelligible report. And finally, the shop practice given at Worcester is not mechanical engineering. So there are only about 1,000 hours out of 8,000 hours that are given to mechanical engineering.

The time given to practice is 2,376 hours, or the equivalent of 8 hours a day for 300 days, or one year. In that time, the paper says, the students learn a trade. This is a very important matter. Prof. Alden says, and I do not dispute it, that "estimated from most practical standpoints, that of his value to his employer, he is equal in skill to the young man who has served an apprenticeship of three years, and this without counting his other attainments. He can put aside all pretensions to education, and taking his place in a machine shop, earn fair wages, say from one dollar to two dollars per day." If that is a fact it is a first-rate reason for the existence of the Worcester school. If it is possible to take a boy of fifteen and give him one year's apprenticeship and turn him out a machinist, then the Worcester school is a grand school. If the Worcester school can do that, educating mechanics and workmen who can make their two dollars a day, then it is a

very important fact and worthy of our attention. But this is not mechanical engineering.

"What constitutes a correct system of education for the mechanical engineer?" asks Prof. Thurston. If a man is going to follow the profession of mechanical engineering in after life, he will necessarily have to acquire after he graduates certain things which he cannot acquire in the schools. He will have to know something about blacksmithing, something about moulding, about how to make his mixtures of iron to get a certain result, how to put work through the shop with the least expense. These are questions of practice, and you may say that after putting a man through the best school it will take him ten years longer to get such experience as will enable him to pass before the world as a creditable mechanical engineer. But the correct system of education for a mechanical engineer refers, I understand, to the education in the college, and I submit that the education he should get there is not an education of practice, of how to do a thing well, how to turn a shaft round, how to put a piece on the planer, and all that sort of thing, but the fundamental principles on which mechanical engineering as a science is based. First and foremost is a knowledge of mathematics up to the calculus and through the calculus, a knowledge of the principles of physics so far as they relate to mechanical engineering, a knowledge so thorough that it would make it impossible for a man to conceive of such a thing as perpetual motion. That cannot be learned in the machine shop.

Let him study in the college the theoretical principles which he cannot obtain in the machine shop, and not use the valuable time required for this purpose in attempting to get in the college that practice which he can best acquire in the shop after he graduates.

It was said during the discussion of this subject at the Philadelphia meeting of the American Association for Advancement of Science, that Chauncey Rose left a large sum of money to found an engineering school in Indiana, the condition of which was that the salability of the article produced by the student should be the test of his standing; that the student should make an article for sale in the Institute and should get his mark accordingly; and I made the statement then that Chauncey Rose might have made a better disposition of his money. There is no greater fallacy introduced into this subject than that of attempting to make a business institution out of a school. The system of the

Washburn school for the purpose of training workmen I think is much worse than the Russian system with all its defects. You might as well in teaching literature gauge the marks a student should get by the salability of a magazine article which he might produce, as to expect him to turn out a piece of work equal to the products of modern machine methods.

Most boys go to college because their fathers send them. They go at too young an age to know what they go for, and perhaps one-half or one-quarter of them make a mistake in going to the particular college they do go to. I know at the Stevens Institute that many of the boys taken in there at sixteen years of age, do not know whether they have any taste for engineering, and I know that after graduating some of them proved a failure as mechanical engineers and went into other professions.

Being recently called upon to give an opinion as to the course of instruction to be pursued in a technical school attached to a university, in which school good facilities existed for courses in both mechanical engineering and machine shop practice, I recommended the following:

"A division of the course into two parts at the end of the sophomore year; all the students to have the same course of instruction in the freshman and sophomore years, without any optional studies; and this course should be little, if any, different from that pursued in the departments of Civil Engineering and Architecture. At the end of the sophomore year the student's record during the two years, his examination at the end of the second year, and his own tastes, would indicate whether he was then fitted to pursue a course in the higher branches of mechanical engineering, such as theoretical study of the steam engine and other prime movers, the designing of machines and other engineering constructions, and the management of manufacturing establishments, or to take a thorough course in machine shop training, which would include extensive practice in the draughting room, the pattern shop, the foundry, the blacksmith shop, and the machine shop, with the view to making him a good mechanic, and to prepare him to become after a few years' experience in the outside world a foreman or manager of a machine shop or manufactory of specialties in wood and iron.

"I recommend this division of the course at the beginning of the Junior year, first, because few students will be found who have natural ability of such a character as to enable them to

become both expert mechanics and good mechanical engineers, and, second, because, if such men could be found, the four years' college course is too short to allow of their receiving sufficient instruction in both branches. If it is attempted to give the mechanical engineering students much training in the shop, they will not be able to devote sufficient time to those branches of study which require the application of mathematics and an extensive knowledge of the principles of machine design. If, on the other hand, the machine shop students have too much of their time taken up with theoretical studies, they will not have enough time in the shop to obtain a proper shop training.

"There will be no disadvantage to the student in after life, provided he has made the proper selection between the two courses, in preventing him from attempting to become both an expert mechanic and a professional engineer at the same time, because in after life he would be compelled to select for his daily avocation a special branch of engineering, and could not become expert in all branches. There is probably no better time in life for him to select the trade or profession which he intends to follow than at the beginning of his Junior year."

The following was proposed as a provisional scheme of instruction, subject to revision.

Freshman and Sophomore Years. All Students.

MATHEMATICS.—Finish differential calculus by the end of the sophomore year.

FRENCH AND GERMAN.—With special reference to technical translation.

FREE-HAND DRAWING.—Only so far as to give facility in sketching, and a knowledge of the principles of art so far as they may be applied to engineering constructions.

MECHANICAL DRAWING.—Including the use of instruments, projections, shades and shadows, perspective and isometric drawing, line shading, tracing and blue print work, and drawing from copy.

CHEMISTRY AND PHYSICS.—Lectures and text-book work.

SHOP WORK.—Elementary instruction in carpentry and pattern making, the uses of machine tools, bench work, foundry work, blacksmithing, pipe fitting, etc. The course up to end of the sophomore year being strictly one of instruction, after the Rus-

sian system, or a modification of it, in which the learning of how to do work is the object in view, and not the making of any product for sale.

Mechanical Engineering.—Lectures and instruction in the materials of engineering and machine-shop practice.

Junior and Senior Years.

a. Course in Mechanical Engineering.

Mathematics.—Integral calculus, and application of same to mechanics.

Mechanics of Engineering.—A thorough course in Weisbach or equivalent text-book.

Kinematics.—Reuleaux, or equivalent text-book.

Physics.—Laboratory practice, with special reference to the mechanics of fluids, heat and electricity.

Chemistry.—Laboratory work in inorganic, with reference especially to metals and their ores.

Mineralogy.—Determinative, with blow-pipe analysis.

Geology.—Elements of economic, with reference to fuels, ores of metals, fire clays, etc.

Lithology.—Elements of practical.

(These last four subjects may be optional in the senior year for students who wish to study mechanical engineering as applied to metallurgy. Physical laboratory practice in electricity, and kinematics, may be omitted by such students.)

Mechanical Drawing.—Difficult problems in drawing, designing of machinery and plants, and original problems. In this course the aim should be not to make expert handlers of instruments, but men capable of doing original and varied work on the drawing board, and of laying out work for execution by other draughtsmen.

Shop Work.—Carrying into practice in the shop the constructions studied in the drawing-room, and studies of the work being done by the students of the machine-shop course, the idea being not to make the students expert with their own hands but to instruct their heads, so that they will know the difference between good and bad work done by others.

Mechanical Engineering.—A course of instruction under the head of the department in the general subjects of engineering design, the theory of prime movers, the mechanical theory of

heat, steam boilers and engines, work in the mechanical laboratory in tests of materials and other experiments, and lectures on metallurgical plants and methods.

CIVIL ENGINEERING.—So much as relates to building materials, foundation and the theory of strains in bridges and roofs.

MARINE ENGINEERING.—Lectures or other instruction in naval architecture and engineering, and marine practice.

b. Course in Machine Shop Practice.

MECHANICAL DRAWING.—Such a course of instruction as should make the student expert in carrying out ideas originated by others, including so much knowledge of the strength of materials as is necessary to correct designing—also to make him a fine workman with draughting tools.

SHOP WORK.—A thorough apprenticeship in the machine shop with not less than five hours' work per day for six days in a week, the shop to be carried on according to the best outside machine-shop methods, with the exception that the end in view must not be the obtaining of the maximum profit from the work of the shop, but the maximum of instruction given to the apprentices. The work should be of as varied a character as possible.

MECHANICAL ENGINEERING.—Attendance upon such lectures by the head of the department as may relate to machine-shop practice without involving the application of the higher mathematics.

"Throughout the whole four years' course, both in mechanical engineering and in work-shop practice, all students should be required periodically to write essays or reports on subjects connected with their work, which are to be criticised by the head of the department as to both matter and manner."

Mr. Couch.—The second and fifth of the queries proposed by Prof. Thurston seem to admit of being considered to some extent in connection with each other.

Referring to the fifth, the difference between the mechanical engineer and the mechanic is very briefly, and perhaps pretty fairly, indicated by the statement that it is the province of the former to design and of the latter to construct.

Intelligent design requires a knowledge of the processes of construction. The shop practice, through which the engineer pupil acquires such knowledge, confers also upon him a certain amount of manual skill, which is, however, incidental and of secondary importance. The engineer pupil, for instance, who labors to pro-

duce true planes and accurate angles with the chisel and file, is gaining little if anything except manual skill, and is wasting his time.

The mechanic pupil, on the contrary, may profitably devote time to any work which will increase his personal ability to execute.

Intelligent *knowledge* of the processes suffices for the engineer; ability to *perform* them skillfully is required by the mechanic.

It appears to me, therefore, that of the two systems of training (second query), that of "construction" is better adapted to the wants of the former class of pupils, while that of "instruction" may perhaps possess some advantages for the latter.

Mr. Sweet.—There is this that ought to be considered in regard to the Worcester school. It was one of the first started in the country, and it still exists. For the last five or six years I have been called upon by young men to know where I would recommend them to go. I recommend them almost universally, if they have already been through a machine shop, to go to the Stevens Institute; if they have not, to go to the Worcester Institute.

I believe that instruction of the class given at Worcester will produce students, a large proportion of whom will make valuable men, while the instruction such as followed at Stevens will produce a few brilliant ones.

It is probably true that the instruction at Worcester is not such as is necessary to produce mechanical engineers, and the question may well be asked, Is it advisable that it should be? Has the experience of the profession been such as to justify the belief that there is a demand in this country for men to practice mechanical engineering as architects practice architecture, or any demand for a college to educate them?

If it takes the natural gift and ten or fifteen years to make a mechanical engineer, the man who has not the natural gift, who spends four years in purely theoretical training, wastes his time, while spent at such a school as Worcester he has his hands to fall back on.

Mr. Partridge.—When a man enters a shop with the intention of producing something, the first question which he naturally proposes to himself is, What are you going to make? The first question which we should ask in discussing the subject is, What are we going to make? When that has been settled, we may with propriety consider the best process for making it. By the nature of the ques-

tion proposed it is taken for granted that we wish to turn out mechanical engineers, but would it not be proper to broaden the subject a little? There are numbers of people in the world who have a mechanical turn of mind, intended by nature for big men, from whom it is not best to make mechanical engineers, yet there are some of these very men who having their head full of mathematics are willing, and even wish, to be mechanical engineers. That brings us to a definition of what can be made out of our boys. Prof. Thurston calls attention to the mechanical engineer and the mechanic. I would cross out that word mechanic, because there is no question in any one's mind in regard to the best method of making one. The task seems to be easy. The Worcester school can turn one out in eighteen months, if the man furnished is the right kind of timber. But, as a rule, we are not interested just now in the production of this class. The question of what we want to make is very much influenced by that other question of what the boy is good for. This can be decided in very much less time than two years. We can do as a railroad superintendent of my acquaintance does. When he gets a boy who promises well and he thinks he will put him through the railroad mill, he sends him down to New York to a professor in whose skill long experience has given him confidence. A report is sent back by mail. The young man knows nothing about it beyond the fact that he carried a letter to a certain address on Broadway, that an examination was made and he came away.

Mr. Kent.—Is he a phrenologist?

Mr. Partridge.—Yes, he is a phrenologist; and while you may laugh at the popular notion that the bumps on your head or the mosquito bites on your scalp indicate the quantity of brains in your skull, it is no longer a question of belief as to whether a man's mental capacity and capabilities for different kinds of work can be ascertained by a physical examination. While you or I may not be able to do it, it can and is regularly done by those who are skillful in that line. One well versed in the science would go through this assembly and tell as accurately as could a composite photograph the character of every man and the kind of work for which he is best fitted. But then, if your faith is not quite up to that, try the young man for two years at school, and find out in that time just what you might have learned in two hours had you chosen to do so. The mechanical engineer is a professional whose work is pretty well defined. The mechanic is

not a professional man, and hence his education need not be discussed at present; but there is another professional whose field has not been clearly outlined, but who belongs to the same great class as the mechanical engineer. In a vague way they are trying to make this man at the Worcester school and in the Russian school. The work is going on much in the way that Lincoln's blacksmith handled the steel. We think we will make an axe. If the material is not as good as we had hoped, we will try and make a clevis, and finally, if that fails, we as a last resort take a good heat on him, put him in the water and make a "fizz." We started to make an engineer, and end by putting him into the shop to be chief engineer of a drill press or bolt cutter. With good material, however, there is a profession which may be reached through the manual training school. There is required in it both skill of brain and of hand. The finger skill must be as great as that of the mechanic, while the mental training should be quite as careful as that of the engineer. Men of this class need a broad, liberal training, both mathematical and physical. I may mention as examples of this type such men as Alvan Clark and his sons, Ross and Dallmeyer of England, and Morrison of this country. They all happen to be optical instrument makers, but they are the best illustrations of the type of men of whom I speak. The profession, never having been named or defined, may best be described as one which calls for mental and physical aptitude for mechanical work combined with high mathematical and scientific knowledge. Men of this kind are scattered all over the country, and occasionally one gets an education which enables him to carry his manual dexterity to the highest attainable point.

The mechanical engineer needs an education of an entirely different character. He does not require the skill of hand, yet he should know quite as well as the man just mentioned how metal is planed and shafts made round and straight.

He must understand the practice, even though he could not put on the overalls, center the shaft, put it in the lathe and turn it up. In other words, he requires a theoretical acquaintance with the details of practice.

How the two classes of men are to be treated was well illustrated by two young men who came into the hands of this same railroad superintendent. Both were sent to the New York professor, who reported that one was born to handle men and could never be taught to do a good job—would always be a botch workman, and

that the other was a born mechanic, capable of doing any work calling for skill. Now what happened? They went back to their city, and both were put into the machine shop to learn the trade. The first, or botch, as he was called, was put through the whole course in from twelve to fourteen months. The superintendent said he had learned enough of the trade to handle men and know a good piece of work when he saw it. In eighteen months he was put in charge of a gang of men. The other man was given three years in the shop, and when he learns the trade his promotion will not place him over a gang of men. In other words, the superintendent gave his born foreman, who had a little mechanical inclination, just knowledge enough of the tools, shop slang and methods of work to enable him to take charge of men and work.

In one sense this man resembled the mechanical engineer who was called upon to prepare and oversee and plan, not actually to execute. The training of this class of men properly falls into such a school as my friend Kent has spoken of. The training which the other man ought to have had, supposing that both are of equal ability, has not yet been outlined. Worcester does not give all that is needed, since it lacks in breadth. Here is our friend Emery who has spent years in acquiring his education, which is of a very special and technical character. Such men as Alvan Clark of Cambridge, Brashear of Pittsburgh, or the late Mr. Sexton, are forced to get a special education for themselves. When we consider the requirements of an education of this character, combining, as it does, the highest mental training with great manual skill, the disagreement, the "pulling and hauling" and misunderstanding between the practical and mechanical engineering courses disappears. We see that there are two distinct lines, needing very different courses of instruction and drill. As I understand the question, we are not called upon to discuss the best method of educating a man who ultimately turns out to be a machinist at $3 per day. They can be best educated in apprentices' schools. This class of men are too small or too poor to take the higher courses of which we are speaking. Measure them up and you will find them limited in their horse-power by a lack of grate area, calorimeter or cylinder capacity. The higher training is physically beyond their reach. In educating them the stimulus of producing work for the market with the least labor, best quality in the least time, is very necessary and beneficial.

In the education of the mechanical engineer and what we may perhaps, for want of a better term, call the engineer mechanic, that is, the men who are scientifically or headwise mechanics, and those who are scientific-fingerwise mechanics, we do not require this stimulus. They are spurred to rapid work by the necessity of filling contracts.

There has been a long and extensive trial in this country with manual labor training-schools, with which few persons are now familiar. The last of the manual labor departments were given up thirty years or more ago. They originated nearly a hundred years ago, and for fifty or sixty years did an immensely valuable service. In these schools the young men who were educating themselves had an opportunity to learn various trades and to produce work for the market. While this country was manufacturing but little, and the prices of all manufactured articles were high, these schools supported themselves, and thousands of men in the last generation got their education by means of the work which they were able to do in the school workshops, the student being paid for work performed at market rates. But it was found that as soon as manufacturing grew and became a great factor in the progress of the country, the schools were no longer successful. The students were between two fires. The desire to make money was constantly tempting them to neglect study and become good mechanics. On the other hand, the love of study influenced them to slight their work as much as possible. In the schools the ordinary English and classical studies were taught, and the students were prepared for colleges. The regular courses were not far different from the old classical college course. The competition of machinery and the difficulty of making a fair division between work and study, finally extinguished the manual labor features.

Wm. R. Jones.—It is very easy to see from the tendency of the arguments of two of the speakers who have preceded me, that they are not practical mechanics. My idea of a mechanical engineer is one who can not only design a job, but one who can do it himself in first-class style—a man who can create a machine. The power to do that is one cause why our people have made our civilization so far superior to that of ancient Rome. The term mechanical engineer, I think, should be dropped. Often when I hear the term mechanical engineer, I ask, is he a mechanic? We can realize that this capacity to create is the highest attribute that God can give to man. If I were to start out to make

a first-class mechanical engineer, I should want to know the habits, the thoughts, the ideas of the boy. If he had an intense mathematical mind, I would advise him to study astronomy and leave mechanics alone. If he has average brain power, with some ingenuity, logical reasoning and application, then I say you may be a mechanical engineer. The idea advanced by Professor Alden I fully approve of. There are hundreds of young men in machine shops that receive no training whatever, and who often make good mechanics—a very scarce article by the way. You take that class of young men and send them to school under competent men who explain every idea, and if they are not competent to make mechanical engineers, they are at least able to make good mechanics. I should say that that school was a good kindergarten school for mechanics. Ninety-nine per cent. of the foremen of machine shops make no effort to train the young men. Then after all you must take another view of the question. A gentleman who is a member of the commission appointed by Parliament to come over to the United States and investigate the reason why American mechanics were so much smarter than any on the other side—Mr. Mather—had a long conversation with me on the subject, and I said, "Will you now please give us your idea of the subject?" He said, "It is your great common school system that is the foundation of it." Professor Muir, of Cambridge, who was here last year, in speaking of the question of education, said, "You have no university in this country?" I said, "I beg your pardon; we have multitudes of them; there is one, the Edgar Thomson Steel Works, and they are all over the country." All the so-called universities are merely preparatory schools. Until a man goes into practical life and develops his talents, he is merely a scholar. You cannot get a mechanic in three years. I give ten years for a man to become a thorough mechanic. But you can give him that preliminary training which is all that any institution of learning in the country can give for any profession, law, medicine, or anything else.

Mr. Oberlin Smith.—I agree with Mr. Kent in regard to the wisdom of his plan of going on for a year or two and then separating students into two classes, getting a "survival of the fittest" members in each of the two. But I disagree with what he says about it not being necessary for a mechanical engineer to know the *How*. I think he should know first the *Whether*, then the *Why*, and the *When* and part of the *How*, leaving it for the mere mechanic

to know the rest of the How and part of the Why. It will not do for a mechanical engineer who may have numbers of men under him, and who may have to contrive new processes which must be commercially cheap, not to know the theory of his work and a good deal of the practice also. For instance, a student in mechanical engineering may learn in three minutes, *if* he has it properly explained to him, why a file when moved back and forth travels in a curved path at its ends instead of a straight line, while a boy put to filing without any theoretical instruction may be six months or a year in learning to run his file straight, and perhaps never knows in all his life why it does not want to run straight of itself. Gradually he learns how to regulate the relative motion and pressure of his left hand and his right hand so that the two movements of force are always equal, and then the file does move straight. I believe that a mechanical engineer must of necessity have shop practice of some kind, and the smarter he is, the quicker, of course, he will learn the theory and part of the manual skill necessary in all these practical operations. But as far as the additional skill, which is purely physical, lies, he ought not to waste his time in learning it. He ought perhaps never to work long enough at filing to learn to file absolutely straight. So with various other operations; he should not waste enough time on them to become perfect—such perfection requiring an automatic training of the muscles like that seen in the pianist who plays a difficult piece from memory while thinking of something else. He should simply know enough of the theory of these operations to guide the other men and to watch them. I agree with Captain Jones about its taking more than ten years for a man to become a machinist, and I think ninety-nine would be a nearer general average so far as my observation goes. I do not think there is more than one boy in twenty now entering our machine shops who ever becomes a good machinist, and he will usually take four or five years to become a skillful journeyman workman.

Wm. R. Jones.—When I came into the room I recognized my old friend, Homer Hamilton, Superintendent of the Youngstown Foundry, and he introduced me to his son. He had that son in his machine shop some three or four years trying to make a good mechanic of him. He pleaded and argued and entreated, and his mother prayed; but they finally became intensely disgusted, and the father said, "I wish you would get out of here—go away." The boy went off to New York and secured a position on the

press there, and to-day is a leading caricaturist on the *Judge* paper. I mention that as an illustration of the argument.

Mr. Stratton.—It strikes me that Mr. Partridge has struck the key-note of this whole business. In former times it was the common custom in cementing steel to take it after the process was completed and break it, in order to find out what portion of it was fit for springs, what for shears, and what for tools. As mechanics or engineers we certainly would not dream of taking that steel and making it into watch springs without knowing whether it was susceptible of the temper of watch springs, and so it is with our sons—if they have not the capacity, it is utterly useless to attempt to make machinists, much less mechanical engineers of them. If they have a capacity for science they will become scientists with greater ease than they will mechanical engineers. It strikes me that the prime factor in the whole business is knowing whether the boy has the capacity to become a mechanic or an engineer, and if phrenology can tell us, phrenology should be consulted on so important a subject as the selection of a profession in life. It is told of a noted capitalist on the Pacific coast that some years ago he sent one of his children to a well-known professor of music to be taught the art of playing the piano, and very shortly after he asked of the professor how his son was getting on with his music. The professor replied, "Not very well; he was sorry to say the child had no capacity." "What," said the capitalist, "has no capacity! why I will buy him one." It is a good deal the same with many of us who send our children to institutions of learning in the hope that application on their part will soon fit them for a pursuit for which they really have no capacity. We think we have the capital to send them through any course we may select, and the consequence is they fail. We see this especially demonstrated in the United States Naval Academy and at West Point. You will find that the boys who go through there most successfully are those that come from the public school with proved capacity, they generally passing a competitive examination before receiving the designation. Yet the gentleman who tries to force his son in and through either of these institutions almost invariably finds out that his son is a failure from lack of capacity. The same is the case in our iron works as well as in most of the institutions wherever we attempt to make a mechanical engineer or a scientific man of him who has not the capacity for this particular calling.

Mr. Dodge.—I am not a very old man, but I have had occasion to contemplate a great many men who were older than I was as mechanics, and it always has occurred to me that the capacity to decide on the factor of safety is really the important point in a mechanical engineer; the man who can make the most judicious selection of the factor of safety in all his operations is the best mechanical engineer. I believe that the matter of judgment or mechanical "horse sense" is the foundation which is the hardest thing to get at, but I believe that in the main the boys will develop that themselves, and, as a rule, will struggle into it. There may be fifty boys who may come into the ranks, and though they are not able to calculate what they want to do, they can verify their judgment. Of all the men that I know who are successful mechanics I would rather have their judgment than their figures. That matter of judgment cannot be put in by education and cannot be taken out by anything.

Mr. Albert Emery.—I think the last is a very pertinent remark. If a man has not got the common sense or the logic to see and to judge, he may make an engineer of the kind Mr. Kent has mentioned, who can go through all these formulas and give you beautiful theories of bridges and matters of that sort; but he will wreck any establishment that employs him if they keep him long enough. The man who would be a successful engineer must be a man who has common sense, and if a man under him presents to him the results of a theoretical computation which he has directed him to make, he must see at a glance in many cases whether they are substantially correct or not. If he says to a man give me the weight of that scale, and the man replies that weighs a hundred and forty pounds, he must be able to say, "I don't want any such figures; I know better; I know the scale only weighs fifteen or twenty pounds," and the draughtsman looks all over and says, "There is a mistake of a decimal point carried this way." The draughtsman has gone through the calculation and has moved the decimal point the wrong way. Now there comes in the rule of thumb; the draughtsman has made the computation and says it is so and so; but the other's common sense tells him that it is not any such strength or weight as the figures show. If this man is to be an engineer he must have mechanical ability, or he is not an engineer that I would care to employ. He should be so much of a mechanic that when he designs a piece of work, he knows whether the shop can make it, and when he designs a piece of work he knows after

it has been made whether it can be put together and taken apart. I have seen pieces of work designed that could not be put together after they were made, and pieces that when they were put together were very difficult to take apart. Now the mechanical engineer may be able in theory to make his formulas, and to show the relations of all these parts and the strains upon them. If he does not understand how to make those things in the shop, he may design a piece of work that is so expensive that no man would pay for it twice. That mechanical engineer who will be the most successful engineer must be the one who, when he designs these different parts, must so design them that they will be made well and accurately, if the thing is to go into commercial work, and he must be so much of a mechanic himself as to know how to turn those parts out quickly and uniformly. When the mechanic goes into a shop to work, if his training has given him so much of the engineering part that he can say how this tool can be changed to make six of these parts in a given time instead of one, and make them accurate, he is of so much more use.

Mr. Babcock.—We men who have attained to gray hairs, who have picked up in practice what knowledge we possess, have a keen appreciation of what education is, so far as we have received one. We have perhaps a keener knowledge of what it should be, by reason of our own deficiencies. I do not know that because we have educated one man up to a point where he can do a creditable piece of work, that therefore we are competent to say how other men should be educated. We are very apt to think that everybody should now be educated in just the same way we were; and in some sense that is a correct idea, because practice is the surest teacher. The young men whose hair is still untouched with snow have little idea of the facilities for an education which they possess in these days, compared to those which their fathers enjoyed. I was particularly struck with this thought in going through the educational exhibits at New Orleans the other day. The difference is very marked between the modes of education at the present time and fifty years ago, or thirty years ago for that matter. Now they are all tending to make better mechanics, for the word mechanic goes much farther than the machine shop. I think it goes into every department in life. The carpenter may be as good a mechanic in his line as the machinist. The cook may be and the housekeeper needs to be a good mechanic in her line. So

I say that the methods of educating the young at the present time have a tendency to make good mechanics in all departments of life, because they educate the hand as well as the head. But in regard to this question of the mechanical engineer, to use an old proverb which I suppose the younger generation have seldom heard, "you cannot make a whistle out of a pig's tail." You must have the material to make a thing out of or you cannot make it, and but one boy out of a large number has the proper material for making a mechanical engineer. If you attempt it with wrong material you may work upon the job as long as you please; you will probably "make a fizz" before you get through. Here is where I imagine is the greatest difficulty in this whole question—to know what your material is before you commence to work. You have all heard, I suppose, the saying of a wise man of old about training up a child. Now I read that a little differently from most people. They put the stress upon the "way." I put the emphasis upon "he," and read "train up a child in the way *he* should go;" and before you do that you must know the particular way in which he should go.

But no matter what the material is, it takes time. There is an inertia in mind as well as in matter. You cannot make a mechanical engineer in one year, nor in two years, nor in any number of years in a school. The school is simply the preparation, but it is a very necessary preparation, and one that cannot be dispensed with. But when the boys have been turned out of the school with all that preparation, they have yet to make of themselves mechanical engineers. The kind of preparatory education required depends upon the boy. One boy should go to Worcester. It would be a waste of money to send another boy there. One boy should go to Stevens, and I know plenty of boys who would be worse off for going there. "What shall we do with our boys?" is a problem, I suppose, which every man must decide for himself. What we are called upon to decide now, if we can, is what kind of a school is required to train the right kind of material to make the right kind of mechanical engineer. I am sure I do not know.

Mr. Hawkins.—Being one of the few gray-bearded members whom Mr. Babcock refers to as having fortunately had the privilege of educating, to some extent at least, one man; and having last year been obliged to decide the question for myself, what to do with a boy whom I desired to have converted into a mechanical

engineer (and whom I placed at Stevens Institute, whether for good or ill, as may hereafter appear), I gave the question considerable thought, about as formulated in the first query of Professor Thurston, with results somewhat as follows, which I think differ considerably from the views advanced by any of the gentlemen who have preceded me:

Mr. Babcock has, I think, struck the key-note of this question in his rendering of the maxim, "Train up a child in the way HE should go," etc.; but it has not, I think, been made so clear by him, or either of the other speakers, how this may best be done, as applied to the making of a mechanical engineer.

We will agree, I think, that no two boys are, in all the natural tendencies and aptitudes going toward the making of a mechanical engineer, alike. We find, indeed, in the same family, such a diversity of talent as would require a vastly different system of training of two brothers, even if naturally fitted for the same profession, and, in many cases, such as to preclude their both following the same profession successfully.

We find boys who have naturally such analytical minds, that the study of mathematics is little more than amusement, while they may lack that faculty called "horse sense" by Mr. Dodge, and the "finger wisdom" designated by Mr. Partridge, to such an extent as to make the acquisition of either uphill work with them.

Again, we find boys naturally endowed with this "finger wisdom," and that kind of generally good judgment that enables a man to arrive at a very close determination of the proper proportion and strength of a structure, or part of one, from mere inspection of a drawing, but who finds the study of the higher mathematics the most onerous of tasks.

We find, occasionally, a boy who has such an intuitive conception of Nature and her laws, and who, therefore, finds so much to attract him in the investigation of everything in the realm of physics, that he has no effort to make in the absorption of these branches of study, while these very branches—physics, chemistry, etc.—present the most difficult problems to boys of differently constituted minds.

Again, I do not believe that, to be a successful mechanical engineer, every boy should be possessed of exactly the same kind and quantity of learning. There may, I think, be as great a diversity among mechanical engineers, and all be good ones, as, for

instance, among doctors or lawyers. Doctors who follow some specialty closely generally become most celebrated, while some lawyers are best at criminal practice, others at patent law, etc.; and, certainly, if each of the two last-named professions is too extended in scope for one lifetime to suffice for its acquisition in all its branches, we may take the ground that mechanical engineering is not less extensive in its scope, and that the average boy would find three or four lifetimes too short within which to become such a mechanical engineer as would comprehend everything that could consistently be considered as coming under that head.

I believe we may have mechanical engineers perfectly adapted to many branches of the profession without a knowledge even of what the calculus consists of; while, for equally important branches, we may so educate others that, while up to the top notch in the pure mathematics, they may not require ever to handle a tool of any kind or know how they should be handled, both "finger wisdom" and "horse sense" being as much out of their line as a murder trial would be foreign to a patent-lawyer.

From the above consideration, it appears to me that Professor Thurston's first query, as formulated, cannot be specifically answered. There are too many qualifying conditions to admit of a categorical reply thereto.

I think that it would be a most unfortunate state of things, that every school of mechanical engineering should have the same set curriculum, so that we could not choose between several with considerable variation among their systems and methods; and that the existing state of things, in which Stevens differs from Worcester, and both of these from the Boston Institute of Technology (among which three I chose in the case of my own boy), and all the others among themselves, is the very best that could be had—unless, indeed, in looking over the various systems pursued, we could still further diversify them.

If a boy exhibits a strong mathematical and analytical mind, I should send him to that school which gave greater importance to physics and manual training, for the reason that he will absorb the former without effort, while he will require to be more thoroughly drilled in those things in which he is naturally less proficient. If he developed a strong mechanical aptitude, and was an intuitive physicist (and these two generally go together), while mathematics constituted a hill for him to climb with diffi-

culty, I should choose for him the school which gave the greatest prominence to the latter, with the assurance that he would acquire the former in any event.

These are the general ideas, Mr. President, which lead me to say that I believe that, in a country like ours, where so many schools of mechanical engineering are now in existence, and where so many more will exist in the near future, and in view of the great scope covered by the profession of mechanical engineering, the greater the diversity existing among their systems the better: and that, instead of our now trying to formulate some set curriculum for a school of mechanical engineering, to which all should conform, we should rather direct our energies to the grading of them, as it were, so that we may all, for our boys (whichever of their natural engineering gifts may predominate), find a "way" in which THEY may successfully "go."

Mr. Chas. E. Emery.—The paper is interesting and instructive. The methods stated are doubtless well adapted for the particular work attempted at Worcester, and embody some features which can well be studied in other institutions. Most of those who have spoken here criticise the prominence given to practical work in the Institution. My educational experience has been somewhat peculiar, as it alternated between periods of study and practice, the former with the direct assistance of schools in earlier years only, the latter in so many different and important directions as to require continual renewed preparation. Although I did not expect to speak at this time, and have never attempted fully to express my opinions on the subject of technical education, I have thought that those here present might desire to hear whether I would advise others to follow, in any respect, the methods which circumstances blocked out for me.

A man's success in life depends first on his natural capacity, and, second, on what he knows. The order in which principles and practice are acquired would appear to be of minor importance, so long as both are finally learned; but there are important advantages in completing the regular course of study in the schools, in comparatively early life, and then blending the practical participation and experience, which are necessarily a part of an education, into the professional practice which follows in due course. Abroad a man cannot attain a leading position until he is forty years old, and the custom is founded on sound principles. The majority of men do not attain proper habits of thought,

do not exercise good judgment, and are not fitted for leading positions until about that time—temperament and physical maturity varying the age somewhat. If young men would realize this law of nature, and make the best of their opportunities while in their business minority, they might succeed, in fact become prominent, in whatever branch they selected. If however a young man is expected to make himself useful in a profession, after a four years' course, the greater part of the period should be occupied in the study of principles, as little time will be available for organized study afterward. The habits of a young man become fixed early in life, and it is essential that he should then acquire habits of study, be associated with gentlemen and form acquaintances which will be desirable in after years. A young man who first starts in a shop to become a mechanical engineer will, on entering college, generally be embarrassed by habits acquired from the class of men with whom he has necessarily been somewhat associated, and find it difficult to conform to the usages of college life and the customs of society. A person who has entered on the practice of his profession with a limited education can afterward supplement it, but at enormous waste of time and energy. I am therefore very emphatically of the opinion that the technical college course should precede practical or professional work. Assuming this, we have to deal only with the question as to what extent practical work shall be introduced into the technical course in connection with the study of principles. All acknowledge that an engineer should know *how* work is to be done. Differences of opinion will arise only as to the necessity of the student's learning how to do work with his own hands in order to know whether or not others do similar work properly. There is no question but what young men should familiarize themselves with the tools and appliances used in their particular professions, sufficiently not merely to know their names and uses but the difficulties incident to their use, and the reason why one workman with a particular tool does better than another. The basis of all manufacture is hand-work. It is within the recollection of most here present, when all the various mechanical operations were carried on with tools largely operated by hand. Machine tools simply imitate the movements of the hand tools, although in a more rigid and satisfactory manner. A young man who thoroughly accustoms himself to original methods can fully appreciate the more refined ones of recent origin. To be a mechan-

ical engineer, a man must learn how to handle a hammer and chisel so as to do effective work without injury to the joints of his hand. He should be able to chip and file pieces of metal to a plane surface ; know how to drill them with the oldest and crudest appliances, and then be taught how to bolt them together and make joints to withstand permanently high pressures of steam, gas, water or other fluids. To one who can perform these various operations thoroughly and intelligently the use of machine tools will be simple. In connection with the hand-work above referred to, a young man would naturally be taught to make drawings in forms which could readily be moulded. This would require a reasonable participation in the business of pattern-making and moulding, and the mixing and melting of metals would come in eventually in connection with the study of their physical properties. It is thought that all this preliminary work can be distributed through the first two years of the college course without occupying a great deal of time. It should be proceeded with as a sort of recreation, organized sufficiently only to counteract the natural tendency of youth to change his plans continually and not to complete anything undertaken. After the initiative steps further advances can be well taught by clinical lectures, with little actual participation. It is certainly unnecessary to make young men sufficiently expert to manufacture articles for sale, as the time required would prevent them from making the best of their opportunities in gaining a knowledge of essential principles and their application to practical operations. In teaching the mechanic, he should be required to continue the use of each particular tool on different classes of work until he becomes expert. In teaching the engineer, he should be first familiarized with the method of operation in detail of each tool, and then be taught the principles of the machine, the proper proportions to make it efficient, the methods of adjusting it, and the special care required in the construction of certain parts to make it accurate. The civil engineer is taught before using his leveling instrument to adjust the spirit level parallel to the line of collimation of the telescope, and to set both at right angles to the vertical axis. In studying a lathe, the mechanical engineer should be taught the method prevalent in the best shops to bring all the bearings of the head and tail stocks exactly in line and parallel with the shears. He should be taught what parts are liable to wear, how such wear is compensated for, and the best forms of construction to prevent it from having undue

influence. He should be taught the quantities of material which can be removed with proper cutting tools in machines of various sizes and proportions, and the improvements in construction which enable modern machines to do more work than the older ones, should be clearly pointed out. In this way the mechanical engineer is at the outset put ahead of the mechanic, as he knows more about the tools which the mechanic is using and is prepared to design others on proper principles.

The same method of instruction can be carried out with all the apparatus used by the engineer. The plan is substantially adopted in most schools in relation to steam engines but is not so general in respect to the boiler. The mechanical features of construction of a boiler are to be studied carefully, such as the riveting, calking, bracing, etc.; the physical properties of the materials are to be examined, and the proper design and proportions of the boiler are to be considered, so as to adapt it to the work to be done and secure the maximum economy obtainable when all conditions are considered. Boilers can be made of very many shapes and can vary greatly in size to do the same work. When weight is a consideration there can be some sacrifice in economy. The mechanic has to deal only with the first and to some extent the second of these considerations; the mechanical engineer must understand all. During the progress of every branch of study, references can be made to forms of construction illustrating each principle under consideration. Success or failure due to the application or non-application of correct principles can be continually pointed out. Attention can be called to the work in progress in the higher classes to show the necessity of the preparatory work in the junior classes, and throughout the whole course of study the mind of the student can be kept interested with apparatus, methods and principles relating to the ultimate object to be attained. The first two years will necessarily be occupied with the study of principles and the preliminary knowledge of practical details, the latter part of the course with independent investigations in the mechanical laboratory and such application of principles as are practicable with the time and facilities at hand. It is felt that in some cases more has been attempted than the students could digest. It is thought that great patience should be exercised in impressing upon the students the knowledge of underlying principles, and in making one or two applications which will apply directly to the regular

experience of the engineer. Too much time is frequently spent in elaborating the various changes and conditions which are possible, and working out hypothetical results, too often founded upon insufficient data, and which have frequently so little reference to practical operations that their value consists only in the discipline of mind required to understand them. It is thought that the existence of all these possibilities should be known to the student, but that there is no necessity that he should work them all out. The information can be briefly conveyed in lectures with references to text-books and current literature. It is thought that in some text-books too much work is left for the student. Frequently a mathematical formula is given, with the general statement that it is based on certain principles already stated, but without references of any kind or a hint of the necessary reductions to secure the result. Granted that a certain amount of this work is necessary to test the knowledge of the student in his preliminary studies, certainly, after this result is accomplished, his time is too valuable for him to be continually at work on problems much in the nature of puzzles. The teacher should be trusted to know that his pupils have the proper knowledge of preliminary principles. It should be realized that the time available is very short, and every effort be made to enable the student to cover as much ground in various directions as possible. Many professors fear to do what they think may be called slipshod work. There is no danger of this if they have impressed upon the students such a knowledge of first principles that in after life they can satisfactorily investigate nearly any desired problem in engineering, using their note books to find references where the subject is treated more in detail than is possible in the limited time available at school.

It is believed that if these methods be rigidly followed out the course of study can be broadened. It is desirable that an engineer should know something more than his specialty. He should have a fund of general information. He has duties to society and to his friends. He not only should know "what to do and how to do it," as ex-President Sweet has said, but he should be able to impress others with the fact that he has this knowledge. It is desirable therefore that students in any special direction should have a general knowledge of everything that is going on in the world, and be able, if possible, to converse intelligently on every technical subject taught in a university course. The superficial education of many women is of special value. They know how to

be entertaining without exhausting their lives with details. Something of this kind should be combined with the hard, closely-drawn lines of a technical course. The old saying, "Know something of everything and everything of something," is valuable. By means of lectures, debating clubs, literary associations, brief lectures of specialists and the like, some superficial general information should be imparted in all departments of knowledge. At the same time a course of study in certain directions should be enforced rigidly, and the students be required to show that they understand the underlying principles and their application to ordinary practice, and care taken that they inform themselves as to the existence of various higher investigations, and have references so that the same can be found as required. It has been thought that, with this method of teaching, engineers of all classes can receive the same education until near the end of their college course. Few young men can know in early life what particular branch of engineering they desire to follow. If all have the same knowledge of principles and all have the same general information in relation to engineering questions of all kinds, no abrupt departure is necessary to undertake any particular specialty. If time be properly conserved, all engineers can be taught in general terms the use of the instruments and many of the details of the work of civil and mechanical engineers so early in the course that, in the fourth year, in connection with general instruction and joint participation in certain laboratory work, such as experiments on the strength of materials, special practice can be had which will fit the young men to be more immediately useful in the particular branches of engineering they have severally selected. Graduates should, however, be instructed that they have not finished their education. They have earned the right to be called engineers and can begin to participate in practical work in a way which can never be taught in schools. They should keep in view the fact that they are really in a business minority until they are forty years old, and be ready previous to that time to forego the most flattering business engagements if the same are not calculated to elevate them in a practical knowledge of their profession. They should be taught, instead of attempting in a conceited way to direct the practical work they are engaged on during their business minority according to views supposed to have been learned at school, but really only a youth's notion of such views, that they should willingly follow along in methods previously in vogue

in the particular establishment, intelligently carrying out the orders of superiors and quietly studying the cause of this or that particular practice, and endeavoring to ascertain why things are done in such apparently curious ways. The result in nine cases out of ten will be that finally a practical reason will be found why certain apparently evident methods were not adopted, and why those inferior in some respects were better when everything was considered.

Many ambitious professors aim too high. Year after year they attempt to improve their course of teaching, even to classes of the same grade, by the introduction of more matter and in greater detail. It should be borne in mind that freshmen one year are as fresh as those that entered the year before; that they have on the average only the same capacity and cannot be made to accomplish any more than the previous class, unless the former course was specially faulty. Minor improvements can be made to advance each class a little above its predecessor, but the work may be overdone. The professor is older than his pupils, his own mind is maturing, and in most cases he is discussing questions and solving problems many years in advance of any members of either of his classes. If he attempts to utilize these higher investigations as portions of his lectures, he may be wasting the time of the students. Evidences of this have in some cases been so conspicuous that it has been thought that the independent privileges usually accorded to professors, at least in universities, should be modified, and the course of instruction, even in detail, brought under one general head. In this way only, it is thought, can the best advantage be taken of time available, and all departments be progressed harmoniously and systematically. This plan has recently received assent in a prominent manner, and although the present speaker could not compromise his business engagements sufficiently to give the necessary direction at a particular place and at the same time actively participate in teaching, it is gratifying to know that another has been found to give his whole attention to the subject, who is clothed with the necessary authority to carry out his work in his own way, with practically the system of organization herein proposed.

The above statements are offered simply as suggestions which if put in practice must be modified to suit the particular conditions of each location and the objects to be attained.

Mr. Green.—This discussion, Mr. President, has taken a very broad range, and the opinion seems to prevail on one side that only

a certain class of men are by nature fitted to be mechanical engineers. On the other side it has been argued that by the training which we can give to our boys we can fit any of them to become mechanical engineers. Between those two points lies the truth. When the Almighty created this world and gave it to man for an inheritance, I do not believe that he intended that because a man had a head of a certain shape, that a certain pathway in life was shut against him, and while I will admit that some men are better fitted than others for particular work, I think that the methods of education may be made such as will fit men for such stations in life as their will pushes them towards. Our minds are made up of the will, of the emotions, and of the understanding, and if a man in the early part of his life, without any reference to what is the shape of his head or who his father was or how much money he may be possessed of, makes up his mind to reach a certain spot, the probabilities are that he will get there. Then the boys that are coming along, whether they are in the workshop or in the college, must be taught, as the gentleman who immediately preceded me said, the fundamental principles of the business. They must be taught what it is to study, and whether the education shall be in the workshop or in the college it does not make any difference. Nature is the same to us all, and the principles that underlie all these things are the voice of nature to each brain and to each soul. It is when we learn properly to interpret nature's speech to us that we receive an education, and that school which will best do that is the school to teach us all. In the early part of the discussion here about schools a little question came up that brought out to me a thing that I have spoken of a thousand times. If a little natural law, apparently insignificant, was against the machine we were building, though our machine was as big as the pyramid of Egypt, it would be a failure, and the best way to train the boys is to teach them what are nature's laws—how does nature act. In the schools of the country, from the common schools to the great colleges and universities which are frequented by all classes of boys, if there was more attention paid to the conduct of those boys at the school, so that their habits would be thoroughly investigated, that they should be kept in certain metes and bounds—in that way more than in any other, the habits, the strength of character and the power for usefulness of our young men would be advanced.

Prof. Webb.—I wish simply to say that I am extremely gratified

by this discussion, and hope that you will all think over it, and that we may continue it at the next meeting. I shall take pains to have a report of the discussion upon this subject at the coming annual meeting of the American Association for the Advancement of Science put in shape so that you can get it before that time.

The President.—It is with exceeding regret that I am obliged to call the discussion of this subject to a close. In a session which has been conspicuous for admirable discussion, more so than any session I think I have ever attended, this seems to me to be the most interesting. It is interesting not only because it is a question which comes to every father and to every mother, but it is an important question which no other tribunal is so well fitted to pass upon as this. While scientists all over the country may add very much to what we want to know on this subject, yet the final test must come from the men who have brought themselves into prominence by the various modes of instruction which they have received. And I know of no higher tribunal in which a question of this kind shall be solved definitely and decidedly than a tribunal which carries in its ranks men who have made their mark in this country and in others, and who are well known as the best mechanical engineers of our own country, and those are the men, in part, whom you have heard here to-day.

ADDED SINCE THE MEETING.

Prof. R. H. Thurston.—I have been very much interested in the paper presented by Professor Alden, and feel that we owe him our heartiest thanks for preparing it. It has been the hope of many of those engaged in this department of educational work that a very full and free discussion of the subject might be secured, in which the views of all who have had experience in this work, and of all the leading men in the profession, might be expressed, and some progress be thus made toward the establishment of a course of instruction in our technical schools that should have maximum power of improving the minds, and increasing the usefulness, of students attending such schools, and at minimum cost in time and money, both to the pupils and to the schools.

The account here given of the Worcester school and its methods is particularly interesting as detailing methods that have, as I know by personal investigation, been fruitful of good results to an

exceptional degree. I have been desirous of obtaining just such a statement, with tabular representation of the distribution of time, from the several schools, and am greatly pleased with the fullness and exactness of the statement given us by Professor Alden. Assuming that the kind of work is to be done which has been so successfully performed at Worcester, I think that it would be difficult to plan a course that should be more efficient than that here exhibited. The two prominent characteristics of the scheme are the considerable time devoted to shop work and the incorporation of a commercial business into the organization of a school of this kind. The result must evidently be, under efficient management, and whatever may have been the intention originally of its organizers, the production of a class of educated and skilled mechanics such as was never before known. I have seen the products of the school, both intellectual and material, and know it to be excellent —just what this outline of the methods of its production would lead us to expect. The men are trained up to a limit of skill which is determined simply by the capacity of the individual; the tools and other work turned out have been, so far as I have been able to observe them, most excellent, and creditable alike to the school and (especially) to Professor Higgins, the superintendent. The Worcester school has always impressed me as an exceedingly efficient school of the mechanic arts. Its course being of but three years' length, and its work so excellent, one is naturally led to ask, What might it not accomplish were it made a third longer by the addition of another year? I am inclined to accept this course as one which may be considered as, on the whole, representative of a very perfect adjustment of means and opportunities to ends. The catalogue of Alumni, with their present positions and occupations, proves it to be capable of turning out men who are needed.

I am particularly pleased with the evident care taken in the apportionment of study and working hours, with a view to relieve the student from that strain which comes of too prolonged effort, and to secure a fair distribution of mental and physical labor. I have no doubt that the statement that no indications of the evil effects of overwork have been observed is the natural result of this care. The throwing of a large part of the shop work into what, with other schools, is vacation time, and the endeavor to secure lengthened periods of practice at other times, seem to me thoroughly excellent provisions for increasing the efficiency of

hours devoted to shop work. The longer the period of continuous application, up to the limit of comfortable endurance, the better the result. I am not at all certain that the extension of this system still farther might not be an advantage; but it is to be kept in mind, of course, that the good effect of the manual exercise, as a relief from mental work, may be lost if this is carried too far. Professor Alden's testimony in regard to the good effect of this manual labor upon the efficiency of mental exercise is as encouraging as it is *apropos*.

The question whether a commercial business and a school of manual training can together form parts of such a scheme of technical education as this is one which has been a prolific source of discussion. It is evident that its decision must be determined largely by the test of experience. It has here been a success, if we may accept the testimony of those who are best competent to testify—those who are conducting the experiment, and have, as they believe, reached a definite conclusion. I should suppose that the success or non-success of such a combination would be determined very largely by the direction which circumstances might give the work done in this way. If the commercial element should be allowed to control, there would be danger that the time of students would be frittered away, in the endeavor to make profits out of skilled men at a time when, having acquired such skill, they should be at once directed to take up another kind of work, and perfect themselves in that, instead of making money for the business department, and thus utterly wasting their own time. Only a wise discretion, and a strong government, could perhaps prevent this abuse. If, on the other hand, the fact that the instruction is the principal object, and the finances the incidental part of the system be kept prominently in mind, the work being done commercially, in the main, by employed skilled workmen, the student being only permitted to practice his art in their company until he has acquired a satisfactory proficiency in the handling of tools and the construction of machines, and being directed to other lines of work as soon as the desired proficiency is attained, the presence of the commercial element is evidently—to my mind—a very important advantage. A machine shop beside the school is certainly not, in itself, a disadvantage. This is as evident as is the tendency to abuse of the opportunities which it offers. A knowledge of shop methods and of business routine is as essential to the young mechanic and engineer as is familiarity with the

use of tools. I can testify to this from a personal experience in which school and college work was enlightened by a shop practice and experience simultaneously gained during spare hours, holidays, and vacations. The practical knowledge so acquired was fruitful during every day of subsequent business life, and later periods of a very varied experience in many branches of professional work in practical, as well as in, as it is called, "theoretical" work. It has been my experience that a healthful mingling of the two lines of work is, as a rule, vastly more valuable to the individual than can be the same amount of time expended in either direction separately. I doubt the possibility of any man doing the highest work of which he is capable in the profession of engineering, or in any direction in applied science, without this intermingling of study and practical experience. The so-called "practical man's" contempt for the so-called "theoretical man," it cannot be denied, has a real basis in the inefficiency of the latter, where unfamiliar with the working side of his profession, and his consequent inefficiency, if not absolute incompetence, when called upon to *do*. The ability to scheme, to concoct problems in mathematics, to invent a new formula or to construct an elaborate theory upon a basis of practically impossible assumptions, does not impress the former class with very much respect for either the judgment or the ability of the latter, when accompanied by absolute lack of every-day knowledge, and of that hard common sense which only the hard rubs of practical life can fully develop. I consider it a matter of the utmost importance that this etherealizing of the mind of the student by pure mental gymnastics should be corrected by the training obtainable only by contact with those who are *doing*, as well as *thinking*. As some one has well remarked, "Dreaming is not thinking," and all mental exercise is apt to degenerate into dreaming when not steadied by beginning and ending with something tangible, and by aiming at practical results. For the engineer, and for the mechanic, life is too short to permit of its devotion, to any great extent, to pure mental gymnastics. *Applied* gymnastics give him the same subjective effects combined with the no less essential, indeed, to him, more essential, objective effect, the acquisition of the ability to earn a living.

It is evident that before it can be said that one or another of proposed courses of instruction is the better for the student, the precise object sought to be attained must be defined. If the pupil is to become a mechanic, working at his trade, he will re-

quire one special line of study and practice; if he is to be qualified to take charge of a manufacturing establishment, directing its administration, he will need another kind of training; if he is to become the superintendent of its shops, to design a special line of products, to become the "engineer" of the establishment, he will probably ask a somewhat different course of instruction; while, if he is to become a "Mechanical Engineer," in the sense in which the term is now customarily used, and is to be made competent to enter into any one of a very extended list of lines of work, such as designing prime movers and mechanism, advising in regard to the use or construction of the best forms of machinery found in the market; if he is to be made competent to test steam and other machinery to determine its efficiency and its fitness for its intended use, he must have a still broader training. It may be that it is proposed to lay out a course of general instruction in the latter direction, and to supplement it by courses of instruction of a special character, the first part forming the regular course of instruction to be pursued by the ordinary student, and the succeeding years of his college life being given to the study of such special line of professional work as he may have chosen, as, for example, naval engineering, railroad engineering, or the engineering of the textile manufactures; it then becomes obvious that the subjects taught, the order of their presentation, and even the methods of instruction, may be found to take shape, in adaptation to the purpose in view, in quite a different way from that above referred to.

It is this latter case which is most interesting to engineers generally. It is certainly that which has most attracted me, and is that in the development in which I have most desired to take part. It is now nearly fifteen years since I planned a general course in mechanical engineering, of which the following is the scheme:

Course of Instruction in Mechanical Engineering.

I.

Materials used in Engineering.—Classification, Origin, and Preparation (where not given in course of Technical Chemistry), Uses, Cost.

Strength and Elasticity.—Theory (with experimental illustrations) reviewed, and tensile, transverse and torsional resistance determined.

Forms of greatest strength determined. *Testing* materials.

Applications.—Foundations, Framing in wood and metal.

FRICTION.—Discussion from Rational Mechanics, reviewed and extended.

Lubricants treated with materials above.

Experimental determination of "coefficients of friction."

II.

TOOLS.—Forms for working wood and metals. Principles involved in their use.

Principles of pattern making, moulding, smith and machinists' work so far as they modify design.

Exercises in Workshops in mechanical manipulation.

Estimates of *costs* (stock and labor).

MACHINERY AND MILL WORK.—Theory of machines. Construction. Kinematics applied. Stresses, calculated and traced. Work of machines. Selection of materials for the several parts. Determination of *proportions* of details, and of *forms* as modified by difficulties of construction.

Regulators, Dynamometers, Pneumatic and Hydraulic machinery. Determining *moduli* of machines.

POWER, transmission by gearing, belting, water, compressed air, etc.

LOADS, transportation.

III.

HISTORY AND PRESENT FORMS OF THE PRIME MOVERS.

Windmills, their theory, construction, and application.

Water Wheels. Theory, construction, application, testing, and comparison of principal types.

Air, Gas, and Electric Engines, similarly treated.

STEAM ENGINES.—Classification. [Marine (merchant) Engine assumed as representative type.] Theory. Construction, including general design, form and proportion of details.

Boilers similarly considered. Estimates of *cost*.

Comparison of principal types of Engines and Boilers.

Management and repairing. Testing and recording performance.

IV.

MOTORS APPLIED TO MILLS.—Estimation of required power and of *cost*.

Railroads. Study of Railroad machinery.

Ships. Structure of Iron Ships and Rudiments of Naval Architecture and Ship Propulsion.

PLANNING Machine shops, Boiler shops, Foundries, and manufactories of textile fabrics. Estimating *cost.*

LECTURES BY EXPERTS.

GENERAL SUMMARY of principal facts and natural laws, upon the thorough knowledge of which successful practice is based; and general *resume* of principles of business which must be familiar to the practicing engineer.

V.

GRADUATING THESES.

GRADUATION.

Accompanying the above are courses of instruction in higher mathematics, graphics, physics, chemistry, and the modern languages and literatures.

This course of instruction was intended to be introduced, with the approval of many of the most prominent members of the profession who were at the time consulted, into the curriculum of the Stevens Institute of Technology, to which institution I had then been just called as Professor of Engineering. That institution had been founded as a school of mechanical engineering, purely; and it was thought possible, there, to give as full a course as is here laid down. The event proved this assumption to be a mistake, and indicated that, even in a special school of engineering of this character, it is not practicable, in the time allowed for the professional studies of its four years' course, fully to cover the ground here proposed. Year by year ground has been gained, and the plan here outlined more nearly carried out; but it will require still better preparation of entering classes than is yet found usual to permit its full acquisition.*

The first division of the course includes the study of the materials of engineering, and should be very fully developed, even to the partial exclusion, if necessary, of the special courses which form the third and fourth divisions. I think that this work should be

* As a commentary on this scheme, attention is called to a statement sent with it to the *Scientific American* Supplement, where it was first published, April 19, 1884, p. 6904.—R. H. T.

done very thoroughly, as far as it is done at all. The practitioner finds a thousand applications of this section of his course presented to him, to one of the later part, unless, indeed, he is making a business of some one of the special branches, in which case, even, it is better that the student should be well taught in the elements of the work, leaving him to study the advanced portions as demanded in his business, rather than to take the reverse course. He always works under an experienced preceptor, when entering into business.

While teaching him the characteristics of the materials used in his work, the student should be given experimental practice in the testing of such materials. If a "Mechanical Laboratory" can be organized in connection with the general course, it will give the best possible means of supplying this kind of instruction. Where shops are organized—and no course in mechanical engineering can be successfully taught without them—it will be found that the natural system includes laboratory and workshops in one organization, under the direction of a single mind familiar with the needs, and authorized to assume the direction, of the course. Experiment and lecture-room instruction must go hand in hand to secure the highest attainable result.

The work summarized in section 2 is largely work-shop instruction. It should be possible so to arrange the course that the manual training and the study should be made to alternate in such order and in such proportion that each shall help the other. I know of no more successful intermingling of brain work and manual exercises than is illustrated in the Naval and at the Military Academies of the United States, and I see no reason why an equal success should not be attained in a technical school in which the workshops, drawing rooms, lecture rooms and the private study may all be made to supply elements of a scheme which should be largely shaped by the physiological and hygienic demands, which, hitherto neglected, must soon be made controlling considerations. I would subject every student entering one of these professional schools to examination by a physician, before admission, rejecting every man not certified to be well prepared to take what must necessarily be a heavy course, and I would then endeavor so to arrange the work of the successful aspirant as to promote good health, and to secure increasing efficiency of body and brain. Physicians who are familiar with the enormous influence of bodily symmetry upon the development of the brain and

nerve power will urge careful training of the left side as a part of the manual labor in the workshops.

In the study of Machinery and Millwork, section 2, which is, perhaps, the most important part of the whole course, the theoretical courses in graphics of construction, and in kinematics may be best at once followed by machine design, the theoretical part of the course being carefully adjusted in such manner that the time devoted to design and proportioning shall not be sacrificed to a serious extent. In fact, the whole course of work in the drawing room should be directed to this end, and the drawing department should be essentially a school of design, and distinctly so recognized.

Beyond this point, the course must evidently be shaped by circumstances, becoming more or less extended, and covering more or less of detail, as time and the practicability of securing efficient instructors, may permit. The whole course should be directed with a view to the preparation of the student for efficient work, first in the shop and designing room, under direction of older and more experienced heads, and finally in the highest and most exacting department of professional work, the creation of new forms of mechanism. The practical application of knowledge to the attainment of distinctly defined ends is to be kept in view as the sole object of all purely professional training. To know, with the engineer, is simply introductory to the greater aim—the ability to do, the acquirement of the power of originating, and of putting ideas in concrete form.

In order that such a course of professional instruction as this may be properly carried out, it is evident that every instructor engaged in the work should be familiar with the demands of practical life and of professional work. Not only should the Professor of Mechanical Engineering be a practitioner, and thoroughly versed in the art as well as the theory of his profession, but the teacher of drawing should be equally familiar with actual work; not simply the principles of descriptive geometry and the methods of the drawing room should be familiar to him, but he should be expert in designing, and should be capable of teaching the student all the details of calculating, forming and proportioning the parts of all familiar machinery. The art of designing must necessarily be largely taught in the drawing room, since it can only be satisfactorily practiced with the aid of the apparatus of the drawing room. The course of instruction in the lecture rooms

should furnish the requisite knowledge of principles and of the facts relating to the strength and other properties of materials, and the modifications of design necessitated by the exigencies of construction; but the actual designing must be done in the drawing room, and the course of instruction there, beginning with the principles of graphics, should lead, at the earliest possible date, into the work of laying out the parts of machinery.

The instructor in the workshops should also be educated and at home with the principles, as well as the practice, of his art; and even the teachers of mathematics, the instructors in the languages—in fact every one who takes a part in the work of educating the young engineer—should be familiar with the applications which the student is to make of the knowledge which he is to communicate to him. Such a course, so taught, must be successful.

CLXXVIII.

SHELL AND WATER-TUBE BOILERS.

BY ALLAN STIRLING, NEW YORK.

No apology need be made here for offering a paper on the subject of steam generators; and if it were intended to advocate any particular form of generator, no more intelligent and appreciative audience could be found for its presentation than the American Society of Mechanical Engineers. It is not the intention, however, to advocate any particular form of boiler, but simply to state some

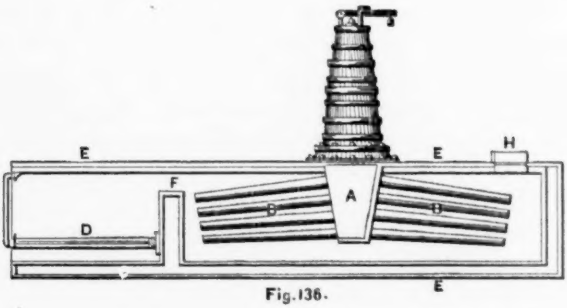


Fig. 136.

facts which are strictly historical and which have been gleaned from various sources. This being the case, it will not be expected that anything original or striking shall be presented; but that the discussion of the general subject of boilers, in which it is hoped the members will indulge freely, may lead to some useful result in advancing the art of boiler construction, an art which is entitled to hold a place second to none in our modern civilization.

It is a common impression that water-tube boilers are modern as compared with shell boilers, but this can easily be shown to be erroneous. In 1802 John Cox Stevens of Hoboken, New Jersey, patented a water-tube boiler in England, and shortly afterward employed a boiler of the same construction to drive a twin-screw steamboat on the Hudson River (Fig. 136).

Although the attention of engineers was thus early directed to water-tube boilers, we find that nearly all boilers built since the invention of the steam engine have been of the shell type. This fact cannot be disputed, and it will be interesting and profitable to discuss the reasons why the shell boiler has so generally been preferred. Your attention will be first called to some of the forms and features of shell boilers that have stood the test of hard usage and worked their way into wide adoption for the generation of steam.

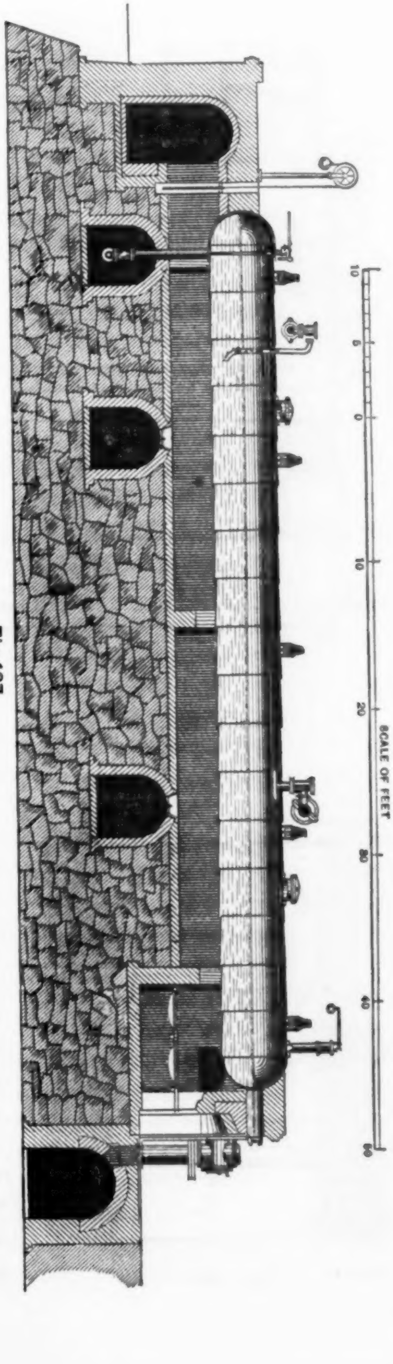


Fig. 137.

The best location for the fire, whether inside or outside of the boiler, is a matter which has received much attention, and each has had its advocates and patrons. Some have maintained that the combustion of gases in a hot brick furnace external to the boiler is so much more complete that it gives the external furnace a great advantage; while others claim that the combustion is as perfect when the fire-place is surrounded with water, and that the radiant heat from the fire gives the internal furnace the preference. John Elder found that furnaces two feet wide, with water legs close to the fire, were quite as good for the combustion of the gases

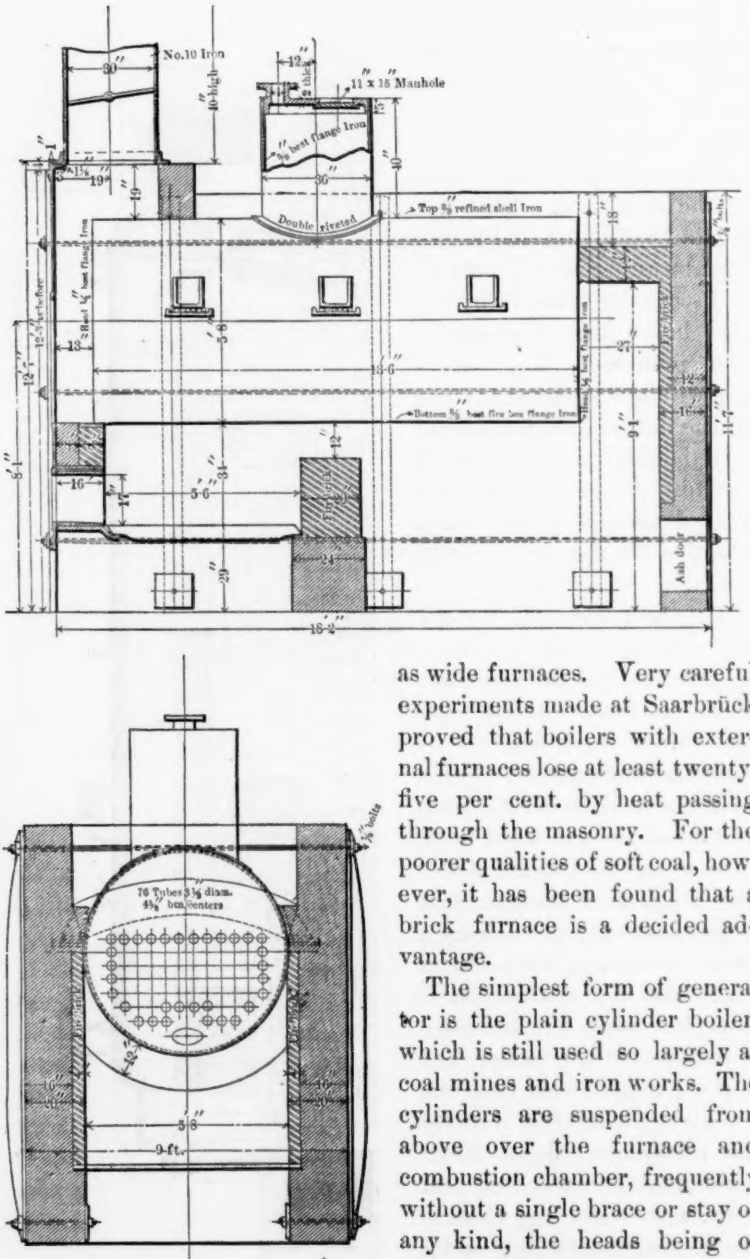


Fig. 138.

as wide furnaces. Very careful experiments made at Saarbrück proved that boilers with external furnaces lose at least twenty-five per cent. by heat passing through the masonry. For the poorer qualities of soft coal, however, it has been found that a brick furnace is a decided advantage.

The simplest form of generator is the plain cylinder boiler, which is still used so largely at coal mines and iron works. The cylinders are suspended from above over the furnace and combustion chamber, frequently without a single brace or stay of any kind, the heads being of cast or wrought iron riveted to

the shell (Fig. 137). After being used for a time they are generally turned upside down, thus prolonging their life. They are simple and easily accessible for cleaning and repairs, hold a large quantity of water, and have a large area of water level. Their great length is necessary so that the heat may be absorbed, as it travels only in one direction. This great length introduces a difficulty due to the unequal expansion of the top and bottom, which has been partially overcome by the use of springs for bearings. These boilers are very heavy and occupy large space in proportion to the water evaporated, and the percentage of loss of heat through the inclosing wall is very great. The principal reason that the writer can offer for the use of such a bad form of boiler is, that while coal and iron men

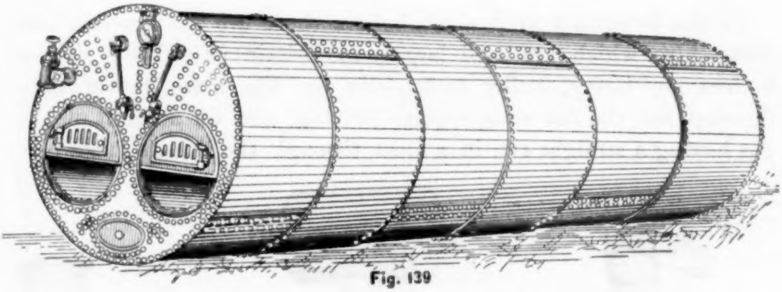


Fig. 139

are generally wide awake in their special branch, they have not always been alive to the importance of the economical production and use of steam.

The horizontal tubular boiler, with which we are all so familiar, has been very little used in Britain and on ocean steamers, but has been and is still very popular in this country. Where the sooty grades of soft coal are used and the water is very muddy, riveted flues or large lap-welded tubes are preferred, as in our western river-boats and manufactories. There is more likelihood of the heat being absorbed in this boiler than in the plain cylinder, because the gas returns through the tubes and is divided up by them, and the metal of the tubes is much thinner (Fig. 138). It has a large area of water level, and the circulation of water, although not clearly defined, is ample. It is practically impossible to make a thorough job of cleaning this boiler by scaling, because its tubes are so close together. The flat ends above the tubes require a complicated system of bracing. The weight and space occupied per horse-power is moderate, and this is probably the best

and cheapest boiler for a given amount of heating surface, at moderate pressures of steam, that can be built. Hence, for stationary purposes, in this country, it is used nearly to the exclusion of every other type of boiler.

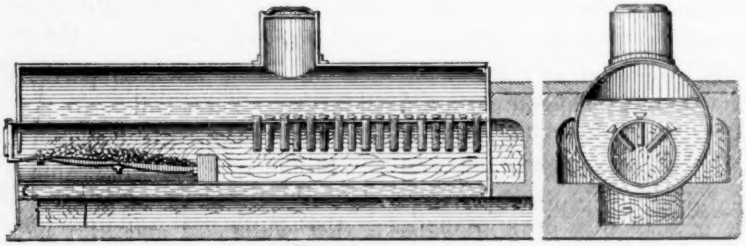


Fig. 140

Of the internally fired shell boilers there are the Cornish and Lancashire, which are used largely in Britain (Fig. 139). These boilers are heavy, bulky and costly for the horse power they develop, and the flat ends require bracing, and on account of the gas having a straight run in a mass through the flues, a large per-

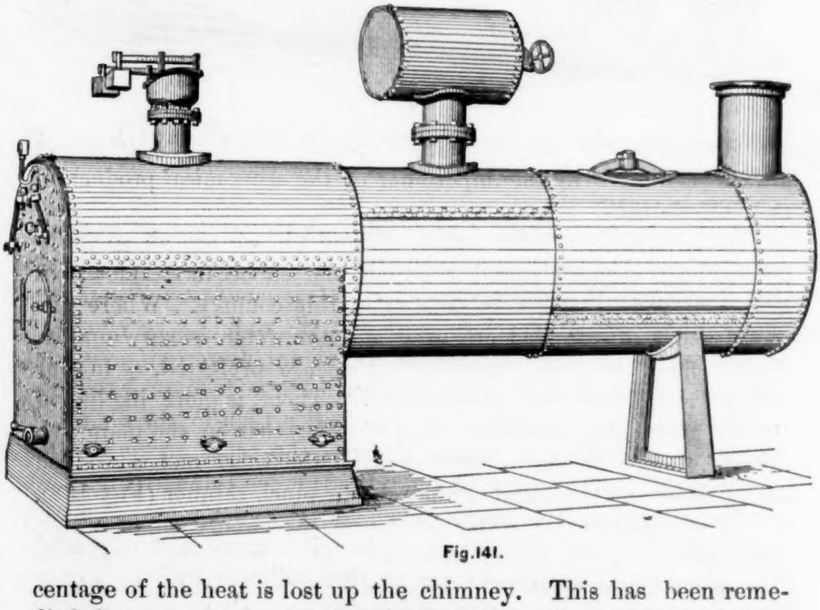


Fig. 141.

centage of the heat is lost up the chimney. This has been remedied, in very many cases, by the insertion of a number of drop tubes or cross tubes in the flues, which serve the double purpose of taking the heat out of the gas and also breaking up the cur-

rents, the result being a material saving of coal (Fig. 140). The expense of thus inserting tubes, in Cornish and Lancashire boilers, has frequently been paid for in three months, by the saving of coal; the economy of coal being in some cases as much as two tons per week for one boiler; and after running two years no diminution had taken place in their efficiency.

Second: The locomotive boiler. This is probably the type of boiler which is most universal. Besides being used to the exclusion of all other forms for locomotives, it has to some extent been employed for stationary and marine purposes (Fig. 141). It has a

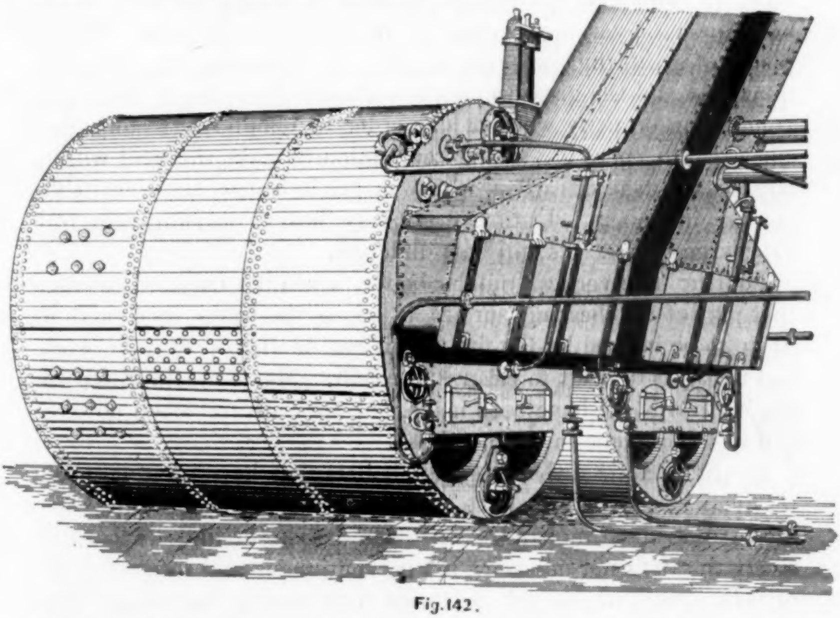


Fig. 142.

large firebox, large area of water level and good circulation, the current being down at the shell and up between the tubes. It may be urged against the locomotive boiler, that it is defective in facilities for cleaning and inspection, and that it has large flat stayed surfaces. But the fact remains, that locomotive boilers *do harder work* and develop more power for their weight, than any other type of boiler. When boilers of large power, light weight and occupying small space were wanted for torpedo boats, the locomotive type was preferred. They are, however, expensive boilers for the amount of heating surface.

Third: The common marine boiler, with its large external shell,

internal furnaces, back connection and return tubes (Fig. 142). This boiler has large area of water level and the circulation of water is good. It has the same features as the locomotive and return tubular boiler, that it is impossible to get at the tubes to clean them thoroughly, and it has large flat stayed surfaces. Their weight, space occupied and cost are all moderate in proportion to the heating surface. One serious objection to this form of boiler, for high pressures, is the great thickness of the shell. It has been found that both steel and iron are much improved in quality by being rolled from a large to a small section. In the case of steel rails, the reduction by rolling is as much as twenty or thirty times, and this has a very good effect on the quality of the rails. Where plates of great thickness are wanted, the reduction by rolling is much less and the great thickness prevents the center of the plates from being properly worked. In order to obtain satisfactory tensile strength, there is a strong temptation to make the steel with a large percentage of carbon, which makes it brittle and unsuitable for use in boilers. Until we get very heavy machinery to roll very large ingots, this will be a difficulty.

Fourth: The vertical tubular boiler, which has these objections: that part of the heating surface is above the water level and is liable to be injured by the fire, that it has small area of water level, and that it is difficult to clean and repair. Many modifications of the vertical boiler have been made, but they are for small powers, and have not come into general use.

It will be noticed that the shell boilers that have been most extensively employed are all so constructed as to render it impossible to clean them by hand. This would seem to indicate conclusively that it is not necessary to the continuous and successful use of boilers, that we should go inside with scaling hammers. The most effective way to keep any boiler clean is to blow it down under steam, at frequent and regular intervals.

One hundred and fifty pounds pressure per square inch is the maximum that has been employed successfully in any of these forms of shell boilers, and it is difficult to see how this can be much increased. The shell of those externally fired and the furnaces of those internally fired cannot be made much, if any, thicker, on account of the direct contact of the flame, and in the case of the internal furnace, the use of the Fox corrugated tube is the thing that has made such high pressures possible as are now carried on board ship.

All shell boilers which have been extensively used have flat surfaces braced, and in the records of boiler explosions it will be noticed that it is the circular parts that have generally given way.

There are some boilers that cannot be classed either as shell or water-tubes, as they have both, such as the "Martin" boiler (Fig. 143), used so extensively in the United States navy; and the "Field" boiler (Fig. 144), which has been largely introduced

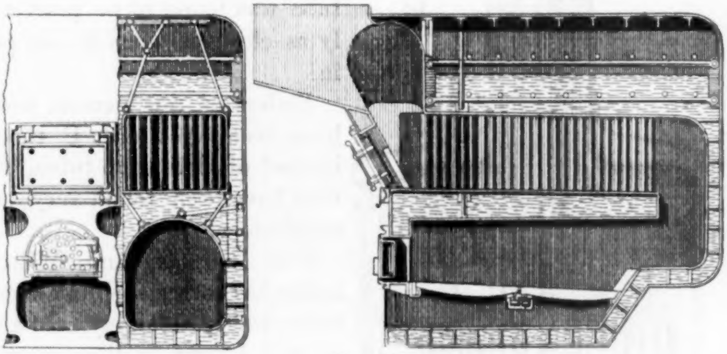


FIG. 143.

in England, and to a slight extent in this country. The "Martin" boiler has the advantage that, when using salt water, the tubes can be cleaned of scale by hand. The "Field" boiler (Fig. 144) consists of a double cylindrical vertical shell with drop tubes from the crown sheet, these drop tubes usually having internal circulating tubes. Boilers of this type have been built without internal tubes and have been found to work well for years. When the internal tubes are used, the rapid and clearly defined circulation keeps the tubes and the tube plates in which they are inserted perfectly free from scale. The drop tube with its internal circulating tube is a very old invention by Jacob Perkins, and is probably one of the greatest of labor-saving inventions. The drop-tube boiler has been used for steamers, locomotives and fire engines and for stationary purposes with very satisfactory results. Merryweather's fire engines, which have obtained a world-wide celebrity, are fitted with drop-tube boilers. That the circulation keeps the tubes free from scale, is shown by th following fact: A "Field" tube was taken out of a tug-boat after eighteen months' hard wear with very dirty water; the tug had been run

seventeen hours daily by one engineer, who, of course, had had very little time to take proper care of the boiler, and consequently a lot of sediment accumulated in the water legs, and the plates were injured. At this stage the tubes were examined to see if the circulation had kept them clean; a saw cut was made two inches up through the center of one of the tubes, a piece was taken out by cutting across, and the tube was found to be practically as clean as when it was put in.

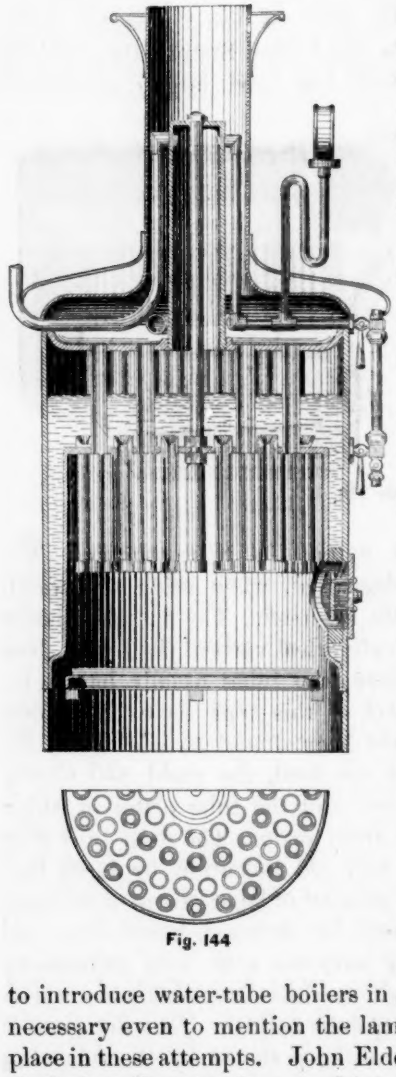


Fig. 144

Boilers of this general form have been built with U tubes, instead of plain drop tubes, but they have been found very unsatisfactory.

The Babcock and Wilcox boiler has both a shell and water tubes, and is so largely used and so well known in this country that its description here is unnecessary. It is hoped that some of the gentlemen present will state their experience with them.

Another form of boiler is made entirely of flat plates, stayed, and worked in sections (Fig. 145).

Coming now to the boilers that are strictly water-tubes, we find that so far as steamships and locomotives are concerned they are conspicuous by their absence. Many attempts have been made to introduce water-tube boilers in steamships, and it is perhaps unnecessary even to mention the lamentable failures that have taken place in these attempts. John Elder made a boiler of inclined tubes six inches diameter for a working pressure of five hundred pounds per square inch, and found that it required four or five times as

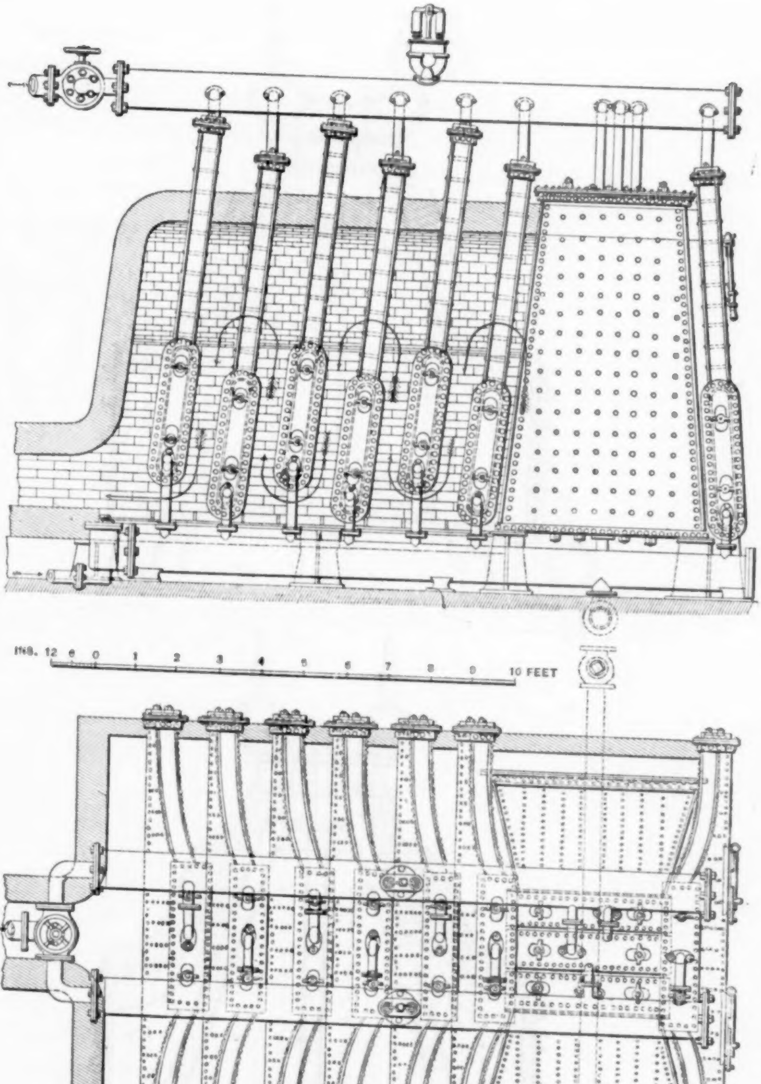


Fig. 145

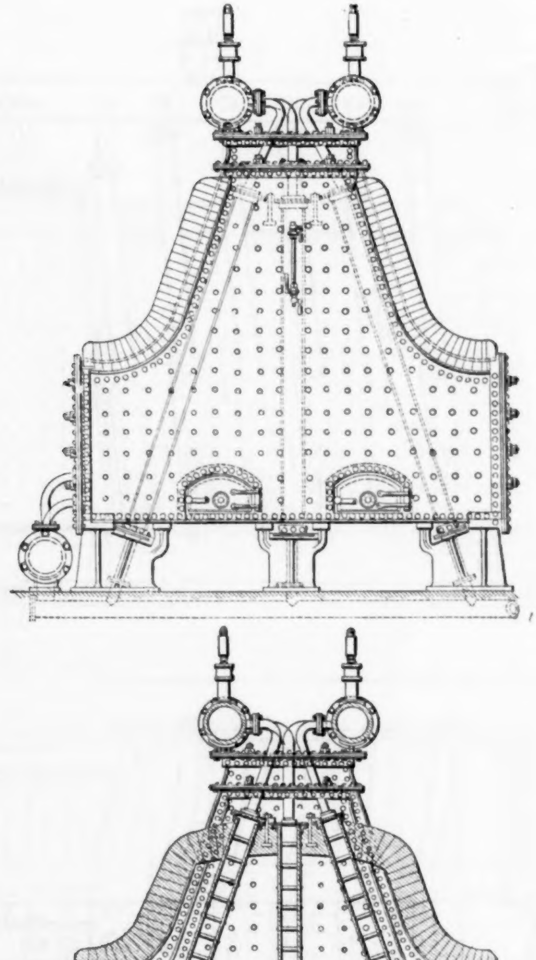


FIG. 145*a*.

much heating surface per horse power as ordinary boilers. Rowan's boilers on the *Propontis* were subjected to a patient and exhaustive test, and gave promise of success for a time, but the final result was that they were complete failures (Fig. 146). In the steamship *Montana* water-tube boilers were tested on a very large scale (Fig. 147). Her boilers had seven hundred wrought-iron tubes, each fifteen feet long and fifteen inches diameter, set in hundreds of tons of bricks. This test cost the owners of the vessel about three hundred thou-

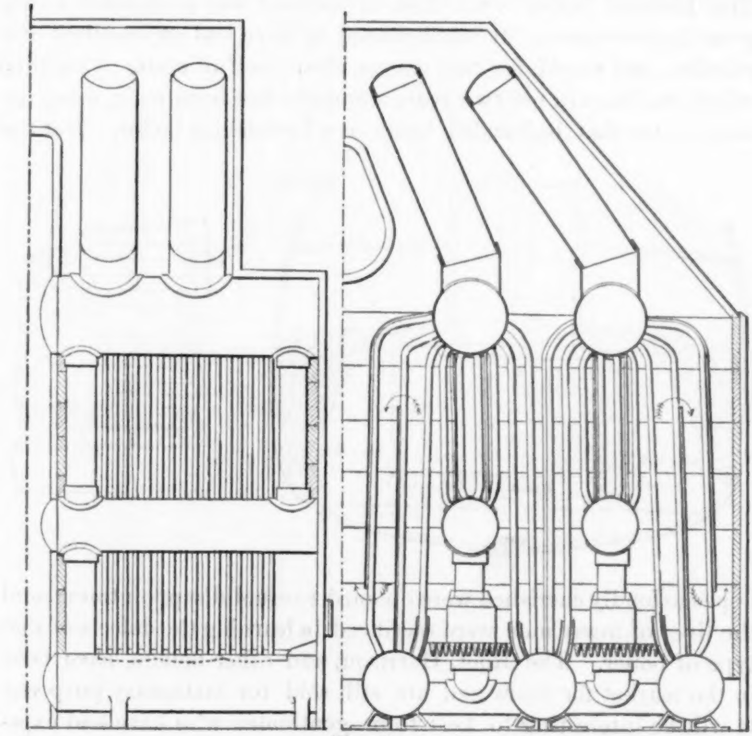
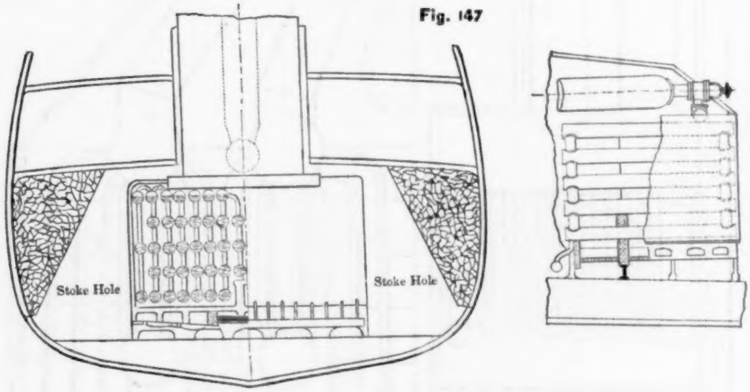


Fig. 146

sand dollars. The enormous weight of these boilers and great space occupied by them had much to do with their finally being removed from the vessel; and even had they been successful in other respects, this fault would probably have prevented their permanent use. The increase in the carrying capacity of the vessel consequent upon the removal of these boilers has no doubt paid for the alterations. It is true that previous to 1869 one hundred and twenty Belleville boilers had been fitted in ships of the French navy, and

that Belleville boilers have been fitted in French ships since, but I believe it is still the fact that not a single ship of the commercial marine is fitted with water-tube boilers. We have no examples of them in any ships that reach this port. Water-tube boilers are used to some extent for stationary purposes, but there have been many failures even here, and in this field many of our engineers prefer the common horizontal tubular boiler, both on account of its comparatively low first cost and satisfactory operation. The Howard boiler when first introduced was considered a very great improvement. It was believed to have had an excellent circulation, and would, for that reason, clear itself of scale. One tube which had been in use two years was quite free from scale, using the same water that had scaled badly in a Lancashire boiler. But the

Fig. 147



hopes so fondly cherished were doomed to cruel disappointment, and the lives of many men were sacrificed in learning the defects of that type of boiler. The Root, Harrison, and other boilers, have been in the market for years and are still sold for stationary purposes. It will be interesting to hear from gentlemen who have had experience with them.

In closing this paper, permit me to call attention to a serious defect in some water-tube boilers (Fig. 148). The steam and water from a large amount of heating surface is compelled to enter the drum through a comparatively small opening, the result being an excessive velocity and great agitation of the water level, which must seriously impair the efficiency of a boiler constructed on this plan.

Notwithstanding all these failures and defects, the water-tube boiler seems to be the boiler of the future for high pressures.

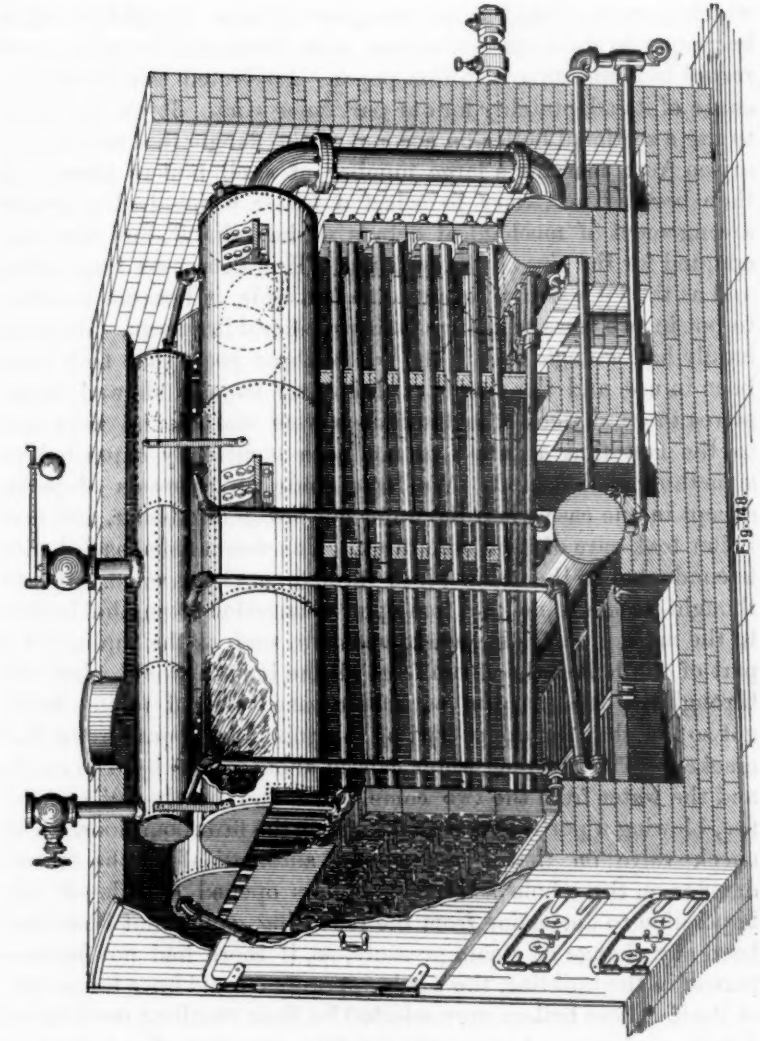


Fig. 148.

DISCUSSION.

Mr. C. E. Emery.—The paper states on its ninth page: "The Babcock and Wilcox boiler has both a shell and water tubes, and is so largely used and so well known in this country, that its description here is unnecessary. It is hoped that some of the gentlemen present will state their experience with them." As probably I have under my charge the largest plant of these boilers

which is concentrated at any one place, I have thought it might be proper to state our experience with them, and have it go on record in connection with this paper. I selected that boiler because of its adaptability for our particular work. It was necessary to use a sectional boiler on account of the danger that would have arisen from massing a large number of shell boilers there. It then became necessary to select a boiler which had a proper arrangement of mechanical details to be reliable, and also one adapted for the size of the building. This firm uses long tubes, and as the lot was just seventy-five feet wide, it became possible to put in two rows of boilers facing a central fire-room. We have hardly had a single leak in the two or three years that they have been in use, and the few that did occur usually showed themselves the first week after the boilers were started, and were due to the fact that a tube had not been sufficiently expanded, or something of that kind. We have made no renewals of parts, except in one case when the water got low in one boiler, and that boiler took care of itself admirably. The steam from each boiler is conducted to the main pipes through a check valve. It was thought unsafe to make a free open connection from the boilers to the main pipes, as in case of accident, such as the rupture of a part of one boiler, the steam from all the boilers would blow out through the one. In the particular case referred to, the water got so low that the upper part of the tube head became hot and cracked. The escaping water caused another tube head to crack, and the water from the two came down and nearly put out the fire, blowing a portion of the fuel out on the fire-room floor. The check valve on that boiler closed automatically; the steam-damper on the whole section of boilers opened a trifle—it was impossible to determine from the recording gauge that there had been any change of steam pressure, so, if there had not been a person in the building, the whole apparatus would have taken care of itself. These boilers were selected for their excellent mechanical details and general proportions. The economy of a boiler, as those who have studied the subject know, is regulated by the amount of coal burned per square foot of heating surface, providing the gases on one side and the water on the other are properly circulated over the heating surfaces. There are great variations in practice in regard to proportions. One firm, which sells a great many boilers of one of the shell types, makes what is called a free boiler, from the fact that it steams freely, but it burns a

large quantity of coal per square foot of heating surface. Another boiler could readily be furnished which would do the work more economically, by putting in more heating surface in relation to quantity of coal consumed, but such a boiler would not be as free as the other; in other words, steam could not be raised in it as quickly, nor would it respond as quickly to changes of demand. The firm building these boilers, being composed of practical engineers, take an intermediate position; they do not attempt to make the most economical boiler possible, or to obtain the power with very limited heating surface. Their boilers, for the proportions they have selected, are much more economical, for instance, than boilers used in the West for soft coal; and, on the other hand, there are few, if any, of the ordinary tubular boilers used in the East which give any better economy; so that, on the average, their boilers are more economical, frequently much more so, than boilers of the ordinary type. I state these things freely, the gentlemen of that firm being members of our Society, whom we all know pleasantly and as good engineers, and it seems to be a proper subject of congratulation that they have made such a success of the mechanical part of their business.

Mr. See.—I have a few remarks to make on this paper. A statement is made on its sixth page, speaking of the common marine boiler: "It has the same features as the locomotive and return tubular boiler, that it is impossible to get at the tubes to clean them thoroughly, and it has large flat stayed surfaces." The statement may be true with regard to the impossibility of getting at the tubes of a boiler improperly designed, but in one of good design the tubes can be readily got at to clean. Such a boiler, provided with proper man-holes, can be got at from all quarters. As to stayed surfaces, I do not see that it matters much whether the surfaces are flat or circular if only the flat surfaces are well stayed. The paper speaks also of the limit of the pressure. Up to the present time the internally fired marine tubular boiler has met the advancing pressure. With the superior material we are now getting in the shape of steel, and with the complete appliances for drilling holes in place in the shell, we have been enabled to increase our pressures, and are to-day building boilers to carry with safety 160 pounds of steam. We do not think we are to stop at this pressure, and, consequently, I cannot see that the remarks with regard to the internally fired marine boiler having reached the limit of pressure hold good.

Mr. Towne.—As this paper is in some sense an historical one, I should be sorry to see it go on record without containing some allusion to one of our American engineers who was a pioneer in the direction of sectional boilers, although not the original explorer, because the boiler of Dr. Alban is probably the precursor of all sectional boilers, and a very full description of that boiler and its advantages which he published, if I remember correctly, in 1841, probably antedates anything else that we have. But Mr. Joseph Harrison, Jr., of Philadelphia, is certainly entitled to credit for his large share in calling the attention of the engineering world to the importance and to the possibilities of safety sectional steam boilers. His effort was, as we all remember, to make a cast-iron boiler, and although the world has not accepted that as a commercial project which embodies advantages superior to those of a wrought-iron boiler, it still was a practical success. It is in use to a considerable extent to this date, the business continuing at present, I understand, in a quiet way, and the boiler accomplished what he set out to do. In mentioning it I can testify from personal experience to one quality which it possesses, namely, that of constituting a safety boiler. In 1866 I was present at a test made by Mr. Harrison, in which the effort was made to blow up one of his boilers. As a matter of precaution it was sunk in a bank of clay, removed from any buildings, with a good fire surface under it, and a heavy fire was kindled. The boiler was filled with water and hermetically sealed ; a pressure pipe was led from the boiler to a gauge at a considerable distance ; and the small party who witnessed the experiment stood there in a place of safety. The pressure rose to several hundred pounds, but could not be got beyond a certain point. The yielding of the longitudinal bolts which tied the cast-iron spheres together was sufficient to open the joints. It would repeatedly reach that point, blow off, the joints close, and the pressure again begin to rise. The exact limit of pressure could, of course, be varied in construction by adopting various sizes of stay bolts. Thus the same result was obtained as Mr. Emery stated in the case of the tubular boiler. The memory of Mr. Joseph Harrison should certainly be honorably preserved in connection with any discussion of this subject.

Mr. Kent.—It is stated in the paper that it is hoped the discussion on it will lead to some useful result in advancing the art of boiler construction. I do not think that discussion is apt to lead to an advance in an art. It often brings up an ocean of past

experience; but I do not think it often leads to an advance. The principal charges I have to make against this paper are two—first, too broad generalizations from the facts which it gives, and secondly, some misstatements of facts, or rather statements of things which are not facts. It is necessary to limit our generalizations in publishing engineering data. The paper says on its second page: "Mr. John Elder found that furnaces two feet wide, with water legs close to the fire, were quite as good for the combustion of the gases as wide furnaces." Fortunately the paper does not make a generalization from that; but it implies a generalization, that because Mr. Elder found that fact it is always true. It is not true generally, and the generalization is too broad. It is further stated on the third page that "very careful experiments made at Saarbrück proved that boilers with external surfaces lose at least 25 per cent. by heat passing through the masonry." We might as well generalize from Mr. Hoadley's paper presented at this meeting, that boilers lose $2\frac{1}{2}$ per cent. by radiation through the masonry. But Mr. Hoadley only says that this particular boiler only lost $2\frac{1}{2}$ per cent., and other boilers under other circumstances lost 4 per cent.

"Where the sooty grades of soft coal are used and the water is very muddy, riveted flues or large lap-welded tubes are preferred, as in our Western river-boats and manufactories." Mr. Holloway said in yesterday's discussion on riveted flues that this preference is due to the experience of many years, and that these engineers use and prefer what they have found to be best adapted to their needs. But I still hold, President Holloway to the contrary notwithstanding, that it is prejudice in favor of what is old that controls this matter, and it might have been stated in the paper that this is the reason why those two-flue boilers are used—simply because of prejudice in favor of what their grandfathers used before them.

"But the fact remains," the paper says, "that locomotive boilers do harder work and develop more power for their weight than any other type of boiler." That is an absolute fact, of course, and the reason for it is that they have to. Of course there is no generalization here about that; but some men might misunderstand it and think that the locomotive boiler is the best for all positions, which it is not by any means.

There is another generalization here about the "Field" tube, on the eighth page, where it says that "boilers of this type have

been built without internal tubes and have been found to work well for years. When the internal tubes are used, the rapid and clearly defined circulation keeps the tubes and the tube plates in which they are inserted, perfectly free from scale." There should be add-

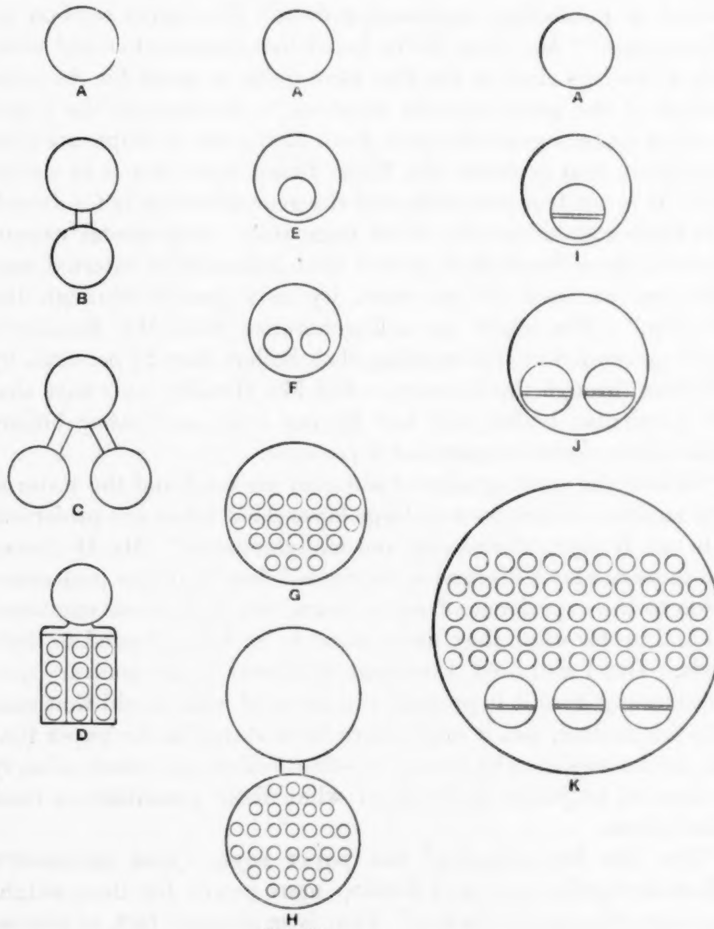


FIG. 224

ed that a "Field" tube, in far less than eighteen months, has been taken out and absolutely condemned because the tubes did not keep the boiler free from scale. No matter how fast the circulation is in any boiler whatever, that boiler will fill full of scale if you do not get some means of removing it from the boiler.

The boiler illustrated on the tenth and eleventh pages is not described except by one line. "Another form of boiler is made entirely of flat plates, stayed, and worked in sections." I would like to see anybody who has ever seen such a boiler. I do not believe it has ever been built, and I do not believe it ever will be. Another generalization is about the failures of water-tube boilers in steamboats. Any one reading this would think that water-tube boilers have always been failures in steamboats. The generalization is entirely incorrect; because water-tube boilers are now in successful use in steam vessels. It is not a fact that "not a single ship in the French commercial marine is fitted with water-tube boilers." The Belleville boilers are in successful use in vessels running between France and Brazil.

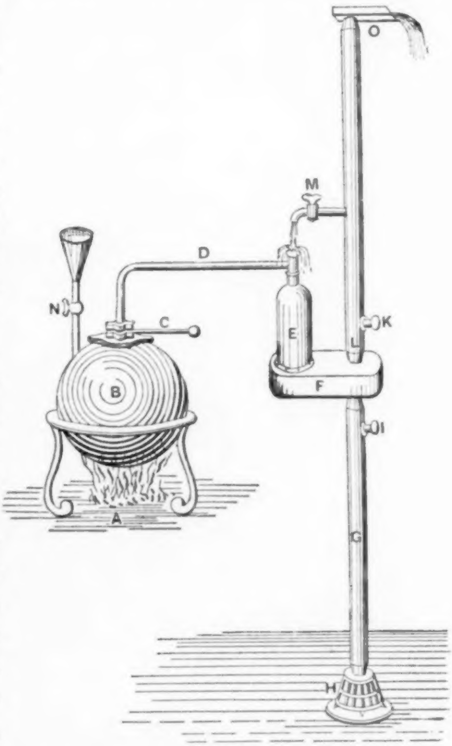


Fig. 241.

There is another statement in the beginning of the paper that is probably true, but the reason why is not given: "Although the attention of engineers was thus early directed to water-tube boilers, we find that nearly all boilers built since the invention of the steam-engine have been of the shell type." That is likely true, and a good reason for it is that a cylindrical shell-shape is probably the best form to put a boiler in; the cylindrical shape being the best shape to resist pressure. I have placed on the blackboard an illustration called "the evolution of the steam boiler" (Fig. 224). Beginning with *A*, the plain cylindrical shell, *B*, *C* and *D*, show the stages of development leading to the water-tube boiler, *E*, *F* and *G*, those leading to the common externally fired tubular boiler, and *I*, *J* and *K*, show the development of the internally fired boilers, the

Cornish, the Lancashire, and the modern marine. *H* is a combination of *B* and *G*.

The locomotive boiler was a special design of boiler built for a different purpose, viz., to go on wheels, and it therefore does not come in the regular order of evolution. On account of the inadvisability of putting a furnace under it, a fire-box was put in it. It is merely a tubular boiler like *G*, with a fire-box attached.

The evolution of the boiler so far shows that safety lies in small diameters; high pressure is also secured by small diameters, and that this boiler, *D*, will therefore likely be the coming type for

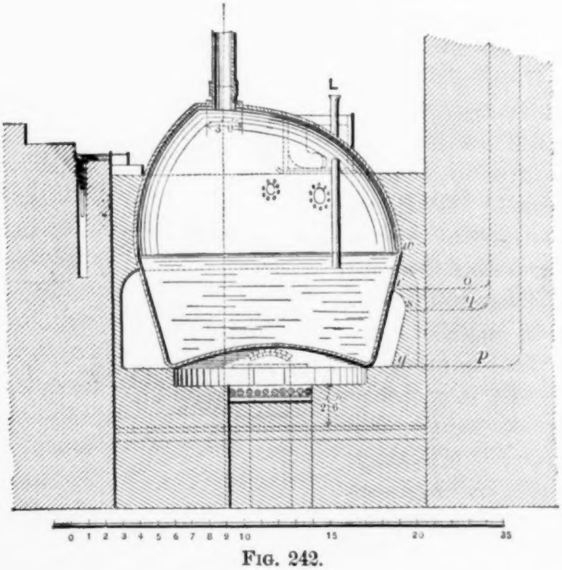


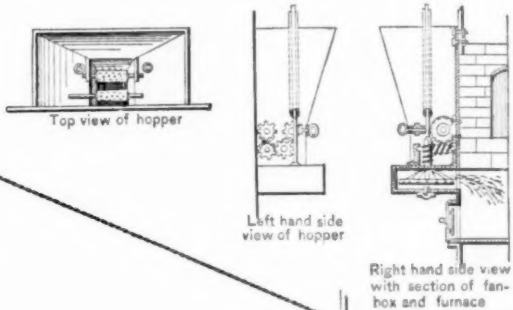
Fig. 242.

marine purposes, replacing *K*, which has reached its limit in 16 feet diameter and plates 1¼ inches thick.

Mr. Durfee.—This matter of the evolution of the steam-boiler is of some interest historically, and I do not think my friend Kent has started with the original type. If we go no further backward in history than the time of the first commercial employment in England of steam as a medium of making heat do work, we find that the form of boiler used by Captain Savery was a hollow sphere of metal, as shown in Fig. 241. Next comes the type of boiler used by Newcomen, illustrated by Fig. 242. Some of these boilers were made entirely of copper, others had only those parts in con-

tact with the fire of that metal, the upper parts being made of lead, and in many cases boilers were constructed of several sections of cast iron united by bolts. About the year 1720 there was a boiler

Fig. 221.



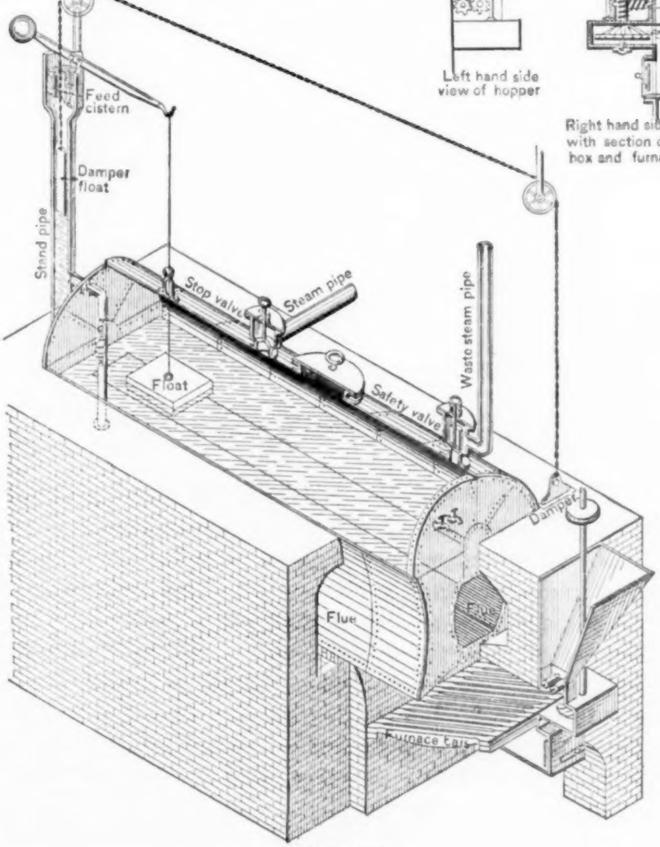
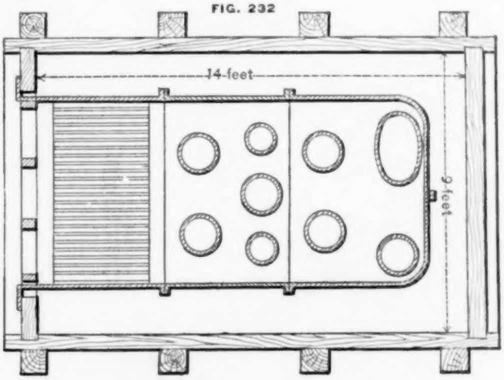


Fig. 220.

used in Cornwall to furnish steam to a Newcomen engine that should not be forgotten. It was principally constructed of granite; large slabs of this material, held together by external clamps and bolts, formed the bottom, sides and top, of a

rectangular box or tank, about twelve feet square and four feet high, through two opposite sides of which passed copper tubes or flues communicating with an external fire-box. There is reason to believe that there were several boilers of this construction in use in Cornwall in the early part of the last century. Soon after the formation of the firm of Boulton & Watt, they commenced the manufacture of a steam generator, which, from the general resemblance of its outline to that of a covered wagon, was called the wagon boiler. Fig. 220 represents one of these boilers having all the latest improvements attached. It will be noted that it is provided with a self-acting "feed," an automatic regulator for the chimney damp-

FIG. 232



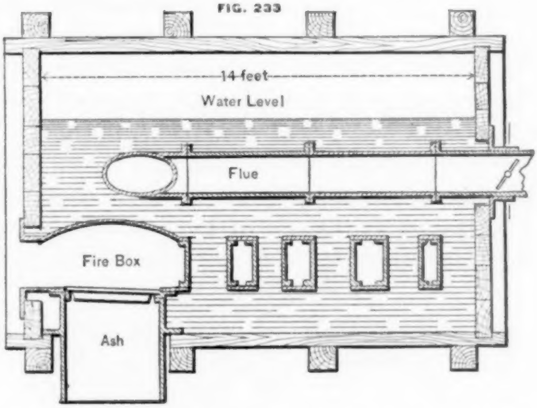
er, and a mechanical stoker, of which details are shown in Fig. 221. This last is of especial interest at this time, as an apparatus identical in form and operation has recently been brought forward as a new invention. It is not improbable that the ideas embraced in the construction of the granite boilers of Cornwall suggested the form of boiler—one of the first employed in the United States—which was used for generating the steam for the pumping engine at "Centre Square Water Works, Philadelphia, from 1801 to 1815." *

This boiler is illustrated by Figs. 232, 233 and 234, Fig. 232 being a horizontal section taken just above the fire grates, and Figs. 233

* I take great pleasure in acknowledging my indebtedness to the courtesy of Frederic Graff, Esq., President of the American Society of Civil Engineers, for permission to reproduce the following engravings and their descriptive text from a very interesting and valuable paper (entitled "The History of the Steam Engine in America") contributed by him to "The Journal of the Franklin Institute," for October, 1876. I am also under obligation to Emanuel Hiltebrand, Esq., Librarian of the Franklin Institute, for highly appreciated assistance.—W. F. D.

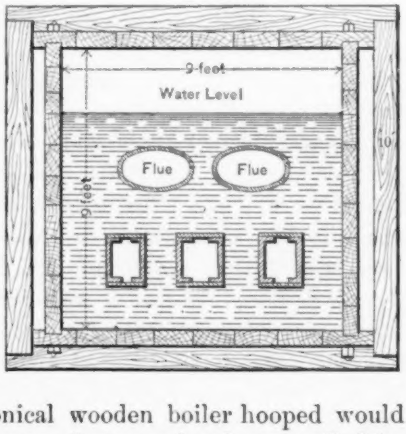
and 234 being vertical, longitudinal and transverse sections respectively. It will be seen that there is a flat combustion chamber back of the fire-box, whose top and bottom are "stayed by short tubular

FIG. 233



castings forming water tubes, and from the back end of which an oval up-take smoke flue passes off—this oval smoke flue was carried by a bend and a return bend forward and back below the water-line. The grate was 3 feet long × 5 feet wide = 15 square feet, while the heating surface was nearly 360 square feet." The above quotation is credited by Mr. Graff to a "Report of Benjamin Henry Latrobe to the American Philosophical Society of Philadelphia, May 20th, 1803." After giving in detail some of the defects of this boiler, and describing the attempts to remedy them, he continues: "I do not, however, believe that everything has yet been done which could be done to obviate these defects. A conical wooden boiler hooped would not be subject to some of them; such a one has been applied by Mr. Oliver Evans to his small steam-engine.* During the two years

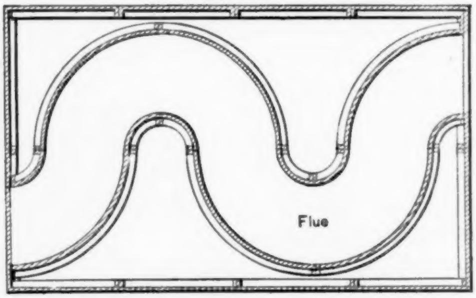
FIG. 234



* Col. John Stevens, of Hoboken, constructed a boiler in the year 1804 whose upper part consisted of a truncated cone made of wooden staves held together by iron hoops.—W. F. D.

which have elapsed since the boilers of the public engines have been erected, much has been done to improve them. Whether the last boiler will prove as perfect in its wood-work as in its furnaces and flues is still to be ascertained by experience. At present nothing can work better. I will only mention one other circumstance, the knowledge of which may prevent similar mischief. In the first boiler erected in Philadelphia oak timber was used to support the sides, bottom and top of the boilers, the plank of which was white pine, four inches thick. In less than a year it was discovered that the substance of the pine plank, to the depth of an inch, was entirely destroyed by the acid of the oak. Means were then used to prevent its further action by the intervention of putty and

FIG. 235



pasteboard, and in most cases by substituting pine timbers in the room of those of oak." *

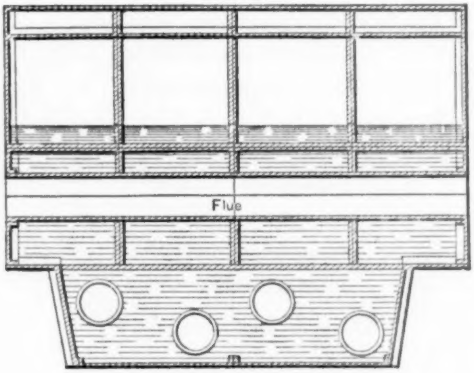
"As might be expected (says Mr. Graff) great difficulty was experienced in keeping these boilers steam-tight,† accordingly, on December 1, 1801, a boiler with cast-iron shell, as well as flues, was put up, and another one, also of cast iron, but of different form, was put in use March 10, 1803. The first was erected in Centre Square. It had a semicircular top, the ends being flat; the fire passed under the boiler around heaters of peculiar construction and through one flue of serpentine plan to the front of the boiler. This boiler had two sheets of wrought iron upon the bottom, just over the fire, all the rest being cast iron." This boiler is

* I am informed by Mr. Frederic Graff that the evidence relative to the construction of this old boiler was "instrumental in deciding a patent suit against Montgomery." This fact is a good illustration of the value of researches relative to ancient mechanism and mechanical methods.—W. F. D.

† The pressure of steam was but $2\frac{1}{2}$ lbs. per square inch.

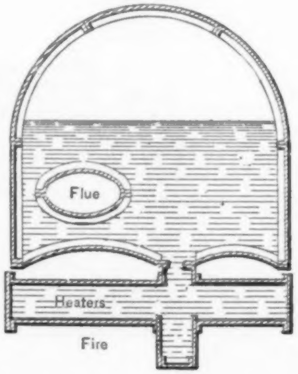
illustrated by Figs. 235, 236 and 237. Fig. 235 is a horizontal section through the serpentine flue; Fig. 236 is a vertical longitudinal section, and Fig. 237 is a vertical transverse section. This boiler was about 15 feet in length, 9 feet in width, and 8 feet in height, exclusive of the heaters.

FIG. 236



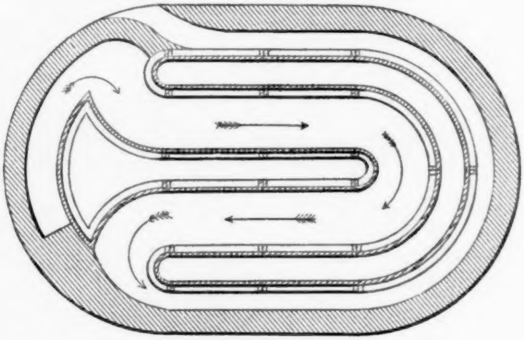
The second of these cast-iron boilers is, according to Mr. Graff, thus described by Mr. Latrobe. "The boiler has straight sides and semicircular ends; it is 17 feet long and 8 feet wide at the bottom, and 19 feet long and 10 feet wide at the height of 5 feet 7 inches. At this height it is covered by a vault, which, in its transverse section, is semicircular, and in its longitudinal section exhibits half its plan. The bottom is concave every way, rising one foot in the center. The fire-place is 6 feet long, and at an average of 4 feet wide, and is under one extreme end of the bottom. The fire-bed is arched parallel with the bottom, leaving a space of one foot high for the passage of the flame. At the end opposite the fire-place the flame descends along the bottom of the boiler, and, passing under an arch of fire-bricks, which protects the flank of the bottom, strikes the side of the boiler at its extreme end. Here it enters a flat elliptical flue, which, passing into the boiler,

FIG. 237



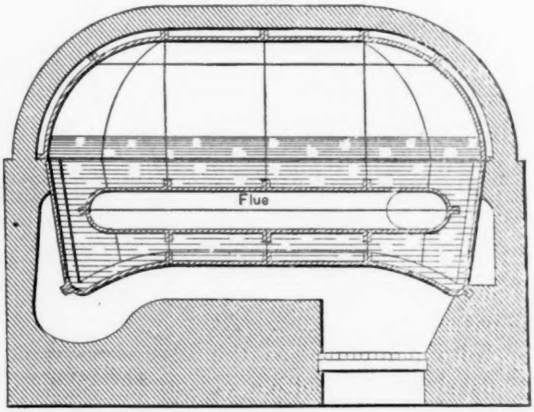
follows its form, returning again and coming out near the place at which it entered. The entering part of the flue is separated from the returning flue by a partition of fire-bricks. The flue, on coming out of the boiler turns short round, and is carried round the whole boiler until it enters the chimney." Fig. 238 shows a horizontal

FIG. 238



section of the boiler taken through the flue. Fig. 239 is a vertical longitudinal section taken through the center of the boiler; and Fig. 240 is a vertical transverse section of the boiler taken at the middle of its length.

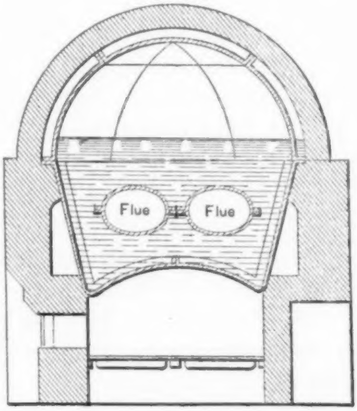
FIG. 239



Mr. Graff states that the three boilers just described "remained in use at Chestnut Street, Schuylkill, and at Centre Square, until the steam pumping works were started at Fairmount in 1815."

In regard to the Harrison boiler, one which I put up myself was, I think, the second Harrison boiler which was erected in the United States, and I believe that the first was put up in William Sellers & Co.'s works. I will ask Mr. Bancroft if that boiler is still in use.

FIG. 240



Mr. Bancroft.—We have one running in the same place. The original Harrison boiler was taken out after about six years' use, that is, new units were substituted for the old ones. The first boiler was put in about 1860.

Mr. Durfee.—It is well known to all of us who have had experience in blast-furnace management that it is usual to generate the steam in long cylinder boilers not only in this country, but in Europe also, and that there has been a great deal of ingenuity as well as money expended in devising methods of supporting these boilers so as to equalize the strain upon them. On taking charge of a Western Works some years ago I found a number of boilers 64 feet long and 42 inches in diameter, supported, or intended to be supported, by five cast-iron arched ribs, R, R, R, R, R (Fig. 218), spanning each boiler. There was no arrangement of

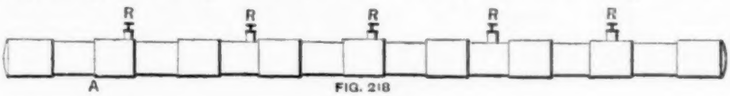


FIG. 218

FIG. 219

springs or balance levers to relieve or equalize the strain upon the boilers, whose ends rose and fell with the temperature of the gas that was burning beneath them. After I had observed this disagreeable fact for some few weeks, during which preparations were being made to remedy the difficulty, one of these boilers burst. It was probably one of the most fortunate accidents of the kind that ever occurred. The second seam in the head tore through the rivet holes for about one-third of its circumference at A, and the whole mass of water was emptied into the fire-box without further

damage. I was convinced before this accident, and its occurrence emphasized my conviction, that it was necessary to do something to relieve the strain on those boilers. I therefore cut each boiler in two at its middle and put a head in the adjacent ends of the two parts, leaving a space of four inches between the heads, and covered the top of this space with a curved plate, and connected the steam spaces of the tandem boilers with a copper tube, T (Fig. 219), curved vertically, and the water space with another copper tube, C, curved horizontally. Each of these half boilers was then supported by two yokes, Y, Y, and the boilers were fed as a single boiler with water. That was the system carried out with all the blast-furnace boilers at that Works, and it has since been adopted at several other furnaces in that vicinity.

This arrangement is believed to accomplish, with a much smaller expenditure, all that any elaborate system of spring or lever suspension can do.

Mr. Albert Emery.—In regard to the Harrison boiler, in 1865 I put in a Harrison boiler which I used at 180 pounds to a square inch. The boiler was tested for 600 pounds to the square inch. If I had carried it to 600, I suppose the stretch of the rods would have allowed the joints to open and the boiler to blow off.

Mr. Bancroft.—I may say that the Harrison boiler put in William Sellers & Co.'s shops was subjected to very severe service. We frequently carried the steam to as high a point as 160 pounds on it for a short time, then falling again to the ordinary working pressure; this ordinary high pressure being sometimes raised in the course of fifteen or twenty minutes. The boiler which we have had has been subjected to that kind of treatment ever since we put it in. We carry high steam pressure on it for sometimes two or three hours, and sometimes for only half an hour. The boiler was tested with hydraulic pressure, the last one put in going up to 600 pounds without showing any leak.

Mr. C. E. Emery.—Since, as Mr. Towne has remarked, this may be considered an historical paper, I wish to add my tribute of respect to the work done by Mr. Harrison. One of the early Harrison boilers was used in the experiments of the Government at the Novelty Iron Works during the war and operated very admirably. With respect to the safety feature spoken of, I did not know that the opening of the joints would take place at a pressure as low as 150 pounds.

Mr. Towne.—Will you pardon me a moment's interruption just to

clear that up? The point of pressure at which opening of the joints will occur, could be fixed at any desired limit by proportioning the diameter of the bolts to that pressure. In this particular case the bolts were probably of small diameter.

Mr. Emery.—I was temporarily somewhat startled one day, when the boiler had been lying with steam up under banked fires for some time, as the water was running out of the ash-pit over the floor. Upon quickening the fires for a little while the leak stopped. The strain on the rods and sections had evidently been adjusted to a practically uniform temperature in all parts of the boiler, and the water had lain still long enough to permit the water in the lower sections to become comparatively cold, so that the contraction opened the joints. There are some interesting features in connection with this boiler, independent of the fact that it was a pretty good boiler. Mr. Harrison was one of the first to construct special machinery for the duplication of parts in the steam-engine line. Several globes were cast together, with outlets at top and bottom of each, provided with finished rabbet joints. His machinery was so accurate, that whole slabs of these globes could be put together and made steam-tight by metallic contact, without the use of putty or any other substance.

I see with us here a gentleman who has very much to do with sectional boilers, to whom some tribute of respect should be paid for his success in that direction. I think Mr. Babcock, who has been so successful with his boiler, will be very happy to accord to Mr. Root the honor of having successfully developed a sectional boiler a little in advance of his own. There was some trouble with the joints of the original Root boiler, which I think has been overcome of late years. It must be acknowledged that many of Mr. Root's ideas are still considered necessary to the success of all forms of sectional boilers.

I recently had some very interesting experience in relation to the subject of marine boilers. I found in two small launches of the Revenue Marine Service, which had been running ten or twelve years, peculiar boilers which may be called modified Dickerson boilers. It will be recollected by many that, in the late years of the war, Mr. Dickerson, a patent lawyer, criticised very emphatically the steam-engineering of the Naval Department. His discussions show that he supposed the whole science of engineering depended upon Mariotte's law. In connection with Mr. Sickles, the inventor of Sickles "cut-off," he designed some ma-

chinery, which through the influence of friends was placed in naval vessels. Among other things, they invented a boiler with water tubes, somewhat inclined from the horizontal, located in the upper part of a large furnace inclosed with water walls. The gases passed upward between the inclined tubes, and then through superheating tubes in the steam space, as in an ordinary vertical tubular boiler. Some practical man left out the superheating tubes and made a practical boiler of it. I recollect that a number of portable engines made in New York had this type of boiler, the engine being attached to the side of the boiler. The apparatus was very powerful for its size. The boilers were known as "Little Giant" boilers, and one of these had been applied in each of the two launches mentioned. The condition of these boilers after some twelve years' use shows that at last we have found a really practical boiler for small yachts and launches. I have recently renewed one boiler on this plan, and put in another to replace a vertical boiler.

Mr. Stratton.—As this is to be something of an historical paper, I would like to pay proper tribute to the intelligence of the gentleman who made a success of this boiler, originally invented by Mr. Dickerson. A large number of them were made, and I saw a great many of them tested. Mr. Myers Coryell subsequently cut out the superheating tubes, and then called them the Little Giant, and made a success where they had previously been a failure. Following the lead of Mr. Towne, in referring to Mr. Harrison, I make this contribution to the skill of Mr. Myers Coryell.

Mr. Babcock.—I wish to discuss this paper a little as well as the question raised in it, why water-tube boilers are not more in use? One statement in this paper, I think, conveys a wrong impression. It is when speaking of cylindrical boilers: "This great length introduces a difficulty due to the unequal expansion of the top and bottom, which has been partially overcome by the use of springs for bearings." I do not think that springs for bearings overcome the difficulty at all. I have seen such boilers sixty feet long. Their average length is thirty feet, and they are usually supported in two places, sometimes in three or five. Where they have been supported in three places, I have seen them lift two inches at the ends, because of extra expansion of the lower portion. If cold water be pumped into them when the boiler is warm, they will take the other form and lift some distance in the center. Now, while spring bearings might equalize the strain upon the points of

support, they do not avoid the difficulty of the pulling stress and serious strains upon portions of the boiler. If the bottom of the boiler is hot and the top is cold, it forms a bow, and there is a line through the center which is very badly strained. The same occurs the other way under the opposite conditions, and the result is that these boilers by that continual strain become very seriously weakened along the central line, and therefore they are probably one of the most dangerous styles of boilers in existence.

There is another little point, in which the paper does not agree with itself. Of the internally fired shell boilers, it says on the fifth page: "This has been remedied in very many cases by the insertion of a number of drop tubes or cross tubes in the flues, which serve the double purpose of taking the heat out of the gas and also breaking up the currents, the result being a material saving of coal. The expense of thus inserting tubes in Cornish and Lancashire boilers has frequently been paid for in three months, by the saving of coal; the economy of coal being in some cases as much as two tons per week for one boiler; and after running two years, no diminution has taken place in their efficiency." Here is a wonderful statement in favor of water tubes. By merely putting some water tubes into a boiler an enormous saving is effected. But on page 12 it is stated that "John Elder made a boiler of inclined tubes 6″ in diameter for a working pressure of 500 lbs. per square inch, and found that it required four or five times as much heating surface per horse-power as ordinary boilers," leaving it to be inferred that it required necessarily more fuel. Why is it that water tubes are more efficient in one case than the other?

As I understand the definition of a water-tube boiler, it is a boiler in which water is confined within tubes, while heat is applied to their exterior. In that broad sense, a plain cylinder boiler might be classed as a water-tube boiler, if you could call so large a cylinder a tube; and, in fact, a number of such cylinders have been connected and styled a water-tube boiler. But any steam generator, if it be made up in whole or in part of water tubes, is a water-tube boiler whether it has a shell or whether it has not. Possibly the author of the paper has somewhat confounded the terms "water tube" and "sectional," which may refer to quite different things. A sectional boiler may be without water tubes, while a water-tube boiler may or may not be a sectional boiler. Mr. Kent has already spoken in regard to the statements of the author concerning com-

bustion in furnaces, that "John Elder found that furnaces two feet wide with water legs close to the fire were quite as good for the combustion of the gases as wide furnaces." This does not agree with modern practice. On the Government railways of Hungary many of the locomotives have had the water legs cut off and brick furnaces substituted with reported saving in fuel as well as equal or greater capacity in the locomotive; that is, the reduced surface has done more work with better economy, the fuel being burnt in a brick furnace, than when it was burnt in water legs. The same is true with marine boilers. The boilers of the steamship *Louisiana* have been fitted with furnaces of fire-brick, in place of the water legs formerly used, with a remarkable saving in fuel due to the superior combustion in such furnaces, without any decrease in capacity.

The author further states: "Very careful experiment made at Saarbrück *proved* that the boilers with the external furnaces lose at least 25 per cent. by heat passing through the masonry." This is a broad statement without qualification. I can speak more particularly respecting those boilers which I happen to know something about, and I have here a record of 20 tests of boilers set in brick, made at different times in various places by a great number of engineers. Some of these tests were made in Scotland, some in England, and some in different parts of this country, from Maine to California. A large share of them were made by the engineers of the works in which the boilers were placed, in their regular work for their own satisfaction, the length of the tests varying from 4 hours to 12 days, but averaging 36 hours to each test. During those 20 tests over 3,000 tons of water was evaporated, a great variety of kinds and qualities of coals were used, and all the tests, taking the total amount of water evaporated and the total coal burnt, gives an average evaporation of **11.292** lbs. of water, per pound of combustible. That is 75 per cent. at least of the value of the combustible burnt. Now we know that probably not less than 20 per cent. of the heat went up the chimney, and if 25 per cent. was lost in radiation, as stated by our author, there must have been at least 120 per cent. in the coal to begin with! The fact is that in well-set boilers not over 5 per cent. is lost in radiation.

Another statement made by the author of this paper is: "It will be noticed that the shell boilers which have been most extensively employed are all so constructed as to render it impossible to clean them by hand. This would seem to indicate conclusively that it is not necessary to the continuous and successful use of boilers, that

we go inside with scaling hammers." That is a generalization which will not be borne out by the facts. The reason why we find so many cylinder and two-flue boilers used in the West, where they have bad water, is, because it is absolutely necessary to have boilers that you can get into and clean. That is why comparatively so few tubular boilers are in use in the West; they soon fill up with scale and become worthless. A case in point may be quoted. A firm in New England who had been using a certain form of tubular boiler with great success for years in their own factory, having an interest in a factory in Indianapolis, sent out two of those boilers to be used there, because they were supposed to be much better than anything in use in Indianapolis. At the end of two weeks the first boiler was taken out; at the end of three weeks the second boiler came out; and they were never put up again. It was impossible to use them in water that scaled so badly. That same criticism will apply to the remarks in regard to the Field tubes or other forms of boilers keeping clean from circulation alone. It is absolutely impossible to have a sufficient circulation in a boiler to keep it always clean. That in some waters a rapid circulation does help to keep a boiler clean and does prevent scale in a measure is true; but in some waters it is utterly impossible to keep the boiler clean by any such means. The boiler which has the best circulation will scale up in some waters and require to be cleaned; in such waters the Field tubes do fill up and they fill up very badly. I presume there are members present who have had some experience with a boiler which was quite popular at one time, called the Wiegand boiler, which was made of Field tubes, as they are commonly called, but which, as suggested, are properly termed Perkins' tubes, Jacob Perkins having patented them in 1832. Those boilers in bad water were continually giving out in the tubes, and were quite useless in certain waters where scale would form in the tubes in spite of circulation. That this tug-boat spoken of ran for 18 months with dirty water and kept clean in the tubes is quite probable. Dirty water and water which causes scale are not necessarily the same thing. A good circulation will keep a tube clean in muddy water. Where mud is allowed to settle and bake it will form a hard scale, but it will form no scale in boilers which have a proper circulation, and a depositing point, with blow-off.

In regard to the failure of water-tube boilers on shipboard, under the circumstances it is not to be wondered at. The boilers of the steamship *Montana*, and her sister ship *Dakota*,

were failures, but any engineer should have known by looking at the drawing of those boilers that they would not last. And, in fact, they were condemned by the Board of Trade before they were fired, and the cause of their failure pointed out. There were several reasons why they should fail. The tubes, 15 inches in diameter and 15 feet long, were connected rigidly at their ends, and placed nearly horizontally, having a rise of only 1 in 20. No provisions were made for differences of expansion. None whatever for circulation. No equalizing pipe connected the steam ends of the several sections, while they were fed from a common water pipe, and therefore some parts were liable to become empty while others were full of water. About one-fourth the tubes, in the hottest part, were cut off from the rest and used for water heaters, with no adequate provision for the escape of steam. Under these circumstances it is not strange that the boiler gave out from unequal expansion in the trial trip. They were repaired, and gave out again. Some changes were made, and the ship started for a six days' trial at sea, but as three tubes gave out in as many days, all from the same cause, the boilers were condemned and others substituted. Those made for the *Dakota* were never fired.

The boilers of the *Propontis* were different, but had some of the same defects. A 21-inch cylinder over the fire had no adequate means of securing a constant supply of water to the highly heated surface. Nevertheless they ran for a year or two, with a greatly increased economy over the marine fire-tube boilers they displaced, finally giving out in those 21-inch drums from overheating.

Away back in the early years, during the evolution of economy in Marine engineering, the elder Rowan did wonderful things. He made triple expansion engines; using two such engines, or six cylinders in each ship. He put in very peculiarly constructed and not very well designed water-tube boilers with mechanical circulation. With high-pressure steam, large rates of expansion, and surface condensation which was quite new at that time, he succeeded in running a ship for less than half the coal that any one else could do at that time. One of his ships, the *Thetis*, tested by J. W. Macquorn Rankine, developed a horse-power on 1.04 lbs. coal per hour. He had his shop full of work, and built the machinery for many ships, every one of which is said to have fulfilled his guarantee of a horse-power for one and one-half pounds of coal. But they all came out of the ships afterward, not so much on account of the boilers as on

account of the general bill of repairs—the engines and the boilers together—and the surface condensers and the numberless pumps and other fixtures with which he produced his circulation. It was a mess of machinery which required a vessel nearly every time she came back to port to lay over a voyage for repairs.

As to water-tube boilers afloat, the Belleville boilers in the French Navy are notable examples. Mr. Emery has spoken of a boiler which was known as the Little Giant, patented by Farron, as an improvement on the Dickerson boiler in the *Algonquin*. The "Dickerson" boiler, by the way, was but a copy of a boiler made by my partner, Mr. S. Wilcox, some ten or more years before, merely adding the superheating tubes which, Mr. Emery says, when left off made it a good boiler. As far as I am aware, the boiler is in use to-day which my partner had made at that time, near thirty years ago, by the same boiler-makers and in the same shop where the Dickerson boiler was subsequently built. A vessel in the United States Government service belonging to the Quartermaster's Department, called the *Monroe*, had a water-tube boiler built by Babcock & Wilcox some eight years ago, which proved a very successful boiler.

Why are not water-tube boilers in more general use? is asked. Because they require a high class of engineering to make them successful. The plain cylinder is an easy thing to make. It requires little skill to rivet sheets into a cylinder, build a fire under it and call it a boiler; and because it is easy and any one can make such a boiler—because it requires no special engineering—they have been made, and are still made, to a very large extent. The water-tube boiler, on the other hand, requires much more skill in order to make it successful. This is proven by the great number of failures in attempts to make water-tube boilers, some of which are referred to in the paper under discussion. Water-tube boilers are not new. From the earliest days of the steam-engine, there have been those who recognized their advantages.

The first water-tube boiler recorded was made by a contemporary of Watt, William Blakey, in 1766. He arranged several tubes in a furnace, alternately inclined at opposite angles, and connected at their contiguous ends by smaller pipes. But the first successful user of such boilers was James Rumsay, an American inventor, celebrated for his early experiments in steam navigation. In 1788 he patented in England several forms of boilers, among them, one having a fire-box with flat water-sides and top, across which were

horizontal water tubes connecting with the water spaces. Another was a coiled tube within a cylindrical fire-box, connecting at its two ends with the annular surrounding water space. Another form in the same patent was the vertical tubular boiler, as at present made. The first boiler made of a combination of small tubes, connected at one end to a reservoir, was the invention of another American, John Cox Stevens, 1805—illustrated in the paper under discussion. About the same time, Wolf, the inventor of compound engines, made a boiler of large horizontal tubes, laid across the furnace and connected at the ends to a longitudinal drum above. The first purely sectional water-tube boiler was made by Julius Griffith in 1821, who used a number of horizontal water tubes connected to vertical side pipes, which were in turn connected to horizontal gathering pipes, and these to a steam drum. The first sectional water-tube boiler, with a well-defined circulation, was made by Joseph Eve in 1825. His sections were composed of small tubes slightly double curved, but practically vertical, fixed in horizontal headers, which were in turn connected to steam space above and water space below formed of larger pipes, and connected by outside pipes so as to secure a circulation of the water up through the sections and down the external pipes. The same year John M'Curdy made a "Duplex Steam Generator," of "tubes of wrought or cast iron or other material," arranged in several horizontal rows, connected together alternately front and rear by return bends. In 1826, Goldsworthy Gurney made a number of boilers which he used on his steam carriages, consisting of a series of small tubes bent into the shape of a U laid edgewise, which connected top and bottom with large horizontal pipes. These latter were united by vertical pipes to permit of circulation, and also connected to a vertical cylinder forming the steam and water reservoirs. In 1828, Paul Steenstrup made the first shell boiler with vertical water tubes in the large flues, similar to what is known as the "Martin," and suggesting the "Galloway."

The first water-tube boiler having fire tubes within water tubes, was made in 1830, by Summers & Ogle. Horizontal connections at top and bottom, had a series of vertical water tubes connecting them, through which were fire tubes extending through the horizontal connections, with nuts upon them to bind the parts together and make the joints, suggesting some recent patents.

The first person, so far as I am aware, to use *inclined* water tubes connecting water spaces front and rear with a steam space

above, was Stephen Wilcox in 1856, above referred to, and the first to make such inclined tubes into a sectional form was one Twibill in 1865. He used wrought-iron tubes connected front and rear by intermediate connections with stand pipes, which carried the steam to a horizontal cross drum at the top, the entrained water being carried back to the rear.

Time would fail to tell of Clark, and Perkins, and Moore, and McDowell, and Alban, and Craddock, and the host of others who have tried to make water-tube boilers, and have not made practical successes, because of the difficulties of the problem.

When we went into the business twenty years ago there was only one water-tube boiler which was made to any extent, and that was the Dimpfel boiler made in the vicinity of Philadelphia. It had a drum above, with tubes starting horizontally from a rear water leg and turning up into the drum. A number of those were made about that time. I think some of you will remember them. Some of them are in use to-day. At that time the Harrison boiler was in practical operation and was a success. I do not call the Harrison boiler a water-tube boiler; it is a series of globes. To Mr. Harrison is unquestionably due the credit of having brought the idea of a safety boiler into prominence before the world. There was no other sectional boiler in the market. Mr. Root came in a year after us, not, as Mr. Emery says, before.

Since then many water-tube boilers have been put upon the market, and after a brief existence have disappeared. I have made a list from memory, some of which promised well, but are now among the things that are past. No attempt is made to give them in the order of dates, but as they came to memory: Griffith & Wundrum, Dinsmore, Miller "Fire Box," Miller Inclined Tube, Miller "American" (vertical conical tube), Phleger, Wiegand, the Lady-Verner, the Allen, the Kelly, the Anderson, the Rogers & Black, the Eclipse or Kilgore, the Howard, the Moore, the Baker & Smith, the Renshaw, the Shackleton, the Duplex or Pond and Bradford, the Whittingham, the Bee, the Reynolds, the Suplee or Luder, the Babbit, the Reed, the Smith, the Standard. There are twenty-seven, which were all I could remember when I sat down to write this list, but I think it could be largely extended. You can see that many engineers have taken hold of the problem, which it is evident is not an easy problem to solve.

There is, however, a water-tube boiler which has come into very extensive use in the old country and into considerable use in this.

That is the Galloway boiler. It consists of a series of water tubes placed in the flues of a Lancashire boiler, and the water tubes are what give it its superiority. I think it is the best large shell boiler that is made to-day without any question, and the safest. The circulation of the water through the water tubes keeps all parts at comparatively even temperature, avoiding the strains of unequal expansion which are incident to the class of large shell boilers.

What advantages has the water-tube boiler that should cause it to come into use? Why should not the sheli boiler, with or without fire tubes, remain, as it has been, the principal boiler? 1st. They admit of the use of very thin metal with ample strength, and those thin surfaces may be exposed to the hottest gases with comparative impunity from burning and overheating, with the consequent strains and deterioration, and therefore the boiler is *better* and *safer*. A greater strength can also be secured in a water-tube boiler, that is, proportional strength. Of course, a water-tube boiler with a large shell like the Galloway boiler has no more strength than any other boiler with the same sized shell; but the water-tube boiler admits of being constructed with greater strength than a shell boiler. 2d. A better circulation of water can be obtained in the water-tube boiler than in any other. The best possible circulation is when there is a rapid flow of all the water within the boiler in one circuit continuously. A variety of circuits interfere with each other and destroy the circulation. That circulation which is possible only in a water-tube boiler is an important element of safety, economy and durability. It keeps all parts at an even temperature, delays deposits, and sweeps away the steam as fast as formed. 3d. A water-tube boiler can be made sectional, a point of great importance for safety. Dr. Alban is credited with being the first to ennunciate the truth that "a steam generator should be so made that the giving out of any portion should not cause a disastrous explosion." As I said before, a sectional boiler may not be a water-tube boiler, but a water-tube boiler may be a sectional boiler, and for that reason a water-tube boiler is better than a shell boiler, because it can be made sectional and therefore safer.

When I was at John Elder's yard, in Glasgow, last summer, Isaw an acre, more or less, of ground covered with boilers for the two ships the *Umbria* and the *Etruria*. They were 16 feet in diameter, made of $1\frac{1}{4}$ steel plates, calculated to carry 120 pounds working pressure per square inch. It was fearful to contemplate such an array of enormous vessels under such a pressure shut up in a steam-

ship where an explosion would inevitably cause great loss of life.

4th. A water-tube boiler possesses the possibility of a longer life if it is properly made. You burn a sheet of a shell boiler and put on a patch or a new sheet, and you have a boiler which has been deteriorated largely by the strain due to that overheating. You are always distrustful of that boiler, and can never consider it as reliable as if it had not been overheated. On the other hand, you may take a water-tube boiler properly constructed and overheat it to any extent you please; replace the overheated parts, and the boiler is just as good as it was when it was new. The life of such a boiler may be indefinitely prolonged by simply replacing worn-out or burnt-out parts. As I said before, we have been making water-tube boilers for twenty years. Our earlier experiments were imperfect, and we do not pretend to say that we succeeded in making an absolutely perfect job, by a long way, when we commenced. I do not know that we have an absolutely perfect job yet. But what I was going to say was that in all that twenty years I have never known one of those boilers to wear out. Some have come out of use because of failure of firms and their going out of business and a few things of that kind, but I have never known one of our water-tube boilers to be discarded because it was worn out. The first boiler we made of approximately the present form in 1869, is still in use and good to-day. 5th. Another element of value in a water-tube boiler of the sectional form is ease of transportation; it can be taken apart and shipped in small pieces; it can be carried on a mule's back, if necessary, over the mountains; it can be taken in through a window; it is not necessary to tear the side of a house down to get it into its place or out. For ship use that is of great importance, for it may be put down the smoke-pipe hatch in pieces and put together in place, and it may be taken up a piece at a time and repaired, thus saving the expense now necessary in cutting up the decks and cabins of a vessel in order to get the boiler out. 6th. Another element of value possessed by the water-tube boiler over the ordinary flue or tubular boiler is greater efficiency of surface. The gases can be made to cross the surfaces and impinge thereon, whereby a given surface is found to be of more value than when the gases glide along parallel thereto. 7th. The water-tube boiler is also capable of perfect cleaning. We are told by Mr. Stirling that most of the boilers in use to-day cannot be cleaned at all, and that, therefore, it is not necessary to clean them. But, as I

said before, it is very important, and the fact that a water-tube boiler can be cleaned from the very worst scale is a very great advantage in favor of that construction.

What are the difficulties with the water-tube boiler? Well, one difficulty is to know how to make them! As stated above, there have been many attempts and many failures. A prominent difficulty is to so connect the parts that unequal expansion shall not tear the boiler to pieces nor produce leaks. Bolted and packed joints have been the rock on which many have foundered. Want of proper circulation of the water has also caused many failures, as in the *Montana*. Another cause of failure has been too small internal capacity per horse-power. The worst case I remember was a boiler made by Babbitt, in which he used $\frac{5}{8}''$ tubes and tried to get a boiler strong enough to drive a good sized boat and small enough to be packed in an ordinary dry goods box. We hear a good deal said by old engineers about the steam space—that a large steam-space is required to allow for fluctuations. But when you consider the small weight of a cubic foot of steam you will see that the steam space bears a much less important place in the boiler than is generally accorded to it. For instance, with an engine working 100 horse-power with 80 lbs. steam, it would require 2,700 cubic feet of steam space, in order that the pressure should not drop more than 10 lbs. in one minute, if no steam was generated for that minute. The ordinary amount of steam space is, therefore, a very small factor indeed in making up for fluctuations. The most important function of the steam space is to insure that the escaping steam shall not take up the water. Steam flowing out through a pipe will pick the water up in large quantities, unless the surface of the water be two or more diameters below the end of the pipe, depending somewhat on the velocity of the flow. The surface of the water must be far enough away from the exit pipe to prevent such an action, and there should be sufficient surface to insure that the water carried up by ebullition has time to separate from the steam, and that is all the steam space that is necessary. A large water surface is also important to avoid rapid fluctuations in water level. Cubic feet of water capacity is a more important element in providing for fluctuations in supply and demand. In the case above supposed, 108 cubic feet of water space would be the equivalent of 2,700 of steam space, that is, the steam generated from that water by a reduction of pressure from 80 to 70 pounds would keep a 100 horse-power engine running one minute. From these figures an approx-

imate estimate can be formed of the amount of steam and water room required in a boiler.

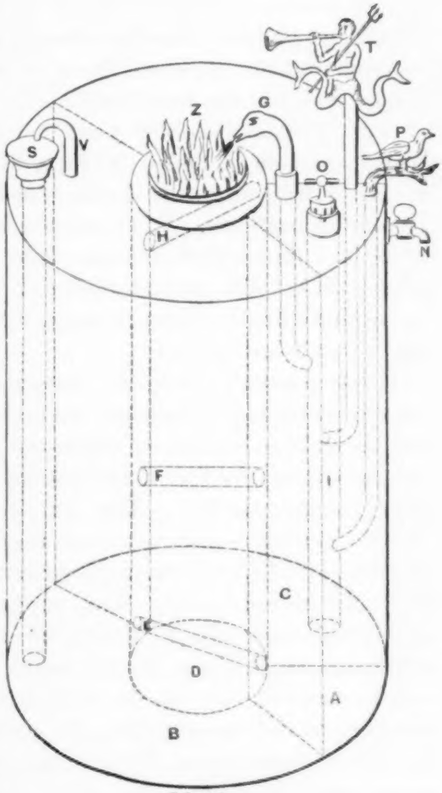


FIG. 218

The point spoken of by Mr. Towne—the stretching of bolts as an element of safety—used to be a favorite idea with Mr. Harrison, and also with most of us in those early days of sectional boilers. We thought it quite an essential thing, and we made our boilers in sections bolted together with long bolts, so that under high pressures the bolts would stretch and allow the pressure to blow off. But we found that they would stretch when we did not want them to, and if there was any dirt in the water it would get between the surfaces and prevent the joints closing again, so that there would be a permanent leak, and that matter of long bolts has entirely gone by. I do not know of any boilers being made with them now expect a few Harrisons.

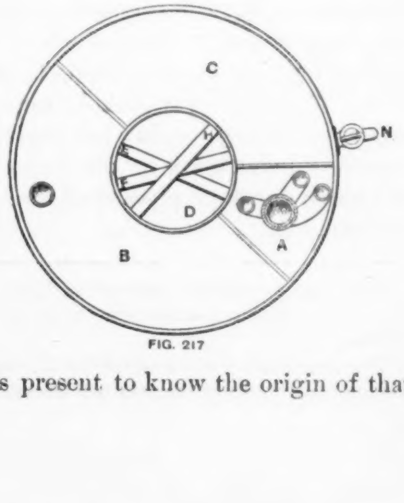


FIG. 217

Mr. Durfee. — As we have gone pretty exhaustively into the history of boiler construction, and the Galloway tube has been mentioned, I think it may be of interest to the members present to know the origin of that

idea of putting a water tube across a flue. It originated at the time when probably the first steam boiler ever made in this world was constructed. I do not know when the first steam boiler was constructed, but the first steam boiler that I have found any record of was made at least 200 years before the year *one* of our era. Its construction is shown in Figs. 216 and 217. The first figure is copied from the first Latin translation* of the Pneumatics of Hero of Alexandria. It represents a perspective elevation of the boiler and its appendages, and shows its internal construction by dotted lines. The second figure (Fig. 217) I have drawn myself to facilitate explanation; it shows a horizontal section of Fig. 216 taken just below its top.

The apparatus consists of a vertical cylindrical shell, whose ends are closed by heads, through the center of which passes a vertical cylindrical flue, D, whose upper end is provided with grates for the support of the fire, Z, the hot gases from which passed downward through the flue. The space between the flue and shell is divided by diaphragms into three unequal compartments, A, B, C, in the first of which steam is generated, the others being simply reservoirs of hot water. The central flue, D, is crossed by three cylindrical tubes, H, F, E, the tubes H, F, connecting the hot-water spaces B, C, act in the same way as the Galloway tubes now in common use, but the bottom tube is closed at the end, E, its opposite end opening into the smallest or steam compartment, A. The compartment B is provided with a funnel, S, whose tube extends nearly to the bottom of the boiler; and also with a safety tube, V, whose curved upper end is immediately above the funnel, S. The compartment C has a cock, N, from which the hot water is drawn. The compartment A has within it a three-way-cock, I, the three discharge pipes of which are connected with the goose-neck blow-pipe, G, the triton, T, and the singing-bird, P, respectively. The three-way-cock, I, is operated by a cross handle, O, and the upper end of its plug has graduations which when brought opposite an index mark on the shell of the cock determine which of the three discharge pipes shall receive the steam generated in compartment A.

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* Heronis Alexandrini Spiritalium Liber. A Federico Commondino Urbinate, ex Græco, nuper in Latinum Conversus: cum privilego Gregorii XIII. Pont. Max. Urbini, 1575.

Hero lived and wrote about 200 B.C., and there are several Greek manuscript copies of his works in the principal libraries of Europe.—W. F. D.

The principal function of this apparatus was to furnish hot water, and it is so contrived that it is *impossible* to draw any considerable amount of hot water from the cock, N, without putting in an equal quantity of cold at the funnel, S. In order to put this apparatus at work the compartments B and C were filled with water to a level above the upper water tube, H, by means of the funnel, S; the goose-neck, G, was then removed and enough water poured into the compartment A to fill it nearly to the lower end of the three-way-cock, I; the fire was then lighted, and as soon as steam manifested itself the goose-neck, G, was returned to its socket and placed in such a position that the fire, Z, was blown by the issuing steam. The three-way-cock, I, could be turned by its handle, O, so that the steam would cause the triton, T, to sound his trumpet, or the bird, P, to warble, and thus announce to interested parties that the water was "boiling hot." In case any steam generated in the compartments B and C, it found an exit through the safety pipe, V, and any entrained water re-entered the boiler through the funnel, S. In case it was desired to draw hot water in any great quantity from the cock, N, it was necessary to supply an equal amount of cold water through the funnel, S, this requirement insuring a constant volume of water in the boiler. This boiler must have been placed over a descending flue. I think there are some features about this oldest of boilers that are not unworthy of imitation.

Mr. Stratton.—With reference to this paper of Mr. Stirling's, I would state in relation to the early water-tube boilers, as referred to therein, that the great difficulty which existed then was found in getting the flues in or out of such boilers, that is to say, it was not possible to insert or draw them through the head after the boiler was once constructed, for at that date the only way of making a tube was to rivet it up of sheets of iron, and were it not for the perfection of the art of lap-welding flues and tubes it is doubtful if we would have many water tubular boilers at the present day. A boiler very often looks fine and works well until you come to repair it; but as soon as that time arrives, from the inability to repair it unless at great expense, the boiler fails. This has been the history of the majority of the water tubular boilers which we have seen introduced.

As regards the locomotive boiler, which is referred to as having done great and good service and stood the test of time well, I would say that the locomotive tubular boiler has and still is doing good service in many instances, but in marine practice it is now found

impracticable, owing to the large amount of flat surfaces that require bracing fo high pressures; the locomotive boiler generates steam rapidly and satisfactorily as long as you do not attach it to a surface condenser, or return the water of condensation and the impurities and oily properties to it which are obtained from the surface condenser by its action on the oils used for cylinder lubrication. But when those cylinder lubricants are introduced into a locomotive tubular boiler they invariably affix themselves to the flat surfaces, and particularly to the crown sheets, almost immediately causing them to bulge between the sockets or stays. I now have three boilers of this locomotive type in New York, which were made as finely as it is possible to have them built, and yet under such conditions they gave out in use, and they were taken out and replaced by water tubular boilers.

As regards the Scotch type of boiler, referred to on the sixth page, there are a great many difficulties attached to it, some of which have already been referred to. First is its great weight. These boilers are generally made from 10 to 12 feet in diameter and 9 to 10 feet long, and made to carry from 60 to 120 pounds of steam. But when they reach a diameter of 12 to 14 feet the weight will approximate, including the water, 65 to 70 pounds weight per foot of heating surface, which is largely attributable to the great amount of dead water which is contained in these boilers below the line of the furnaces. The cylindrical form of construction is carried out in order to maintain a uniformity of structural strength. Another of the difficulties which also attaches to this form of boiler is the size of the furnace, which seems to be arbitrary as to diameter in a great majority of instances, from the fact that the grate bars pass across at or about the center of the cylindrical furnace, in order to admit the proper amount of air under the grate of the furnace, the space existing over the fire is very limited, and therefore the products of combustion do not have the proper opportunity to burn in order to produce the desired effect before passing through the tubes. These boilers, in connection with the surface condenser, are now causing engineers generally a good deal of trouble from another cause, namely, the great thickness of the furnaces, which has to be from $\frac{5}{8}$ to $\frac{3}{4}$ of an inch thick in order to be sufficiently strong to resist the pressure. The best cylinder lubricant known is mineral oil, and as it carries a large percentage of wax with it, commonly known as paraffine, after the volatile properties of the oil have been evaporated in the cylinder the residuum passes into the boiler, and it has

a chemical affinity for the impurities of the water, and particularly for any saline matter that there may be in the water within the boiler. This affixes itself very strongly to the heating surfaces, and it seals the water, as it were, from the heating surfaces; under those conditions there is nothing left for the furnace to do but come down under high heat and great pressure. I have seen on one of the largest steamships a furnace that internally was like a bath-tub; forced in under such conditions that a man could lie right in it, and this is a difficulty which engineers have to deal with continually in this class of boiler. This subject is now agitating some of the best minds in the engineering profession. The great weight of this boiler has reached a point when it is considered excessive, and engineers are anxious to know how it is best to get over it; the only way seems to be by the use of water tubular boilers, from the fact that the same results can be produced with a boiler weighing 40 pounds per foot of surface as compared with 60 to 65 in the Scotch boiler.

Mr. Shock in his book on marine boilers has made special reference to taking three thicknesses, and I think in some instances as many as four, of thin boiler steel and riveting them together, and making the boiler of three or four thicknesses of thin steel in order to overcome the difficulties that we now experience in making these boilers of such thick metal as an inch and an inch and a quarter. The difficulty which is attributed to this class of iron or steel is, that it is not sufficiently compressed at the center under the rolls to make it perfectly homogeneous in the central parts of the sheets. This suggestion of Mr. Shock would seem to overcome the difficulty to a certain extent, but should there be a leak between two of these sheets it would tend very soon to produce exfoliation between them, which would seem to be an inherent difficulty with that class of construction.

In referring to the matter of the waxy properties affixing themselves to the heating surfaces of the Scotch boiler, I will say that they are overcome in the water tubular boiler in this way: the circulation in the water tubular is in one single direction. The current of circulation rises at the front end, and descends at the back, all the agitation there is from the rapid circulation of the water is found within two feet of the end of this drum, the steam being taken out through a dry pipe from the back end of the boiler, which renders it unnecessary to have a large superheating chimney as is adopted in most of the Scotch type of boilers. Then the descend-

ing current going down through the rear leg gives ample opportunity for the impurities of the water to settle, and they ultimately find them deposited in the mud drum at the bottom. I had a conversation recently with an engineer in New York who had tried this experiment, and found the deposit in the mud drum as described.

Mr. Stirling in his paper has made reference to the fact of *his* not knowing of a single water tubular boiler being in use in a commercial way on any of the great commercial highways. I would call his attention to the steamer *Ortogal* of the Messageries Maritimes. She runs between Marseilles and Rio Janeiro. Myers Coryell, Esq., who is now in Europe, has corresponded with me since he has been there on this subject, and has made a close examination of this vessel and the methods of construction of the Belleville boilers, and he states that the performance is very satisfactory indeed. As another reference in the same direction, I would call attention to an issue of the *Mechanical Engineer* of May, 1885, wherein reference is made to the *Milan*, a vessel recently constructed in the French Navy, having Belleville boilers. She has recently been under trial for over a month. The tests demonstrate good economy. She is believed to be the fastest war vessel afloat. In a six hours' speed trial she made an average of 18.4 knots per hour. The engines develop 4,000 horse-power. These boilers are of the sectional water tubular type, weighing many tons less for the same horse-power than the usual cylindrical shell boiler. This latter saving being principally caused by the less quantity of water carried.

Mr. Barrus.—Mr. Stirling says in regard to the vertical tubular boiler that it has these objections—" That part of the heating surface is above the water level and is liable to be injured by the fire, that it has small area of water level, and that it is difficult to clean and repair." Then he goes on to say, " many modifications of the vertical boiler have been made, but they are for small powers and have not come into general use." There are quite a number of vertical boilers in use in New England cotton mills. They are mainly of the Corliss type, and in a 140 horse-power boiler it would consist of the main shell, which is in the neighborhood of 7 feet in diameter, with a dome 30 inches in diameter, 6 feet high at the top, and a water leg of the same dimensions at the bottom. The tubes, which are 10 feet long, lie concentrically around the outer part of the main shell. There are in that boiler 248 2-inch tubes.

What I wanted to call attention to was the fact that the boilers are now in use, and have been in use a great length of time. I have asked a number of people who have used them how they liked them, and they say that they have given every satisfaction and have caused no trouble. I know of one instance where they were about to put in some new tubes, and I asked how long the old tubes had been in, and they said twelve years.

The question what are the advantages of water-tube boilers over shell boilers, is one which I would like to speak about. I have tested a number of boilers of both types. I have found that, working under economical conditions, the water-tube boiler is more economical than the shell boiler, and I wish to suggest one reason why it should be so. In a properly designed type, the course of the products of combustion is from the furnace through and along the tubes and out to the chimney. The course of the feed water is first from the chimney end of the drum down into the vertical tubes near the chimney, then forward through the inclined water tubes, and finally back to the drum. Now the cold water coming in at the chimney end of the heating surface, would first strike that portion of the surface where the escaping gases have the lowest temperature. Consequently, a higher difference of temperature between the water and the escaping gases is secured, and the heating surface is more efficient than in other types of boilers.

Mr. Jones.—I would like to say a word or two in regard to the vertical boiler just referred to. I think that care should be taken in making statements before this Society, but I venture to put myself on record as making the statement that a vertical boiler is not justifiable in any case, unless no other form of boiler can be applied. The statement made that these boilers were of use in the New England States is correct, and it is practicable to use them there, because there they have pure waters. The Cambria Company were persuaded to put a boiler of that kind into their works, and in the course of a year it had to be taken out. It is a form of boiler that should never be put in by an engineer when any other can be used.

Mr. A. H. Emery.—Before the matter is closed, I would like to refer to a remark that Mr. Babcock made. He said that one advantage of the sectional boiler over the shell boiler, was that the shell boiler had to have the side of a house taken out in order to be taken in; but I would mention a greater advantage which the sectional boiler has in that it does not itself take out the side of

a house when it is to be removed, even if it has burst in some part.

*Mr. Stirling.**—The hope expressed in the paper, that the "general subject of boilers" would be freely discussed has been realized. One of the speakers ventured to express the opinion that "discussion does not lead to an advance." We find, however, by reference to Art. I. of the Rules of our Society, that one of its aims is "to promote the arts by means of the reading and discussion of professional papers." There can be no doubt that an interchange of views between men of such extended experience as Messrs. Emery, See, Babcock, Durfee, Bancroft, Barrus and others, must have a tendency to further that progress in arts, which is declared to be one of the prime objects of our Society.

The method of curing the trouble with long cylinder boilers, illustrated in Figs. 218 and 219, is very effective, and no doubt is superior to any system of springs or levers.

The question whether we have reached the limit of pressure in the Scotch Marine Boiler, has been referred to; and when we find such a man as Mr. See taking the ground that very much higher pressures can be carried by that type of boiler, we must admit that the question is unsettled. It will be interesting to watch the developments in this direction. Attention is called to this gentleman's statement, that it does not matter much whether the surfaces of a boiler are flat or circular, provided the flat surfaces are well stayed.

The account given of the behavior of the boilers of the New York Steam Company is interesting and instructive.

The attention of the speakers who referred to the advantage of the long tie rods that were used in Harrison boilers for preventing explosions is invited to the remarks of Mr. C. E. Emery and Mr. Babcock on this subject. The trouble with these tie rods is given as the reason why the Harrison boilers failed so completely in England when tried there a number of years ago.

One of the gentlemen said that he did not believe the statement that boilers made of flat plates, as illustrated in the paper, Figs. 145 and 145*a*, had ever been built. Boilers of this type were built by Mr. D. Davies, of the Crumlin Viaduct Works, near Newport, Monmouthshire, England. In March, 1876, one then had been in use for thirteen months at these works, where it took the place of two Lancashire boilers, and up to that time had given no trouble.

* See Proceedings, XIth Meeting, page 376.

This boiler was thoroughly tested and found to evaporate over four pounds of water per hour for every square foot of heating surface, with an evaporation of over nine pounds of water per pound of coal.

This speaker also said that the use of "riveted flues or large lap welded tubes" in the West is due to "prejudice," and that Mr. Holloway, our President, says it is the result of "experience." After reading Mr. Babcock's remarks on this subject, the members can judge for themselves who is most likely to be right in this matter. Mr. Kent illustrates, in Fig. 224, what he is pleased to call the evolution of the steam boiler, but the locomotive boiler is not found in this illustration, although it is probably the one whose use is most extended. He says the locomotive boiler is a troublesome and a dangerous thing, but does not offer us anything better.

Some of the speakers objected to the generalization in reference to the "Field" tubes. This "generalization" was warranted by the fact that there are over 1,500 "Field" boilers in use, and that over 140,000 "Field" tubes have been supplied for insertion in existing boilers. These boilers and tubes are distributed all over England where the water is exceptionally bad. Several 50-horse-power "Field" boilers can be shown, each occupying a floor space of less than forty square feet, which after eight years' hard work are evaporating 10.93 lbs. of water per pound of coal. The experience of twenty years proves that there is no trouble with scale in the "Field" tubes; the circulation is so violent that the sediment is not allowed to adhere, and the other parts of the "Field" boiler can be kept clear of scale in the same way as other boilers are. The attention of the gentleman who referred to the "Wiegand" boiler is called to the following points: 1st. The "Wiegand" boiler differs from the "Field" boiler in several essential details; so it is not surprising that, while one is a great success, the other should be a failure. 2d. When economical results from the introduction of water tubes were spoken of, the reference was to the "Field" tubes; and when reference was made to John Elder's experiments with water-tube boilers, it was to boilers with inclined tubes; so that there was no contradiction, the statements referring to two different things. The Marine Engineers to-day look to John Elder as one of the fathers of their profession, and the writer of this paper joins with them, and will receive any statement given on his authority with great respect. It is altogether likely that these experiments of Mr. Elder have had much

to do with the exclusion of inclined water-tube boilers from the commercial marine. Some fault has been found with my generalization as to the absence of water-tube boilers from commerce, but very few exceptions have been cited, and they on comparatively untraveled commercial highways. We are referred to a fast steamer of the French Navy, just out, with water-tube boilers; but the French yacht *Hirondelle* made sixteen miles an hour with Belleville boilers fifteen years ago. We are also told that the United States Goverment steamer *Monroe* "had" water-tube boilers. Why is the past tense used? What has become of them?

Reference has been made to Dickerson's boiler as likely to work well for marine purposes. No doubt the removal of the superheating tubes would be an improvement. And the evidence is clear that with that improvement it has done well for small light work; but it unfortunately belongs to the class of "inclined tube" boilers which John Elder experimented with and condemned.

Promises have been made regarding the future of inclined tube boilers; but the trouble with all this class of boilers is that several of the tubes discharge their steam into the steam space through the same header. This feature will prevent their ever being driven up to the capacity per square foot of heating surface of boilers like the Scotch Marine or locomotive boilers. By reference to the illustration in the paper, it will be seen that the tubes in the boilers of the *Montana* were slightly inclined; and while it is true, as one of the gentlemen stated, that they failed because of unequal expansion, this unequal expansion was caused by the headers being insufficient to carry away steam as fast as it was generated. The velocity of the the current in these headers was most violent, a complete rush instead of a steady flow. The force of the current was so great that bolts, rivets, etc., were carried up by it. With comparatively light firing, our modern inclined water-tube boilers do very well; but placed over a heavy fire with strong draught, the result would be as disastrous as in the *Montana*. Instead of the steam getting away immediately to the steam space, it has to twist itself through the numerous bends of the header, joined at every step by more steam issuing from the tubes above, the neck nearest the top being compelled to convey the steam generated in the whole of the tubes below it. When the inclined water-tube boiler is placed over a strong fire, such as the locomotive men find necessary, it will be found that the steam chest and lower water tubes have changed places, and that it would

be advisable to fix the stop-valve to the lower water tubes; for it would be found that as much steam could be got there as anywhere else. The water would be converted into steam faster than it could be replaced by the ordinary course of circulation. In the locomotive or the Scotch Marine boiler, there is ample area for free circulation both for the water and for the escape of the steam, and this is probably the main reason why these boilers have proved so satisfactory.

As to the list of water-tube boilers that have proved failures, there are two exceptions that should be made; one the Whittingham, which is still in use, and the other the Shepherd vertical conical tube which is still being manufactured.

One gentleman tells us that the failure of the *Propontis* was due to her three-cylinder engine; but the trouble was really with the boilers, as is shown by the fact that there are many three-cylinder engines working now successfully in connection with the Scotch marine boiler.

Some of the speakers have criticised the reference in the paper to the matter of cleaning. One of them says: "We are told by Mr. Stirling that most of the boilers in use to-day cannot be cleaned at all, and that therefore it is not necessary to clean them." This statement was never made by me either in the paper or elsewhere. What I said was that it was impossible to clean thoroughly by hand with scaling-hammers. Locomotive men do not get inside of their boilers to scale them, and even when inside of a tubular boiler I have found it impossible thoroughly to clean the center tubes of a group. We all know that boilers must be kept clean; but it is coming to be better understood that one of the most effective means of keeping boilers clean is to use a compound that will dissolve the scale or prevent it from hardening, to take advantage of the abrading and transporting power of water in rapid circulation, and then blow out the scale at regular intervals while under steam. There are cases where the water is so very bad as to make it necessary to go inside, and one speaker says that such water prevents the use of tubular boilers entirely.

One of the speakers stated that the water-tube boilers referred to in the paper as failures, had failed because the tubes were riveted; and, also, that the wax in oil causes much trouble in boilers working with surface condensers. The fact is, that the water-tube boilers illustrated in the paper were built about twelve years ago of welded tubes; and that the wax in oil gives no serious trouble is evidenced

by the fact that boilers attached to surface condensers often work continuously for weeks at a time without difficulty.

The Corliss boilers, to which reference was made in such an interesting way, should have found a place in the paper; and Mr. Barrus is entitled to thanks for bringing them forward in the discussion.

As to the leakage through the masonry of externally fired boilers, the loss is not always 25 per cent., but no doubt it is as high as that in many cases, particularly where plain cylinder boilers are used. 11.292 lbs. of water per pound of coal is a very high evaporation, as the gentleman who made the statement knows. In many cases it is very much less than that, and would leave 20 per cent. to go up the chimney, 25 per cent. for radiation, and some to spare.

This gentleman took exception to the classification in the paper of shell and water-tube boilers, but he did not mend matters by classifying a plain cylinder boiler as a water-tube boiler.

Two of the speakers have advocated externally fired boilers, and have referred us to some German locomotives and to the steamer *Louisiana* to sustain their position. It is very strange if there is any advantage in external furnaces for locomotives, that none of the British or American locomotive men have found it out. As to the reference to the *Louisiana*, her designer, Mr. John Baird, was one of the best engineers in the country, and she has from the first been known as a swift and economical steamer. Before giving, however, so much credit for economy to the substitution of brick walls for water legs, we should have a record of careful tests. John Elder's experiments were no doubt made with care, and his results as stated in the paper can be depended upon.

In conclusion, I wish to say that the additional statements made by me in this discussion have been gleaned from various sources and no claim is made for originality.

CLXXIX.

THE ADAPTATION OF STEAM-BOILERS TO WARMING DWELLING-HOUSES.

BY JOHN W. ANDERSON, SOUTH BEND, IND.

THE requirements of boilers for warming houses are so different from those used for supplying power, that there are none of the celebrated boilers which have excited so much interest that are suitable for use in the house we live in.

For this purpose a boiler should be safe; not only safe from explosion but from discharging poisonous gases into the house.

It should be durable; so durable that one boiler will last an ordinary life-time with slight repairs on grate, etc.

It should be economical in fuel; so that warming a house by steam may cost less than by other methods.

It should be easily accessible for cleaning, and attended by so little trouble that the members of the family or servants may have no dislike to keeping it in order.

It should be self feeding, and so nearly automatic in operation that only a few minutes' attention morning and evening will be necessary to keep up a regular pressure of steam.

It should be so simple in construction and management, that any person of ordinary intelligence can understand it and run it successfully.

Much thought and considerable money have been expended in the effort to construct boilers that would fill these requirements; and some valuable progress has been made during the last ten years.

It would be interesting to follow the course of these experiments and note the results, but as that would be impossible in a short paper I will confine myself to one that has come prominently under my notice. It is such a radical departure from the ordinary construction that I think it worthy of consideration, and as I am not the inventor, I feel freer to point out its merits.

This boiler has scarcely yet been introduced to the public, and still it has been well tested under a variety of conditions during the last two winters.

The boiler may be classed with the type known as water-tube boilers; most of the water being contained in the tubes, and the action of the heat is principally on the outside of the tubes.

Referring to the accompanying Figures, 200 and 201, which are taken from a boiler which is designated as size No. 4—A is a circular drum 36″ diameter by 18″ deep. The shell and upper

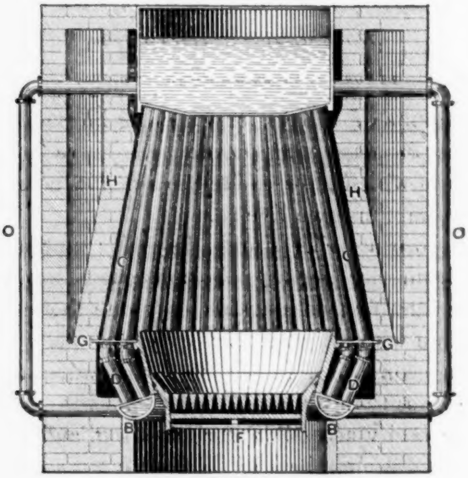


Fig. 200.

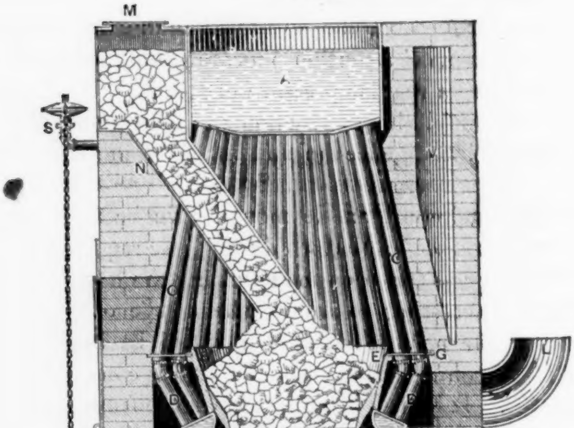


Fig. 201.

head of this drum are ordinary boiler iron. The lower head is cast-iron 1″ thick. This cast-iron head has a raised boss at the center on the upper side to receive the screw-end of a bolt, which runs down through the upper head making a very effective stay-

bolt, and is pierced by eighty holes in two circular rows, staggered, tapped for one-inch (inside measure) tubes. These tubes are 46″ long and flare outward toward the lower ends.

The bottom of the boiler consists of an annular cast-iron drum or tube head, B, which is tapped for the same number of tubes as the head of the upper drum.

The tubes in the annular drum are 10″ long. They are united to the upper tubes by 45-degree ells. These 45-degree ells are of special pattern, extra heavy, and they, as well as the drums, have all the holes counter-bored the size of the outside diameter of the tubes, so that all the threads on the tubes are completely covered.

The faces of the drums are at a suitable angle to take the tubes at right angles.

The annular drum, B, has bosses on the outside to take all the necessary connections, as circulation tubes, feed and blow-off pipe, etc.

The tubes are omitted at a part of the front of the boiler, leaving an opening for a door to reach the inside and for a chute to carry the fuel to the grate. The fire-pot is a heavy cast-iron flaring ring, resting on the inside upper edge of the annular drum, B, the top being slightly higher than the top of the 45-degree ells.

At the top of the 45-degree ells are a row of flat plates which we will call flame plates, each having two holes, slightly oblong, which fit loosely around the tubes, with a washer on each tube that lays on the flame plate to make a close joint. The inner ends of the flame plates rest upon a flange on the fire-pot and the outer ends in the masonry which encases the boiler. The flame plates form a partition which divides the fire chamber from the tubes below the bend.

The space which surrounds the lower portion of the tubes under the flame plates is utilized as a draught flue, and is connected to the fire chamber by flues running down at an angle through the masonry from the center of the upper drum.

Two tubes outside the masonry are connected to the upper and lower drums, which maintain a good circulation of water if all the heating pipes are shut off.

The fuel magazine is an iron box, with a close fitting cover, about the same height as the upper drum, and of sufficient length to hold the desired amount of fuel. The width is right to set against the drum and come flush with the brick-work in front, about 14″.

A cast-iron tube of suitable size forms an inclined chute from the bottom of the fuel magazine to the center of the boiler over the fire-pot, keeping the grate supplied with fuel. The inclined chute, N, passes in over the front door through the space between the tubes. The grate, which swings inside the annular drum, has very convenient means for shaking and dumping by levers placed outside the boiler front.

The boiler is inclosed in brick walls which are built hollow and supplied with a sufficient number of cleaning doors to reach all parts easily for the purpose of clearing it of soot and ashes, and also giving it a free circulation of air when not in use. It is easily freed from soot by using a small hose for throwing a jet of steam against the tubes.

This construction admits of a fuel feeder without cutting away the center of the upper drum, thus obtaining a larger water and steam space in a boiler of given size. It affords a high and spacious fire chamber which gives an almost perfect combustion of the fuel, there being very little deposit of soot upon the tubes or escape of smoke from the flue.

The bend in the tubes permits the use of screw threads for uniting them to the drums, which has much greater strength and durability than the expansion of tubes in smooth holes.

The bend also allows the tubes to expand and contract lengthwise without straining the joints, and permits the use of smaller tubes, thus subdividing the water into smaller columns and obtaining more efficient action of the heat, resulting in economy in the generation of steam.

The circulation of the water is very perfect, there being no dead end at any part of the boiler. The water flows in at the lowest and coolest part and rises steadily through the gradually increasing heat to the upper drum. All the tube ends and other parts exposed to much heat are submerged in water—an important condition for durability.

All the joints are iron to iron either screwed or riveted, no packing or bolts being used at any part.

The fuel magazine being flush with the front is much easier to reach for filling than if it was in the center of the boiler. The size of the magazine is not limited by the amount that can be cut out of the center of the boiler, but can be made of large size without interfering with any other part.

The placing of the magazine at one side, together with the good

combustion, obviates the liability of gas rising through the feeder, as it frequently does where the feeder is perpendicular over the fuel.

The position of the tubes and cleaning doors enables the attendant to clean the boiler in a few minutes without disturbing the fire.

The boiler being designed specially for steam heating with low-pressure gravity apparatus, it has not been deemed necessary to provide any means for cleaning the inside, except the blow-off cock which relieves it of all sediment that settles in the lower drum.

Though it is made for low-pressure heating it is very strong and would be safe with one hundred pounds' pressure.

To test its power of enduring ill treatment, one boiler was allowed to boil dry, leaving a strong fire in it which kept the tubes red hot for several hours. When the water was let in again it showed no leak or other signs of injury.

Where this boiler has been in use it has been the delight of the housekeeper, doing its work with the least possible care.

The inventor * seems to have made an important advance towards the perfection of steam-heating boilers.

In trial tests with other boilers, it performed the same duty as tubular boilers of approved construction and in good condition with twenty-five per cent. less fuel, and with some other boilers of good reputation it showed a saving of thirty-three per cent.

DISCUSSION.

Mr. Towne.—The subject of this paper happens to interest me, as I am using a steam apparatus in my dwelling-house, and am confronted with some difficulties in obtaining satisfactory results from the ordinary boiler. If this boiler proves on experience to keep tight it certainly has some advantages. Without discussing that, however, I wish to put on record here, very briefly, a minor but pertinent fact relating to the use of such apparatus in dwelling-houses, a fact which, if appreciated in advance, will lead to the employment of boilers of slightly larger capacity than would otherwise be provided. Any of us who have given consideration to the subject know that we all suffer in this country from the dry atmosphere of our houses in winter, and that the use of steam-heating apparatus, either direct or indirect, has rather increased

* Geo. H. Asire.

the difficulty by introducing very large volumes of moderately heated and very dry air. In my own house I have tested the humidity of the air by using a Mason's hydrometer, and have found that in ordinary winter weather the percentage of moisture in the house was under forty per cent., and from that down to thirty, while the proportion considered desirable from a health standpoint should be at least sixty per cent., and more than that would be better.

I attempted to remedy this defect by the ordinary appliances of evaporating pans and porous cups in front of the registers, and finally, as a mere experiment, by the hanging of wet cloths, saturated with water, over the registers. The effect of all that I could do in these ways was to increase the moisture in the atmosphere about *two or three per cent.*—nothing to speak of. I then resorted to the following expedient, which is, I think, worth noting: I made a connection with the steam pipe at several of the box coils under the floors, where the inflowing current of air is passed through the coils to be heated, and by means of a little regulating valve on the floor above I am enabled to permit a jet of steam to issue just under the radiator at a point where it mingles with the inflowing air. If desired, I can open that valve so wide as to get a visible flow of steam through the register. By means of this arrangement I can at any time increase the moisture of the air up to the point of saturation if I choose, and the amount of steam so introduced is simply limited by the point at which condensation on the windows occurs. You cannot get too much, as condensation will begin to take place when you get up to about sixty per cent. The difference in the atmosphere of a house resulting from a proper moistening of the air is greater than any one appreciates who has not tried the experiment, and all of us who have steam apparatus in use in dwellings would find very great benefit from making an attachment of this kind whereby they could keep the moisture in the house up to a reasonable degree.

The connection that all this has with the paper just read is simply that the use of steam in this way of course increases the evaporation necessary from the boiler, and for that reason it is desirable to have a boiler slightly larger than is ordinarily used.

Mr. Root.—What do you mean by sixty per cent.?

Mr. Towne.—Sixty per cent. of the dew point.

Mr. Kent.—Is there any noise?

Mr. Towne.—Almost none. There is a steam coil almost

directly under my library floor, and the noise of this steam jet in it is so slight that it is rarely noticed.

Mr. Babcock.—I wish to call attention to the statement: "The bend in the tubes permits the use of screw threads for uniting them to the drums, which has much greater strength and durability than the expansion of tubes in smooth holes." It is quite evident from that statement that the writer of the paper has had little experience in that line. Cutting a screw thread upon a pipe takes away a large share of its strength. It has therefore not "much greater strength" than a pipe that is not so cut away. That is self-evident. It is also evident to mechanical engineers that a pipe or any other piece of metal which is nicked as it is with a screw thread is very weak at that point, particularly against any bending stress.

All boilers made with screw threads and exposed to any bending strains have given way very rapidly at those threads. Corrosion is another serious difficulty with such boilers. It may be possible to cover such a thread by a projecting part of the piece in which it is screwed, and thus prevent any serious corrosion at those points; but this cannot be done when the tube is screwed into a plate-iron connection. Other criticisms might be made on this boiler. It would seem that the author of this paper has endeavored to show a good reason why this boiler is different from other boilers in common use, with a chamber for fuel feeding directly down to the grate. There are several somewhat similiar boilers in use of different makes, and they are doing very fair work. He also seems to have tried to find some good reason for making that bend in the pipe. He says it admits of screw threads being used. I cannot quite see how he can use screw threads and get the thing together. The difficulty of cleaning is also an important objection. I fail to see any special advantage in this boiler over those that are well known in the trade for house-heating purposes.

The President.—I suppose the intent of the magazine feature of the boiler is that a house servant will be able to manage it.

Mr. Babcock.—But boilers in common use have the magazine. There is one called the Dunning, and then there is the Gorton boiler which is quite similar.

Mr. Kent.—Fire-tube boilers or water-tube?

Mr. Babcock.—Some have fire tubes and some water tubes. Mr. Gorton made a water-tube boiler similar to this, only the tubes were not spread or bent at the bottom.

Mr. Barrus.—The statement is made that in comparing these with other boilers, they were found to perform the same duty in some cases with twenty-five per cent. less fuel, and in other cases with thirty-three per cent. less fuel. I see nothing in the construction of the boiler that should make it more economical than other good types, and should therefore very much doubt the reliability of that statement.

The President.—That is quite an indefinite statement, but if the statement that precedes it is true it certainly does not apply to any other boiler that I know of—"Where this boiler has been in use it has been the delight of the housekeeper, doing its work with the least possible care." That does not apply to a great many boilers in use in house-warming.

Mr. Kent.—There may not be any error at all in that statement, but it is a case of that implied generalization that I referred to yesterday.

Mr. Babcock.—The satisfaction might possibly result like a trial I made in my own house. I had been using a boiler which consumed about two tons of coal a month and I thought I would take it out as it was too small. A friend of mine had a new kind of boiler that he wanted me to try. I put it in and found that it used four tons of coal a month, which I said to him I did not think was any great improvement. "Well," he said, "the boiler is not big enough," so he put in a bigger one and that took six tons of coal! It is only fair to say that I fired the first boiler myself, while the others were cared for by my gardener.

Mr. Partridge.—In this matter of household heating a difference in cost of running between one system and another of between forty and fifty per cent. need not excite any surprise. It is not like running a steam-boiler to take power from. You take two houses of precisely the same dimensions, the same exposures, and the same number of rooms heated by the same heating apparatus, and differences as great as Mr. Babcock has indicated between his three different boilers, are easily to be found. I can call to mind a case at present of two similar houses where I think the cost of heating is three times as great in one as in the other. The factor of the servant-girl is a very much larger one than the factor of the fireman in the case of an ordinary boiler. Yet according to the old Aveling & Porter experiments, the firemen made a difference in their figures of fifty per cent. in the number of revolutions they got from the engine with a fixed load. The poorest of their fire-

men got 2,600 revolutions, the next best man got 3,600, and the man that they finally selected for their work got 5,200 revolutions; of course the coal consumed would have shown equally great differences if they had kept a record of that instead of making the coal fixed and the amount of work variable. In household economy the servant-girl is a factor which influences the economy of the boiler very much more than its construction.

In regard to the magazine, I would like to ask whether those who have used boilers with a straight magazine have found any type in which the fire does not run up into the coal occasionally and the whole body of coal become ignited? If this form would overcome that difficulty, it is worth more than all the other points claimed.

CLXXX.

THE OXIDATION OF METALS AND THE BOWER-BARFF PROCESS.

PRESENTED BY WM. H. WEIGHTMAN, NEW YORK.

In connection with the able paper by Geo. W. Maynard, on the Bower-Barff Rustless Iron Process,* the society will perhaps permit me to present the following report of Lieut. Col. A. R. Buffington, Ordnance Dept. National Armory, Springfield, Mass., as of considerable value not only from the information contained, but from the freedom accorded for use of the process defined and developed by Col. Buffington.

The report is made to the Chief of Ordnance, U. S. A., and is as follows:

NATIONAL ARMORY,
SPRINGFIELD, Mass., Oct. 7, 1884.

SIR:—Within a few days past I have been called upon by the Department for remarks relative to the "Bower-Barff process" of oxidizing the surface of iron and steel for preservation. To-day a representative of the parties controlling this process called upon me with specimens of their work, one a piece of gun barrel. In this way the subject has again been called to attention, and at the same time the discovery is made, by a comparison of work and a fuller knowledge of the Barff process, that a method now in use at this armory is superior to it for the oxidation of parts of small arms. Six or seven years ago the Barff process, so far as my knowledge extends, was made known in this country, and attracted my attention relative to the oxidation of parts of small arms, but my interest extended no further; the difficulties attending the application and the high degree of heat required, presenting obstacles I did not care to encounter experimentally. Some time after, either immediately before or after my change of station to the Watervliet Arsenal, I saw a public notice of a lecture in England in which it was stated the lecturer asserted the oxidation of iron and steel surfaces by the application of melted nitre to be the magnetic or peroxide of iron. This or a similar notice was seen by one of the assistant officers at Watervliet, and he called attention to it and suggested that it be tried, which I instructed him to do with the lariat rings there manufactured. The result proving satisfactory, the rings were thereafter, up to the time of my leaving the arsenal, so colored.

About this time, being on a board of officers in New York with the late Colonel Benton, then in command of the armory, I called his attention to the subject with a view of applying the process to parts of small arms. He soon after died, and no further action was taken until some time after I succeeded him here,

* Transactions A. S. M. E., Vol. IV., p. 351.

when my attention was again called to it by the master armorer observing the coloring of some metal, oxidized by nitre, which I had brought with me, and asking how it had been done.

I then began experimenting, but succeeded only at first, with the help of one of the workmen, in securing a good color for the tip of rifle stocks. This part, malleable iron, had always been case-hardened. The case-hardening, besides not coloring uniformly, injured the files in fitting the rammer to the groove in assembling the arms. From this success, without entering into details, we have progressed until now tips, bands, band-springs, guards (fully assembled), and butt-plates are colored with the nitre, and some experiments with barrels, after one (the first) rusting of the present browning process, would indicate that it is but a question of time until all metal parts of the rifle not requiring to be hardened are so colored. But after the success with the tip when the process consisted simply in immersion in melted nitre at about the melting temperature, after a blueing made in the ordinary way with heat (the malleable iron, it was found, required this preliminary blueing), a change was made. Whether the resulting satisfactory work is due to that change, or simply the result of more skill and judgment acquired by practice, as to degree of heat, time of immersion, etc., I am unable to say. The change consists in putting in the vessel with the melted nitre a few pounds of peroxide of manganese. It was known to me that the presence of this mineral increased, without any chemical change in itself, the oxidizing properties of chlorate of potassa. Why not as well the nitrate of potassa? I tried it with the result as above. I have made no secret of this whole matter, and it has already attracted the attention of one private arms company to the extent of coming here to see and learn the process. But it has occurred to me, the armory process being superior in color (to the specimen of gun barrel shown), economy, and simplicity of application, and the heat required not affecting the temper, which the Barff process does, of tempered parts subjected to it, the United States should be secured in the use of this valuable method without payment of royalties, by the publication of the history of its use here. The simple oxidation of iron and steel by nitre could not be patented, as books of chemistry have long ago published it, but the process as above described might be.

I therefore make this report with a view to its speedy publication. The method used here, in brief is as follows: A cast-iron pot or vessel, open to the air, built into a furnace of fire-brick and heated from below, contains the nitre and oxide of manganese at a temperature sufficiently high to cause ignition and combustion of a *pinch* of saw-dust thrown upon the surface of the nitre. The parts to be subjected to oxidation are suspended by wires from a stationary hook above the pot, in the center of the nitre; after immersing and moving them about through it so as to stir up the mixture, the parts are put in cold and *finished*, excepting color, and left suspended as above from five minutes up, depending on the size and color required. After the lapse of the required time, or after satisfactory color is obtained, which can be determined by raising the charge out of the nitre and looking at it, it is plunged into a vessel of hot water to cool and remove the adhering nitre; then into oil, completing the process.

[Signed] Respectfully your obedient servant,

A. R. BUFFINGTON,
Lieut. Col. of Ordnance.

Since preparing the above for presentation to the society, I have had the pleasure of a direct communication from Col. Buffington,

which like the official report to the Ordnance Department is substantially brief and to the point.

The communication is as follows:

April 18, 1885.

WM. H. WEIGHTMAN,
82 Astor House, New York, N. Y.

DEAR SIR:—In reply to your request of the 17th inst., I take pleasure in stating that the process of oxidizing by nitre has been improved to the extent of assured success, but there is little to add to the publication referred to. All metallic parts of the Springfield rifle, except the parts requiring to be case-hardened and the small parts colored in tempering, etc., are now oxidized by nitre, and by it simply as related in said publication without any preliminary or after process except in the case of the stock tip, malleable iron, which requires the previous blueing heating in the ordinary way on a plate. The rifle barrels require to be immersed in the nitre *vertically*, which is done on a suitable fixture holding eight or ten arranged cylindrically around a central rod having an eye for hooking to a small pulley block of a set above, arranged for lowering the barrels into and raising them out of a deep pot containing the hot nitre. After withdrawal, the barrels are left suspended in the air for the nitre to drip off and to cool to about the temperature of boiling water, into which they then are immersed vertically as in the nitre. An after immersion in a bath of oil (sperm) completes the process.

The barrels are treated in this way to prevent any *springing* and *set* from the perfectly straight and finished condition in which they are, before treatment. They are colored *inside* and out; the nitre having unrestricted access to the interior. The uniformity of product, the beautiful color, the economy of time, labor, and material, the simple and inexpensive plant, mark this process as an important advance in the manufacture of small arms.

A comparison by application to a smooth revolving wheel of the "browning" oxidation with the nitre as to durability gives a ratio of about 5 to 3 minutes in favor of the nitre; and a blow on a barrel colored by nitre does not remove the coloring, leaving a bright spot, as is the case with browning.

The degree of heat required varies, but not much, with the part to be colored. Whether the highest used is sufficient to heat to a redness visible in the dark, I am unable to say, as I have not tested it. The use of pyrometers has not proved satisfactory; such as I have used are unreliable and dependence is placed on the experience of the workmen.

The cast-iron vessels for heating the nitre, if perfectly sound castings, will last indefinitely. An imperfect one is liable to crack, break and precipitate the nitre into the fire. An accident of this kind will fill the room suddenly with a dense suffocating gas, but not otherwise dangerous—explosion does not appear possible in the absence of sulphur and charcoal.

The fire may be made either with hard or soft coal. I have tested the matter as to explosion, by pouring the nitre into a red-hot ladle and again emptied at once a large ladle full upon the hot coals of a soft coal fire with no other result than a sudden filling of the room with the gas aforesaid. Care should be taken to allow no oil, bits of wood, charcoal, in short any combustible matter or water to fall into the melted nitre or be introduced into it with the parts to be colored. *Minute* explosions on the surface and in the nitre take place occasionally and are attributed to oil or bits of combustible matter adhering to the metal parts im-

mersed in it. Care should be taken also when heating up a pot filled with melted nitre cooled into a solid mass. The bottom heating first, melts the nitre in contact producing the evolution of oxygen before the nitre above melts. This oxygen is soon under tension and will escape with more or less violence as soon as it can find passage through the melting nitre. The vessel should therefore have a weighted cover until the whole of the nitre melts.

No accident has happened here in the use of the nitre, beyond the cracking of an imperfect pot, and I look upon the process as a perfectly safe one.

The nitre should be pure—should be refined. The crude nitre of commerce will not give satisfactory color—it will not be uniform. Parts are found occasionally spotted and streaked. These are treated a second time with the desired result.

The streaking is attributed to some peculiar structure or inferiority of metal.

A surface perfectly smooth and free from the markings of the polishing material but not highly polished appears to be the most favorable for the action of the nitre.

A smooth polished surface of *cast* iron treated with the nitre is given a very pretty bronze color, which might be desirable for cast-iron products to which, in whole or a part, such surfaces are given.

Respectfully yours,
A. R. BUFFINGTON,
Lieut. Col. of Ordnance Commanding.

DISCUSSION.

Mr. Green.—Having lived for many years in the anthracite coal regions, and recognizing oxygen as nature's great destroyer, and having had all my life much trouble with the oxidation of metals —in fact, it is the greatest trouble that I have ever had—I have brought to present to the gentlemen of this society a couple of samples, in order that they might see how anxious we are to find some means to prevent it. If there are any gentlemen in this room who have lived for any length of time in the mining regions, and are acquainted with the mine pumps of the coal regions, it is not necessary for me to say one word. They know. But some of you do not know; and I will show you some few little points. I exhibit a section of what we call a 12-inch column-pipe, which was taken out at Mine No. 4 at Spring Mountain, and through which the water of that mine had been pumped for three years. This is a cast-iron pipe, made from a mixture of one-half, tolerably hard No. 2 foundry, and one-half scrap—a pretty hard iron. The joints were made of rings wrapped with cotton flannel and tar, and this one was put in without being protected. You will observe that that pipe, which was originally one inch thick, or a little over, has now become eaten by oxygen so that in certain parts of this pipe it is about an eighth of an inch thick and in another part about a

quarter of an inch thick. What happens in a mine when this state of affairs goes on? At the time when we want the pumps to work the best and the most water is to be handled, these pipes will burst, and before we can take out a pipe which weighs from 1,200 pounds to two tons and put another one in, the mine water has risen up dangerously near or over the pumps and we are in trouble. Furthermore, the working parts of the pump, being subjected to this oxidation, are often so corroded as to be rendered useless, and we can truly say that the oxidation of the parts of our pumps is the terror of our lives. The funny part of it is, the trying part of it is, that all that trouble is caused by the presence in the slates that accompany the coal in the mine (not in all coal veins, but particularly in the big vein, the Mammoth Vein E, out of which is mined about one-third of our entire anthracite product) of these beautiful crystals of iron pyrites, of which I exhibit a sample. The mere presence of that pretty little thing in the big vein costs two cents a ton at least for every ton of anthracite coal that is mined, so that when ten millions of tons of anthracite coal are mined from that big vein in each year as now, two cents a ton makes two hundred thousand dollars a year. Chemists tell us that iron (Fe) and sulphur (S_2) combine in different ways, and that it is only some of the forms which are liable to be broken up, releasing free sulphuric acid and leaving oxide of iron behind. It is where the separable forms are present that the acid water, flowing down the gutters of the mine and going into the pumps, makes the trouble. But there are further complications to be expected in a mining region where they pump mine water into streams. Where these streams used to afford beautiful trout the sulphuric acid will make them uninhabitable, and no fish will be caught there.

When the decomposition is complete, however, in surfaces which have been long exposed to atmospheric action, there is no more acid to be leached out, and the water flowing from the mine becomes fresh again. I know that in mines like the Big Mammoth Mine, in the upper levels it used to be so bad that it would eat up the pumps continually, but in time these upper levels became sweet again so that we could drink the water. I venture to say that I may catch trout in some of those now troutless streams again, after the mines are abandoned which discharge into them, as my grandfather did before the mines were opened.

Those of us who have been in the coal regions, and have had this thing to fight from our boyhood up, have found out ways and

means—not to fight nature's laws, for that we never will do—to flank this acid corrosion somehow or other. We will fix old nature so that the pipe we put in shall maintain its integrity and perform the function for which we put it there. We take a section of pipe nine feet long, from eight inches to twenty-four inches in diameter, and weighing from six hundred pounds to two or three tons. We clean it off and heat it a light red, and put it into a bath of asphaltum and coal tar, and give it a good dose of that. Now a great many would think that that would do; but that will not do, because exfoliation takes place under the coating. Now we will take that pipe and tar it, and then go to work and put in

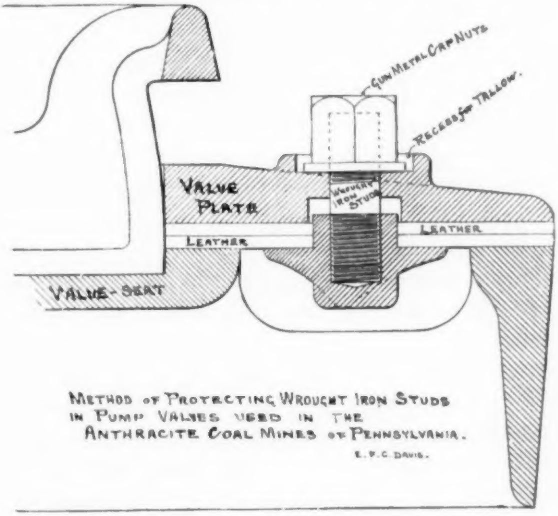


FIG. 226.

wooden staves all around it. Those staves are about an inch and three-quarters wide all round. They are driven in from both ends, wedged in place and sawed off square, so that then we have a pine lining in there, and an asphaltum back to that inside the iron, and that is about as far as we have gone yet.

Mr. Davis.—I would like to show you one or two specimens of the additional results of Mr. Green's little oxidizer. (Mr. Davis exhibited some specimens.) The way those wrought-iron studs are put in to protect them from the action of the water is shown in Fig. 226. The pump valve proper is there shown and the leather washer; the stud is tapped into in the bottom plate. There is a recess around the nut to be filled with tallow, and a

solid bronze cap nut comes on the stud. In a great many cases that answers the purpose. That eating away is not entirely a chemical action, but is to a very great extent caused by the mechanical action of the friction of the particles of sediment and dirt in the water. The chemical action of the acid softens the surface and the mechanical action of sediment cuts it away. We are making some experiments with a view to getting an enamel coating inside the pipes and castings that would make that action less. But I am sorry to say that our experiments have not been of long enough duration to prove anything definite. So far they have consisted of coating a core with a paste made of silica and carbonate of soda and carbonate of lime and a little boracic acid. We coat the core with that about as thick as we can get it on, say $\frac{1}{16}$ of an inch. In none of the cases in which we have put enameled castings in have they shown any signs of wear. I hope at some other meetings to tell the Society more about it.

Mr. Woodbury.—When nitre is heated the evolution of oxygen gas is very rapid, but if black oxide of manganese is mixed with it, the gas is produced at a slower rate, and perhaps the difficulties arising from the escaping oxygen would be abated if the black oxide of manganese was mingled with the nitre. It is quite possible that it might affect the iron ; but, at all events, it would stop the rapid ebullition.

Mr. Towne.—As a small contribution to what is said about the Bower-Barff process I may mention that I have watched some tests and found that while some articles treated by it stand wet weather very well, others, such as wrought-iron boat trimmings, subjected to the action of salt water and the weather, do not stand at all. The exposure of one season has in the latter case destroyed the protection, and the metal had oxidized much worse than if it had not been treated.

The President.—How long has the process been in use in a practical way in this country?

Mr. Towne.—I think the apparatus in Brooklyn has been there some three years.

The President.—The test you refer to is a test of one year.

(ADDED SINCE THE MEETING.)

Mr. Weightman.—An examination of the ring presented by Mr. Green on the occasion of the discussion of the paper on Oxidation of Metals, develops a quite interesting subject—viz., how it hap-

pens that, the pipe being in a vertical position when at work, the ring of oxidation is not concentric with that of the original pipe casting. At one portion the outside good metal remaining is only an eighth of an inch thick, and this gradually increases to one-half an inch at a directly opposite point in the circle.

Thus, as shown in the figure, at *C*, in Fig. 227, the oxidized

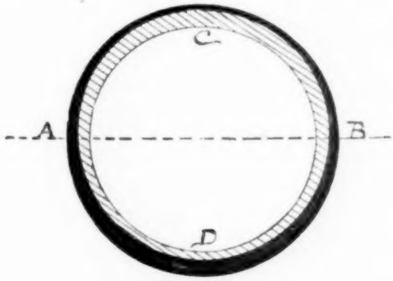


Fig. 227.

metal represented by the section lines is the thickest, and directly opposite at *D* the oxidized metal is thinnest. Another feature that may be noticed is that the parting line *A B* of flask or pattern is perpendicular to *C D*, or the center line of varying thickness of oxidized metal. From this defined state of affairs, and the fact that the pipe stood perpendicular while at work, we may conclude that there has been a gradual diminution of the density of the metal from the bottom, *D*, upward, or there has been an increased porousness of the metal from the bottom, *D*, upward. This varying porousness would give varying chances for oxidation of the metal increasing from *D* at bottom to *C* at top. We may also conclude that the part *C* was at the top when cast, and *D* at the bottom, giving increased density in proportion to depth of metal.

Had the metal been of the same density or character throughout, the pipe would have lasted one-third longer, or in proportion to the varying thickness of oxidation at opposite points in the circle. It might be advisable to try casting these pipes under considerable head or under pressure of any kind, or casting them perpendicular, after gun methods. It is very clear that difference in metal had something to do in this case with the speed of oxidation or hasty giving out of the perpendicular pipe.

It might also be well to try the effect of the nitre and manganese upon the insides of the pipes and under moderate penetrating pressure, the pipe being heated to required temperature of the mixture.

CLXXXI.

THE TORSION BALANCE.

BY WILLIAM KENT, M. E., NEW YORK.

"In the torsion balance proper the wire is stretched out horizontally, and supports a beam so fixed that the wire passes through its center of gravity. Hence the elasticity of the wire plays the same part as the weight of the beam does in the common balance. An instrument of this sort was invented by Ritchie, for the measurement of very small weights, and for this purpose it may offer certain advantages, but clearly if it were ever to be used for measuring larger weights the beam would have to be supported by knife edges and bearings, and in regard to such applications therefore (as in serious gravimetric work) it has no *raison d'être.*"—*Encyclopædia Britannica, 9th edition.*

THE above summary disposal of the torsion balance as having no *raison d'être* may be placed alongside of the predictions that a steam-vessel would never cross the Atlantic, and that the electric light would never be subdivided. The impossible has become not only possible, but an accomplished fact.

Several examples of the perfect torsion balance are exhibited herewith, ready for use, and your careful inspection and criticism is invited, so that you may form your own opinion as to whether or not they are the most perfect balances ever constructed, and whether it is not likely that they will eventually displace all balances made on the knife-edge system.

Prof. Frederick Roeder, one of the inventors of the new torsion balance, who died in August last, was in his younger days a student at Göttingen. There he witnessed experiments made by Professors Gauss and Weber with torsion wires as substitutes for knife edges in balances, but these experiments proved unsuccessful. Mr. Ritchie next made some elaborate experiments in the same direction with no greater success, as is mentioned in the article on Balances in the last edition of the *Encyclopædia Britannica.*

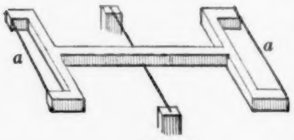


Fig. 157

Prof. Roeder repeated the experiments made by his former teachers, and discovered that one of the causes of their failure was owing to the fact that the pivot wires were directly attached to the bifurcated ends of the beam, as at *a a*, Fig. 157, and there-

fore could not be tensioned without bending these ends, unless the beam was so heavy that it would interfere with delicate weighings. In order to avoid this difficulty Prof. Roeder made light frames, which were stiffened by the wires being tensioned over them (Figs. 158 and 158*a*). Fig. 159 shows the simplest arrangement of scale with three frames and a single beam, and Fig. 160 a common counter scale on this system.

Scales were then constructed with a single horizontal frame,


Fig. 158
FRAME

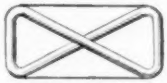


Fig. 158 a
FRAME WITH WIRE

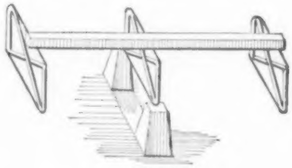


Fig. 159

one of the bands of which acted as fulcrum, the other as pivot. (Fig. 160*a*.)

Dr. Alfred Springer was associated with Prof. Roeder while the latter carried on his experiments in Cincinnati, and made many improvements in the system. Since Prof. Roeder's death Dr. Springer continued developing the invention, and has brought it to the state of perfection shown in the samples here exhibited. Their joint invention is fully protected by a number of patents

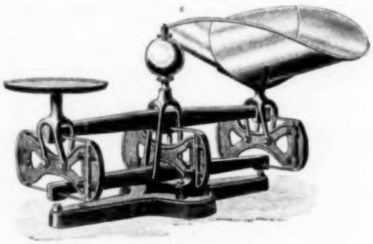


FIG. 160.

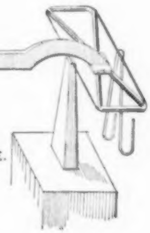


Fig. 160 a
LETTER SCALE.

in this country and in Europe, and several additional patents for modifications are still pending.

The simplest form of torsion balance is a very light beam supported at its middle point, which is also its center of gravity, by a stretched wire, the wire being firmly fastened to the beam. A weight placed at one end of the beam will exactly balance a weight at the other end. The sensitiveness of such a balance depends upon having the torsional resistance of the wire almost infinitely

small. This requires a very thin wire, and as thin wires, when stretched horizontally, are not strong, the balance can be used only for very small weights. Such a balance was Ritchie's, mentioned in the *Encyclopædia Britannica*, and it was a total failure for large weights. If the wire is made large enough to have an appreciable strength, its torsional resistance prevents the balance being sensitive. This torsional resistance also increases directly as the arc of twisting, up to the limit of elasticity of the wire.

To get rid of the effect of the torsional resistance in diminishing the sensitiveness of the balance was one of the chief ends of Messrs. Roeder and Springer's efforts. They accomplished it in a number of different ways, but the simplest, and the one which will be most generally advisable to adopt, is the placing of the center of gravity of the beam above its point of support. In knife-edge balances such a placing of the center of gravity would make the beam top-heavy, or in unstable equilibrium; the center of gravity would always tend to reach its lowest point, and tip the beam. In the torsion balance, however, this top-heaviness acts in the opposite direction to the torsional resistance of the wire, and may be made to entirely neutralize it. We thus have the torsional resistance exerted to keep the beam horizontal, and the high center of gravity tends to tip it out of the horizontal. The adjustment of the position of the center of gravity so as to neutralize the torsional resistance is most easily made by having a poise placed immediately above the center of the torsional wire, and making it adjustable vertically by means of a screw and nut. When the torsional resistance is entirely neutralized, the balance becomes infinitely sensitive, and any smaller degree of sensitiveness that may be desired may be obtained by simply lowering the poise.

The advantage of the torsion balances over balances made with knife edges is almost self evident. Whatever advantage any knife-edge balance can possibly have, the torsion balance has. The disadvantages of knife edges are all avoided, and these are so numerous that the simple statement that the torsion balance is a balance that dispenses with knife edges is its best recommendation.

A good knife edge is difficult to make, and still more difficult properly to adjust in position. It is imperfect at its best and limits the sensitiveness of the beam by its friction. This friction increases as the edge wears by use, or is corroded by the atmos-

phere, or bruised or crushed by overloading, or as it becomes clogged with dust. The torsion balance is easily made and adjusted, its action is frictionless, and ordinary atmospheric influences or dust have no effect on it.

The balances now on exhibition were made by Prof. Roeder and Dr. Springer in a small room in Cincinnati. A complete line has been built from an assay balance sensitive to $\frac{1}{200}$th of a milligramme to a platform scale of 1,000 pounds capacity, so as to test thoroughly the principle before beginning to manufacture for the market. Experiments are still in progress upon a variety of details, and a number of new designs are being made.

DISCUSSION.

Prof. Webb.—What should be a sufficient load on such a scale as that—the largest safe load?

Mr. Kent.—On this scale probably ten pounds would be a safe load, but I do not know what would break it, probably thirty or forty pounds. It is intended for a ten-pound strain.

Prof. Webb.—Thirty or forty pounds might stretch it so as to be useless?

Mr. Kent.—No; I think in the present state of the art, the first thing would be a giving out of the brazing of one of these wires, and they can be very easily replaced. There is a scale made with a double beam as large as that one in which a man can stand without breaking it.

Mr. Woodbury.—It has been stated that the tension upon the flat wires of the piece passed around was regulated by means of the musical pitch of the stretched wire, although one side is slightly shorter than the other; there is a difference in the pitch of nearly a musical third on the piece sent around.

Mr. Kent.—It is not necessary to have them exactly in accord. The testing of the tension by the pitch is only a convenient means of getting a tension far below the breaking point. The scale is not based on the principle of the torsional resistance of the wire at all. The wire only makes an axis of rotation. A piece of string or rope or anything that is firmly tensioned will do just as well, provided it supplies an axis of rotation.

Prof. Sweet.—If you had a string, Mr. Kent, it would not give you any element of resistance; it would not return.

Mr. Kent.—I do not care to go into that, because I have already

made a scale with a string which will return to the center, but it is patentable, and I do not want to publish it.

Mr. A. H. Emery.—This paper says: "Your careful inspection and criticism is invited so that you may form your own opinions as to whether or not they are the most perfect balances ever constructed, and whether it is not likely that they will eventually displace all balances made on the knife-edge system."

Mr. Zimmerman read a paper before the Engineers' Society of Western Pennsylvania, April 21, 1885, in which he speaks of the merits of this scale and its advantages over all others and its degree of perfection, etc.

Mr. Zimmerman states in his remarks, I believe, that there is one which is a gift from Dr. Springer, now being used in Cincinnati. He has one in New York on exhibition which was examined by the Yale and Towne Manufacturing Co., and by several prominent engineers in New York, and was found to be sensitive to the one-hundredth part of a milligramme.

I dislike exceedingly to appear in opposition to the claims of the paper just read, or to show the imperfections of work which is thus brought to us, and yet my duty to the Society and my relations to the scale business are such that it seems imperative that I should speak, lest by not speaking I may give occasion for an impression that I acquiesce in these statements which are put before you as facts. Some of these I am happy to say are facts, but others I am compelled to believe to be at least very considerable exaggerations. In regard to the scales which were examined by me, I would say that Mr. Towne and myself were shown several of those scales in New York, and we were mentioned in a paper published by the parties in interest as among the gentlemen who had examined those scales, and in such a way that the public are led to infer that we fully coincide and agree with those statements made, which is not the case. We did see the scales there, but we did not examine them, except as you have examined these by taking a look at them. But two of those scales were brought to Stamford. The first one was a scale similar in appearance and construction to this one—a five pound counter-scale, with a 16-inch beam. After that was tested, another was brought to us by Dr. Springer, called a prescription scale, which is a scale necessarily more delicate in some senses than the other, and is somewhat different in construction; but the same general principles occur there which occur in this one. The gentleman who

presented the paper in speaking of these scales, states that they were packed in a case, and that the case was dropped. The one which was brought to us corresponding to this is not in condition to withstand uninjured the shocks of freighting and ordinary use.

In regard to the qualities of this scale, there are several things which enter into the scale which ordinary members of the Society, and even engineers do not have occasion to find out. But those specialists who go into the use of scales technically and carefully have occasion to learn them. The difficulty is that the actual sensitiveness of a scale does not show its efficiency for use. For instance, on this scale, if we take off the counterweighting bob, it becomes, as has been stated, so stiff as hardly to be a scale; that is to say, the least load it will show is so large that we do not recognize it readily. Now the center of gravity of the beam is raised above the center of its motion, which cannot be done with a knife-edge scale because it becomes top-heavy and tips over. But it is done here with safety and with great advantage to the scale. It has been done by me in all of my scales, so that this is not new or peculiar to this scale at all; I do not know that it is so considered. But the center of gravity of the torsion balance may be changed rapidly by raising the bob, to increase the sensitiveness of the scale. Now the ordinary need of commerce is, that this scale should remain constantly sensitive. It does not require that it should be absolutely constant, but substantially so. The particular scale which we tested was sensitive to a movement of a quarter of an inch on the indicator by a load of six hundred milligrammes, or about nine grains. A third of that quantity would readily be detected, which would be three grains; that is with the scale unloaded. The scale was loaded with three pounds, and then we got a movement of a quarter of an inch with a less load, and with five pounds we got a movement of a quarter of an inch with still less, so that with five pounds on, the movement would be readily detected by a careful observer—the movement caused by a single grain. Now we might have moved this ball up higher, and shown greater sensitiveness; but such sensitiveness is of no use, because the scale has vagaries which cover the range of a grain by a great many-fold; and therefore the sensitiveness to the grain in that particular scale was of no sort of use. For instance, when the three pounds was placed on the platform in the center, and the load placed here to balance it, it varied from three pounds by more or less as it happened. The weight was

moved to the back of the platform, and the balance was disturbed by 2,900 milligrammes. The three pounds were then moved to the other side of the platform, and the balance was disturbed by changing that from minus to plus, or 2,900 more, making a disturbance of 5,800 milligrammes—a disturbance many times that of the sensitiveness of the scale sufficient to be seen, and a disturbance much greater than would be permissible for a scale that claimed any merits of accuracy. Five pounds was then put upon the scale, and it was found to be somewhat more sensitive, as I said before. The five pounds when moved from the center to the back of the scale caused a large disturbance. The disturbance caused by moving that from the back to the front of the scale was a little over 10,000 milligrammes; I do not remember just how much over, but it was sufficient to give us the one two-hundred and thirtieth part of five pounds. Now the scale showing a grain, as you can detect by carefully watching, with that load, would be showing a rate of sensitiveness of one thirty-five thousandth part its load. That, you see, is more than one hundred times, a great deal more than one hundred times, its rate of accuracy. So that the rate of sensitiveness is of no sort of use in a scale of that quality, I will not say of that kind, because I believe that kind can be made much more perfect. Dr. Springer afterward said, that the scale which he brought me had imperfections, and he sent it to me again to show those imperfections, and on that showing he said that he could pull so hard here as to slip the fulcrum plates in their seats; but I must say in regard to that, that while that imperfection did exist, and he could slip the fulcrum in its seat, this did not happen in the test I gave it, simply sliding the load from one side to the other of the platform, in which operation the movement of the fulcrum in its seat did not occur. But let us see what does happen which gives that great disturbance in moving the load from one side to the other. Now what happened was something which is due to the action of the band in every instance in which it could practically be used in this form of scale. But, before going into that, I would like to mention the test of the other scale which he brought us. That scale was for one gramme or one thousand milligrammes. He claimed it to be delicate to the thirty-second of a grain. Now this scale would show a movement of the pan—there were two little indicators coming out from the platform, so that we gauged a smaller movement. Now this five-pound scale has no indicator,

and a movement of four or five hundredths would be a movement as little as we could expect to see readily, but the indicators of a small scale double the real amount and make it very discernible, so that a movement of two hundredths here and two hundredths there makes a separation of four hundredths, which we readily gauge with the eye. Now that would be caused by about two milligrammes. The two milligrammes would correspond to the thirty-second of a grain, and that is what the ordinary observer would get readily if he was careful. But I must remark here that that is two full doses of some kinds of medicines. That is a medicine scale, and the least thing we could detect there readily is, therefore, about two full doses of some kinds of medicines; a scale, therefore, which I should assume was unfit for the use for which it was made. A load of one milligramme is one-thousandth part of its maximum load, a proportion which seems to me to be due to a class of scales which I call very coarse, rough scales. Now as to how far that could be borne out, I will mention here that I had a five-pound balance of Fairbanks—a little counter scale, with a beam about two-thirds as long as this, or perhaps not as much—I should say nine inches, and that scale shows readily a seven-thousandth of its load. To compare that scale with this, or rather, to compare this with the ordinary scale, after testing this, and while Dr. Springer was there, I worked a few minutes on that to see what that would do, and I found by putting five pounds on that as I had on this and shifting it from one fulcrum to the other, the greatest disturbance I could get was about $\frac{1}{1250}$; that is, that it was only one-fifth of the disturbance I got on his scale, which has a longer beam, and was supposed, therefore, to be a better scale. That is a scale, I will state, which I have used for about eight years with various loads from small ones up to five or six pounds, and it has had no repairs in all that time, and no adjustments, but, as I say, weighed in that condition after eight years' use more than five times as closely as did this one. Reference is made in the paper to this scale as being good in that it has no friction. In that respect I must fully agree; it has no friction. But I must disagree when it is called the most perfect scale and the best form for the use of the fulcrum. They have taken what seems to me to be an exceedingly bad form of fulcrum, and which will account for these vagaries which we have detected in this test. Those vagaries occur for this reason—the fulcrum which is stretched around this little casting is under tension for a length of about 5″ in this scale. It consists of

a little band of highly tempered steel which could be made straight while placed in clamps and ground on the edges so as to be a parallel piece of metal. But as soon as that piece of metal is relieved from its clamps, it ceases to be straight, and has to be made straight by repeated grindings, until finally it comes to be comparatively a straight band. As soon as it is strained up on its frame it will cease to be a straight band, and, therefore, when it is put under here, it is put under strains of unequal tension. Let us magnify a piece, conceiving it to be made up of a number of parallel bands side by side. Now each of these little strips which we divide this into has got a different rate of tension, and that rate will change every time we grind it and strain it up again. Well, now, what happens from that? That beam here has cut into it a seat to put that band in, and it was said to us in the course of conversation that Pratt & Whitney had stated that they could put those seats there to within one forty-thousandth of an inch, which for that scale would be considered entirely accurate, and the adjustment of the plate is of such width as to just set in there. But when you come to take these bands, and suppose that they have been set absolutely to within one forty-thousandth of an inch you suppose what is a mechanical absurdity. They cannot practically be set at these points to within one or two or three thousandths of an inch. But supposing you set them with perfect accuracy, then the unequal strain which the different sections of the band are under, gives the center of action of these bands at some other place than the measured center existing in that band. That is, the fulcrum center differs from the measured center, and we find that it does not correspond with the center of those bands, and there is no means of adjusting it. But let us change the load on the scale by adding a pound to each pan. These strains have now all been increased but not with uniformity, and there is introduced a new series of strains, and we will have a new fulcrum line different from the preceding one. When we set the load on one side here, we have one series of strains. When we set it on the other side, we have another series of strains; and so we have thrown that fulcrum from one place to the other, oscillating about, and it is a very variable fulcrum, the test showing that it has had large practical range of movement in the scale which was tried not a little distance of one forty-thousandth or a thirty-thousandth or a ten-thousandth, but a very large range of movement, as if the strain had gone far from one side of the band toward the other.

The same evil exists in the smaller scale, in kind; it is different in degree. Its bands there are smaller, narrower, more readily made of uniform tension; but it is the same kind of evil, and hence I shall expect to find the same kind of results. I have asked Dr. Springer to allow me to test his finer scale, although I do not think it has anything like the fineness claimed for it. In Mr. Zimmerman's paper it is said that the scales being shown to us have a sensitiveness of one one-hundredth of a milligramme; whereas the prescription scale had a sensitiveness of one milligramme, not the hundredth of a milligramme.

Mr. Kent.—Did you test the assay scale or the analytical balance?

Mr. Emery.—No; he promised to send that.

Mr. Kent.—I think he refers only to the assay scale and the analytical balance.

Mr. Emery.—He speaks as if he refers to the prescription scale.

Mr. Kent.—He made a mistake if he did.

Mr. Emery.—I suspect that the analytical scale to which Mr. Kent refers is a very much finer scale; but, as I said, I have not yet had the opportunity to test it, although Dr. Springer has promised to send it to us. But it will be subject to these two evils—evils which to my mind must destroy the torsion band scale as an instrument of precision, not as an instrument of use, because it may be useful and sufficiently accurate for many purposes; but I should not consider it sufficiently accurate for my purposes. In regard to the delicacy of this scale, the center of gravity is raised above this to overcome the resistance of these bands. Now if you take that band and put it in a straight line and fasten it to the two ends of the straining frame, it is clamped practically by its own tension, and we will find that bar undergoing strains of torsion due to the bending of the beam very readily. It has been said that it need not be elastic resistance. It must be. If it is not elastic resistance, the scale has friction.

Here is a thing that comes in, however. When the band is strained up with the load originally put upon it, that has one strain of tension. When the band is afterward loaded by these beams and its connecting parts, and these platforms, it has another strain upon it which is quite different. When we add to each pan a pound, it has another strain on it which is still different, and if we go on until we have about ten pounds on each side of the scale, in the proportion in which these bands were

brought to us, and in the position of sag which they had, they are strained up at that time with a load of about 90,000 per square inch of section, which is a pretty high load for a working load, when you consider that a man in putting on five pounds may be giving a shock which will bring a strain on this band more than is caused by laying on ten pounds. That load, as I say, is excessively high. Now, the load increasing, the strain of that band increases rapidly. Suppose this sag is increased as it is by the stretch of the band, as you load this more the resistance is increased in a greater degree. The band wants to draw itself straight over these lines, and as the load is increased it wants to insist upon it, and its resistance therefore is rapidly increasing. The balance weight here does not overcome that. This remains at a constant position whether you put on a small load or a large one; so that if these three fulcrums were put on one plane the scale would increase in stiffness rapidly as the loads were increased. That is, as the strain is increased on these bands rapidly, the sensitiveness of the scale would rapidly diminish. In this scale the load is doubled or even trebled—the initial load on the bands due to the load here, and therefore there is a large increase of strains and a large difference of sensitiveness. But if you have one pound here you have one degree of sensitiveness, when you have three you have a greater, and when you have five you have still greater. This control over the sensitiveness of scales would be a desirable thing, but I must add that placing the centers not on the same level with these plate fulcrums has been claimed after careful examination in the Patent Office by other parties, and claimed broadly, so that in this scale it cannot be used. It would be subject, therefore, in the ordinary commercial use under that patent to be restricted to putting those on a level, which will cause the scales to increase in rigidity or stiffness as the load is increased. Now, as regards the real fineness of the scales, I may be allowed to state that when Dr. Springer was at our place, I took him out to the shop and showed him a platform, a square one, about sixteen inches, in a little testing machine we have, where we carry a load of sixty thousand pounds, and on which a tenth of a pound gave the same movement of the indicator as did a milligramme or a thousandth part of the maximum load on his prescription scale; that is, his prescription scale showed for a thousandth part of its load a movement of indicator which was equal to the movement of our indicator in this testing machine where

the load was $\frac{1}{15}$ of a pound, which was $\frac{1}{600000}$ of the load the scale was made to carry. They can hardly be considered, therefore, as of the same degree of sensitiveness. Now, there is in this machine another platform of the same size, and another twice as large. They are put in different positions; but the one that is twice as large is used where the load which is to be weighed, when we want to know pretty closely, goes up to one ton. The ratio to the other parts is seventy to one, so that the sensitiveness is seventy times as great—seventy to one. And I may state here, that while we weighed five pounds on the scale of Dr. Springer, once in front and once at the rear of the platform, that is the load that the scale is intended to carry—five pounds—the difference in the two weighings was $\frac{1}{130}$ part of the load. We had occasion to weigh three, ton-standards last week. We weighed each of them four times and in an open room where the wind was acting on our indicators and on these large platforms, and the drafts caused a very considerable disturbance, a number of grains, perhaps thirty or forty or fifty grains at times, but in weighing the ton of two thousand pounds we were weighing 14,000,000 grains—the ton was weighed in each of those cases four times with a maximum difference between the lightest and heaviest weighing of about one hundred grains. Now, here is a maximum difference in the five pounds here twice weighed of about 150 grains. We had a maximum difference of the ton on that scale weighed four times of a hundred grains. So that that scale weighed the ton weight more accurately—not relatively, but absolutely—with greater accuracy than did this five-pound scale weigh its five pounds. That would be four hundred times the rate of fineness of this one. Now, to come down to really fine scales, we will not consider what this analytical scale may be, which, I have no doubt, is finer than these scales of his which we tested, but I have no reason to suppose that it is as fine as they claim it to be by a great deal from what I know of scales, and if I judge it by comparing their claims for those tested with the actual results we obtained in their tests, I should not expect to find a very good or fine scale. The scale which I will call attention to is one of my own which I arranged for weighing some standard weights a few years ago where the weights did not run to a ton as they did in this case last week, but to 200 pounds. I omitted to say that I showed Dr. Springer where we had one platform about twice the size of that, and another one about five times the rate of sensitiveness as the one where we

weighed a ton, on which last-mentioned platform we weighed 500 lbs. That scale shows distinctly a grain with 500 pounds on, and two grains there causing a movement equal to that caused by a milligramme on his little prescription scale, so that you can stand five feet away and see two grains distinctly shown on the scale which weighs 500 pounds: 500 pounds is 3,500,000 grains, and 2 grains would be $\frac{1}{1750000}$ of the load. But in the scale which I arranged for weighing my standards, which were to be standards not for use in the shop, but standards for making standard weights, I rigged the balance so that with 200 pounds on the fulcrum the scale would show distinctly $\frac{1}{10}$ of a grain or $\frac{1}{14000000}$ of the load. Two hundred pounds was weighed nine times, and the maximum difference between weighing it nine times—that is, the difference of weights, between the heaviest weight so recorded and the lightest weight, was one part in 2,350,000. Now, if we take the fineness of the scale, we find it to be ten thousand times the fineness of the five pounds' counter scale of Dr. Springer. I think I hardly need say more in regard to the fineness and the merits of these scales, with the exception of one point upon which I would like to touch. It is stated that this scale is indestructible practically by acids, or, in precise words, that it was uninjured by overloading, and that it was uninjured by corrosion until the corrosion got clear through the plate. Now, referring to this diagram, you remember that these plates are under unequal tension. If by some means or other they had got them under equal tension and the acid got to work and cut unequally, then we should have unequal tension from that source, and then this would become a scale of error instead of an instrument of precision.

Mr. Hawkins.—It seems to me that the mere placing of the counterweight so as to have it in position to bring the center of gravity at all times high enough to overcome or equilibrate the torsional resistance of the wire or band is defective, and for the following reasons. The paper states that the torsional resistance increases directly as the arc of twisting; and the supposed compensating action of the weight is evidently based upon that assumption.

Now, I think that we all know pretty well that a wire or band of that character offers torsional resistance in proportion to the arc of twisting only when it is not under tension; but, if it is placed under tension, then its resistance to torsion must increase in a much greater ratio; and, as in the specimens shown, their

torsional members are in the form of flat bands, I think it applies still more strongly to them than to the round wire, because the band may be considered as equivalent to being made up of a series of such wires side by side, all of which, except the central one, receive, besides the actual twisting about their axes, a transverse bending at the points of application of the beam to them and of their application to the truss or frame; and the greater this bending becomes, for the outer parts of the bands, the wider the bands are: so that the act of torsion of such a band is, for several reasons, not to be compared to a round wire twisted when either under or not under tension. But no one will contend for a moment that even a round wire will resist torsion proportional to the arc of twisting, if it be either under tension or so confined at the ends that it cannot shorten as the twisting is performed, as, under the latter circumstance, the act of twisting must either shorten it or bring it under tension.

In every example shown or described, the wires or bands are not only so held by the ends as to prevent shortening by the act of twisting, but must necessarily be under more or less original tension.

If it be understood, then, that these wires or bands must offer resistance to torsion in a greater ratio than proportional to the arc of twisting, because they are either brought under tension by the act of twisting, or because any original tension they may have must in the same way be increased, it follows that an increase of tension in them from any other cause must still further cause them to depart from the conditions assumed in the paper. Thus the placing of any load upon such a scale must increase the tension, and, therefore, the resistance of twisting; in which case, if the scale could be so arranged or adjusted as to position of the counterweight as to be in stable equilibrium, with the beam horizontal or at any given arc of twisting, the imposition of the load must destroy this adjustment by increasing the tension on the wires or bands, and require the readjustment of the weight for every change in the load.

But the fact that such a band or wire cannot, under any circumstances shown or explained, resist twisting exactly proportional to the arc of twisting is not the only thing which must interfere with the correct action of this counterweight in compensating the torsional resistance: the weight placed above the beam, itself describes an arc equal to that of the arc of twisting of the wires or

bands; and the gravitating action of the weight is not proportional to the arc described by it, but proportional to the cosine of that arc. While this variation is very small, it is in the wrong direction, it being a diminishing quantity for equal arcs, while, as shown above, the resistance to twisting of the wires or bands is not only an increasing quantity without load on the scale, but increasing additionally in some function of the load applied.

Under these circumstances, it would seem to be impossible so to place the weight, in the first place, as exactly to equilibrate the torsional resistance of the wires or bands at any point, except that of horizontality of the beam or where there is actually no torsion, if the torsional resistance of the wires or bands is at any given arc to preponderate sufficiently to restore the beam to a horizontal position. Immediately that the beam departs from the horizontal, the increasing resistance to torsion will be greater than the diminishing effect of gravity upon the weight; and the preponderating load in the scale pan will be incorrectly indicated at any arc whatever just to the extent of the disparity at the given arc between the gravitating effect of the weight and the resistance to twisting offered by the wires or bands. If any arc should be chosen at which the gravitating action of the weight shall exactly equilibrate the torsional resistance of the bands or wires, then the beam would not be restored to horizontality; because, for any lesser arc, the gravitating action of the weight must preponderate, and the beam could not be in horizontal stable equilibrium.

Mr. A. H. Emery.—In regard to the fulcrum changing and growing stiffer owing to the tension, I remarked that as this band was drawn down under heavier loads that it did grow stiffer. If that band were straight, and we consider its neutral axis while this band was going down, one side would be going down but the other side would not go down so much, and if it was level the change there would be like to the change here. That would compel it to take on a tension due to that torsion. Now, if the fulcrums spring down, and we consider the front side of this band as toward us, and we stand at the center of the scale and the pan is moving down relatively, the nearer side of the band will be going up and will be decreasing its tension, while the farther side will be going down more rapidly and increasing its tension, giving a different action and a different fulcrum length from what we had in its higher position under the strain produced by the smaller load.

Mr. C. E. Emery.—I would like, as a matter of interest, to ask of the last speaker which form of apparatus was used for the sensitive scales. Were they on springs?

Mr. A. H. Emery.—The scale that you refer to was the one we used for testing our hydraulic supports. It is not an hydraulic scale; the first fulcrum is a plate, $\frac{1}{10}$ of an inch thick and $\frac{4}{10}$ wide, inserted $\frac{15}{100}$ at each side, leaving $\frac{1}{10}$ of an inch free exposure. The fulcrum on the platform here first mentioned carries loads of 60,000 pounds. When you load the hydraulic support, then the fulcrums in the system are under compression. The fulcrum that supports the beam is under compression likewise; and the one which suspends the platform on which the ton was weighed is also under compression. This latter has a fulcrum about $\frac{3}{100}$ of an inch in thickness, and consists of two parts separated about 15 or 18 inches, a couple of rods running down to support the platform. All the fulcrums are under compression until you get to the fulcrum which suspends the balancing weights. Those fulcrums which receive the loads directly and also on the second beam are all under compression. There are no tension fulcrums on that scale until you get the one that suspends the small weights which balance those loads put upon the main platform.

Mr. C. E. Emery.—That hydraulic support you speak of is not a part of the scale?

Mr. A. H. Emery.—That is a part of another scale we were testing incidentally. This would have received its load from the action of the hydraulic press forcing the load down on to e hydraulic support merely to give it load, and that load being weighed by what I term this plate fulcrum scale. If you will look at these fulcrums you will see they consist of a piece of steel set directly in this massive beam, something which is exceedingly simple compared with a bridge truss, sufficient in length to get delicacy of action and sufficient in strength to support on that long bridge 60,000 pounds. It would become a very massive structure in a torsion balance, and when it was said that it was contemplated to use this plan in testing machines, I considered that the speaker could not have sufficiently contemplated the problem.

Mr. Kent.—I will first reply to Mr. Hawkins about the point as to the increase of torsional resistance with the arc. I am not certain about the increase of torsional resistance with the increase of tension of these bands. Mr. Emery holds, I believe, that it is true;

but I have not investigated that problem so as to answer definitely at present. But as to the other statement made by Mr. Hawkins that the torsional resistance increases uniformly as the arc, while the effect of the weight increases at a decreasing rate with the sine of the arc, is entirely correct. It is fortunate for the torsion balance that it is correct. In all these scales the arcs of rotation are only two or three degrees at the farthest, so that the total amount of variation from the perpendicular would only be a very small quantity, and for such small arcs the sine of the arc is practically the same as the arc itself. It is the same as the arc itself at the beginning. We have the weight here tending to fall—its tendency to fall increasing as the sine, but the sine does not increase as rapidly as the arc increases. If it did increase as rapidly, or a little more, then the weight of the poise might soon preponderate over the resistance, and the scale would become top-heavy. As it now is, the weight and the resistance being nearly balanced at the beginning, the torsional resistance of the wire becomes the greater as the arc increases, and tends to bring the beam back to its middle position, which is a very fortunate thing for the balance.

In regard to the remarks of Mr. Emery, I feel flattered that this criticism has been drawn out from one who has made a specialty of the subject of scales and testing machines for a great many years, and who perhaps stands at the head of his profession in the world to-day, in the making of fine weighing apparatus. We all know the history of the Watertown testing machine, and that some of the best mechanical engineers of England said that if they had seen it on paper only they would have said that it was impossible to build it; that there did not exist in the world the mechanical refinement necessary to build it. The Watertown machine exists because it is the perfection of mechanical workmanship, and probably would not exist if it had not had such workmanship brought to it. The difficulty there was in building that Watertown testing machine we all know, and that it cost its inventor more than \$100,000—for which, I am sorry to say, he has never been repaid—and that when after years of labor it was completed at last, it was a machine with about a million pounds capacity, and by far the most perfect machine of its kind in the world.

The original inventor of this scale, Professor Roeder—he is dead, I am sorry to say—was a chemist, and not a mechanical engineer. Dr. Springer is also a chemist. They hired a model-

maker mostly accustomed to do fine brass work. Professor Roeder, Dr. Springer and that model-maker were the only three men who ever had anything to do with building scales. Is it any wonder, then, that they are very imperfect as samples of workmanship and disappoint an expert in such matters?

Yesterday I made a test of that candy scale in the same manner as Mr. Emery did, and I got somewhat different figures. He got an error due to the placing of that weight on one side of $\frac{1}{230}$ of the load. The candy scale is a much better scale than the one he tested. The error in placing four pounds on that scale as far eccentrically as I could without its falling over was $\frac{1}{640}$th. Placing it eccentrically in the opposite position, the error was $\frac{1}{5120}$th. The error due to elevating one end of it $\frac{3}{16}$ of an inch was one part in 2,560. Each of these errors was an error that could only be made by a man weighing with gross carelessness. No man places a weight on a scale so far eccentric that it will almost tumble. No man raises a scale $\frac{3}{16}$ of an inch out of level. The scales, when built as they should be built, would be adjusted first on a level table. Then when you put the scale on a table that is not level, all that you have to do is to adjust it by screws, and by manipulating these screws you have a better level than almost any other level in existence. The scale itself is a leveling machine. Criticisms have also been made of the method of construction of this machine and the necessity that these errors should always happen in this machine. These may be very just criticisms of the particular type of scale tested in Stamford, this ten-pound grocery scale, a very imperfect piece of workmanship, but in numerous other scales all the errors thus criticised are absolutely avoided.

The statement is made in the paper that "several examples of the torsion balance are exhibited herewith, ready for use, and your careful inspection and criticism is invited, so that you may form your own opinion as to whether or not they are the most perfect balances ever constructed, and whether it is not likely that they will eventually displace all balances made on the knife-edge system." After all that has been said, I see no reason to withdraw that statement if you will put the limitation in it that each is the most perfect scale, *for its purpose*, ever constructed. For instance, this tea scale is the most perfect tea scale ever constructed. Do not compare this scale, made for weighing tea, with the Watertown testing machine.

Mr. Emery did not test any of the torsion scales in which the pans are suspended from the beam. In these the effect of the eccentricity of the load is entirely eliminated. It is upon this principle that the assay balance is made, which is said by Dr. Springer to be sensitive to the $\frac{1}{200}$ of a milligramme. Chemist's scales are also built on this plan. Dr. Springer informed me that one of the analytical scales built by him three years ago, and at work ever since in one of the universities of Ohio, and used by the students, has remained more sensitive and more accurate than a scale which he imported from Germany at a cost of $200.

Great stress has been laid upon the errors of this band due to the internal strains in it, and due to the crookedness which will follow an attempt to grind these bands parallel. They are not made by grinding, and I do not believe ever will be. They are made from a round wire which is drawn repeatedly from a steel ingot. I would like any one to prove that a round wire drawn repeatedly from a five or six-inch ingot or a twenty-inch ingot, if you please, is going to have its center of strain any reasonable distance from its center of figure. Prove to me, if you will, that it is one-millionth of an inch out of the center. I think it would be harder to prove that than to prove the negative.

I think the best analytical balance to-day in the world is the torsion balance, which has been exhibited by Dr. Springer. If there is one balance finer, it is his assay balance, a small one, which, he says, is sensitive to $\frac{1}{200}$ of a milligramme.

Now, having referred to what I believe to be the best scales for their uses in the world, I will show you the worst torsion balance in the world. I made it myself last night, and I will end this reply with this letter balance which I submit to your inspection. (Mr. Kent here exhibited a letter scale, putting the pieces together as he spoke, and showed that it was sensitive to $\frac{1}{640}$ of an ounce, while its capacity was one pound.)

There is one criticism Mr. Emery made about the strain in the wire when the balance is loaded to full capacity being 90,000 pounds per square inch. He told me how he figured it, and I found two mistakes in his calculation, which divides the 90,000 by 4.

CLXXXII.

EARLY EXPERIMENTS INVOLVING THE FLOW OF METALS.

BY W. E. WARD, PORT CHESTER, N. Y.

FORTY years ago, the physical properties and characteristics of metals were less understood, in some respects, than they are now; and it followed that the less elaborate technical knowledge of that period had to be supplemented as far as possible with superior practical skill. It sometimes happened, because of inexperience, that the qualities of metals which are rightly regarded as the most useful and indispensable to the great majority of industries, seemed to be, in exceptional instances, almost fatal to the complete success of others. Experts in wire-drawing were not blind to the fact that the possible extreme reduction in section of rods while passing through the die plate, was due to their malleable and ductile qualities. When the same kind of metal was subjected to intense compression, it was observed that its cohesive integrity was equal to sustaining, without rupture, a limited lateral displacement quite as uniformly as it did the tensile strain required for drawing into wire, as had been familiarly illustrated in upsetting the heads on bolts and rivets, which is a test of the severest kind on the fibrous structure of metals. The significance of these now well understood characteristics of some metals, did not appear to induce any special inquiry, or awaken an interest equal to their importance, until M. Tresca, of Paris, became engaged in conducting a series of careful and exhaustive experiments, with the view of determining, if possible, the law now known as the "Flow of Solids." A report of his labors was presented in a lengthy paper to the Institution of Mechanical Engineers at their meeting in Paris in 1867. This interesting report embraced the results reached by him in experimenting with lead disks under compression in a variety of ways, as well as with ceramic materials. He furthermore subjected heated wrought-iron disks to similar tests and observed, that in all the materials employed in his experiments the tendency was invariably to flow "in the direction of least resistance." The conclusions of Tresca, reached by his own methods of original research and patient

labor, gave some little importance to a feature developed in a series of experiments made by the writer as early as 1845—the first stages of which involved such a sharp contest with the flow of metals as completely to defeat the success of a novel machine, which was designed and constructed for the purpose of heading long countersunk screw blanks and rivets in solid dies. The device employed for holding the dies (four in number) was an indexed roulette, which was actuated in the usual manner. The half revolution rest-periods peculiar to the common roulette movement, among other promising advantages, permitted, while at rest, the feeding of the wire for the blanks at one point, the heading of a blank at another, and the discharging of a headed blank at a third point simultaneously; so that nearly all the movements in the machine favored the use of eccentric motions, to the exclusion of cams, which simplified construction and resulted in harmony of movement. But when making a practical test of the machine it was soon discovered that the headed blanks were so firmly upset and fixed in the dies that a resistance almost insurmountable was presented to the mandrel provided for discharging them from the dies. A further and even more serious difficulty appeared in the bursting of the dies during the heading process; although they were $1\frac{3}{4}$ inch in diameter by $1\frac{1}{4}$ inch long, and the wire used no larger than No. 3 wire gauge—yet with the most careful tempering, they failed to withstand the lateral strain involved in heading countersunk blanks. The cause of the trouble was not clearly revealed, until a number of blanks were made designedly in a die which had been burst in this manner, all of which showed a thin fin or spine of metal which had been forced out from the sides of the blanks during the heading operation, into and along the whole length of the fracture in the die. The question then confronted was, whether such a fracture could occur under the amount of compression required for heading a screw blank, and a lateral flowage of metal follow into the fracture, *unless* the metal operated on was subject to a law that controls alike substances of every material consistency, whether solids or fluids. From the evidence furnished by the results of the experiments, there seemed to be but one reasonable conclusion, which was, that, as a rule, the movement of solids analagous to liquids under compression, will at some definite extreme, react on a lateral barrier to their movement with the same persistence (less the difference in friction) as they exert in the longitudinal directions. The dies containing the

blanks during the heading operation rested solidly against an immovable, hardened steel block, which sustained the longitudinal thrust resulting from the heading movement. As there was no relief to the die under pressure after the limit of space for the blank in the die, had been filled up, the lateral strain within the die before the completion of the heading, was so much in excess of its resisting capacity, that the bursting of the die under the circumstances was just as inevitable as the bursting of a hydraulic cylinder when loaded beyond its ultimate limit. The practical remedy was within easy reach when the *cause* of the difficulty had been made clear; and when the question had been reduced to a matter of mechanical device, it was not long before a simple and effectual way opened which led to final success. The conditions simply required that a relief movement be provided in order to save the die from excessive lateral stress during the severest part of the heading operation. To accomplish this it was necessary to introduce a new order of mechanical devices, such as would invariably give the required relief to the dies before the breaking strain was reached. The main feature required in the mechanical improvement was the employment of a mandrel actuated by an adjustable cam in combination with a single stationary solid die, mounted in a suitable frame so as to perform the triple duty of transferring the blank to the die and supporting it while it was being partially headed, and finally discharging it when the heading was completed. This arrangement required that the centers of the hole in the die, the mandrel, and the heading punch should all be located upon a common axis. When the wire for a blank had been fed in the machine and gauged to the right length, it was then cut off and transferred by a simple device to the rear end of the die, and in line with the axis of the hole where it was met by the mandrel, ready to move forward in the same axis. This movement of the mandrel carried the wire blank into and far enough through the die, to supply the exact quantity of material required for forming the head on the blank at the opposite end of the die. At this stage of the operation the most important function of the mandrel was called into play. During the period of rest, after transferring the wire blank to be headed into the die, the end of the mandrel then constituted the temporary seat for the shank end of the blank to rest against, and it was timed at this point to remain stationary long enough to sustain the heading pressure required from the opposite side, to form a bulb of metal of sufficient size to make a full completed head on

the blank. This accomplished, the mandrel retired out of the way, while the heading movement continued until the head was finished at the end of the stroke; then, as the heading punch receded from the face of the die, the mandrel again returned and performed its last function of discharging the blank from the die. It is clear, that by this method no serious lateral pressure could be exerted during the formation of the bulb; and it is evident that any surplus metal in the bulb, not required for making the head, flowed harmlessly back into and along the line of the shank, thus saving the die from all danger of bursting, and from the difficulty encountered in the first experiment in discharging the blanks after they were headed. It furthermore made the way easy and practicable to head screw blanks and rivets in solid dies at least three inches longer than had been possible heretofore by that method. By far the most interesting feature developed in the experience, was the discovery of the cause which had defeated the first experiment, and which so readily suggested the means by which all that remained questionable in the problem could be disposed of. It furthermore indicated, that as a fundamental principle, there is a constant tendency in substances when yielding under compression, to flow "in the direction of least resistance." The illustration of this tendency as witnessed in the partial upsetting and final distribution of metal under compression in solid dies, afforded good ground for concluding that the same tendency was discernible in the results of other mechanical agencies employed in changing the forms of malleable metals, whether through a system of cold or hot rolling, forging, or through any other appliance by which they are forced from one form into another.

DISCUSSION.

The President.—This paper, presented by Mr. Ward, illustrates in an exceedingly practical way the fact that solids, as well as fluids, will seek relief from pressure in the direction of the least resistance. The paper further illustrates the fact, that there is a degree of compression that may be brought to bear upon metals, which will force the metal into the pores of the wall of the matrix, or die, in a manner in effect amounting to a weld. There is another, and a very important feature connected with the flow of metals under pressure, especially where, as in the case cited by Mr. Ward, the flow is in the direction of enlarging the metal beyond its original size, as in upsetting it; and that is, the marked change

that takes place in the structure of the metal when compressed, as compared with the same metal under tension. This is, however, a very wide field, and possibly Mr. Ward intentionally avoided letting down the bars, and thus inviting discussion in that direction. I think there are members here who have had a good deal of experience in dealing with the various metals, and that we may expect an interesting discussion on this paper. But, while I think there are many here who can discuss it intelligently, there are very few who can antedate his experiments. I should be very glad to hear from any member present of his experience in that direction.

Mr. Durfee.—I do not know that I can say anything that will interest the members in regard to the particular kind of flow that is described in the paper; but there is a problem involving the flow of metals which I think has never been properly investigated. I certainly myself have not had the time or the opportunity to devote to it, and I will illustrate by means of a rough sketch the problem that I have in mind. If we have two rolls, R, R, as in a rolling-mill, and a mass of metal (A, Fig. 213) passing between them—its movement being in the direction of the arrow—the metal will pass out from between the rolls reduced to a certain smaller cross section, but increased in length. I do not know myself of any authority that states with precision how much faster for a given rate of reduction the issuing metal moves than the periphery of the rolls, whether it moves any faster; or at just what point of the arc of contact B C between metal and rolls the flow of the metal is equal to the movement of the periphery of the rolls. It is a very interesting question that has to do with the reduction of metal by rolls for plates, rods, beams, rails, and, in fact, all shaping of metal produced by rolling.

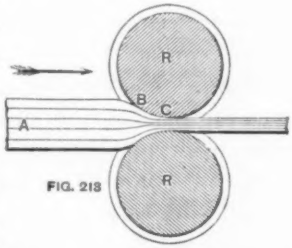


FIG. 213

Mr. A. H. Emery.—I would like to show one point in regard to the flow of metals which was presented in the Watertown arsenal experiments, in the bursting of cylinders called gun blocks. Those cylinders were 11 inches in diameter and 22 inches long, with a bore 3.3″ in diameter. They were all filled with beeswax 10″ deep. The steel plunger which pressed upon the wax in this case had to be hardened, inasmuch as it flowed out laterally and filled the cylinder tightly, so that it was very difficult to remove

it, in the first experiments before it was hardened. At the bottom of the plunger it was turned down to a small diameter; I should say about 2 inches, and around that was placed a little copper casting, dressed cup-shaped to act like a packing, and fitted on the end of the steel plunger. Its shape is shown in Fig. 228. The press-

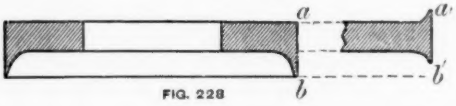


FIG. 228

ures are carried on these cylinders up to 90,000 pounds per square inch. (In the bottom of the bore under the wax were placed cast bronze blocks about 1.″5 long, of a diameter 3.3″ to fill the bore.) The bronze blocks at the bottom of the wax were increased in density by the compression by 6 or 7 or 8 per cent., owing to the cavities, I suppose, which were filled up under this pressure. This copper packing, however, took this action—as the wall of the cylinder separates from the plunger under the elastic action of the cylinder, the pressure being high, there is a little opening made at the corner of the plunger, between that and the wall of the cylinder. The result was that the upper corner of the packing ring at *a* flowed up into this opening, and when the test was done the copper cup was about the shape shown at the right of the cut. This lip at *b′* is very much decreased. The lip is now mainly on the upper side, as at *a′*, which at the end is not more than two or three-thousandths of an inch in thickness, while at its bottom part it might be six or eight-hundredths thick, and ten or twelve hundredths of an inch long. The copper really must have flowed in there at the outset with an opening of a very few thousandths, as at the upper edge of this lip at *a* the space is only two or three-thousandths thick as it stands, showing that the copper would probably flow through an opening of a thousandth, or half a thousandth, or a quarter of a thousandth of an inch, provided it had sufficient pressure. I made some experiments at Chicopee in pressing lead through holes or grooves. I have had an opening of that kind of one two-thousandths of an inch wide, and forced the lead right up into it three or four-hundredths of an inch, thus forming a ring this thickness and width.

Mr. Durfee.—There is one illustration of the flow of metals that has occurred to my mind since I was on my feet before. It takes place in the manufacture of metallic cartridges. You all

know, those of you who have examined a metallic cartridge, that it has the general shape of a champagne bottle (Fig. 214). The form of the cartridge before it assumes this final shape is cylindrical, and by suitable mechanism the part A B undergoes an "upsetting" process, the metal being thickened and the diameter of the cartridge reduced, and this is accomplished so perfectly that the cartridge is perfectly smooth throughout its whole surface. The metal, of course, along the upper part of the cartridge must be very severely upset. It is very thin; I cannot pretend to say how thick it is;* but it is not much thicker than an ordinary piece of brown paper, such as is used for rough drawings, and the difficulty of smoothly "upsetting" this thin metal was, to the officers of the United States Patent Office, apparently so insurmountable that when application was made for a patent, the examiner asserted that the thing could not be patented, because it *could not be done*, and the applicant had to bring a great many samples of the article itself and affidavits from a number of persons to show that it had being done, and was being done on a regular manufacturing scale.

FIG. 214

Mr. Dodge.—I would like to speak of some experience in this connection, similar to what Mr. Emery has just shown us. I had a testing machine which was never strained higher than 10,000 pounds, and in it, a number of large brass washers having a width of about a half an inch all around, took the thrust of the pressure bearing against a cast-iron block. I found that a hard brass washer under the thrust, would flow out sideways and make a neat cup nearly half an inch high. I noticed that the tendency to flow into this cup-shape, seemed to be increased because of the rotary motion of the pressure. In further experiments, after I had noticed this flow and the formation of the cup, I tried one in which the rotation of the washers was prevented. I found that if they were prevented from revolving, all the pressure I could get, even to

* By the kindness of A. C. Hobbs, Esq., General Superintendent of The Union Metallic Cartridge Company, I am enabled to give the following dimensions:

Outside diameter at A before tapering		....	0.581"
" " " " after	"	....	0.531
Thickness of metal at A before	"	....	0.012
" " " " " after	"	....	0.014

It is evident from these figures that a flow of the metal must have taken place longitudinally as well as radially.—W. F. D.

bursting one of the thrust-blocks, produced no flowing action at all.

Mr. A. H. Emery.—The reason of that was that the friction of quiescence replaced the friction of motion. The friction held it down in your latter experiments so that it couldn't flow.

Mr. Hamilton.—I was in the southern part of Ohio last week, where they are about changing the method of making nails, or rather changing the material from iron to steel. I went into an office there where a gentleman had contracted for half a million dollars' worth of machinery to make steel nails. He had specimens in his office and a hammer and anvil. He had nails of all sizes made from the same plate. It was impossible to break some of the nails or knock the heads off with a hammer. Other nails, made from the same material, would fly like glass. Those people are greatly troubled in regard to that matter. They have decided now that if they can cut steel nails at a certain "critical" temperature the head

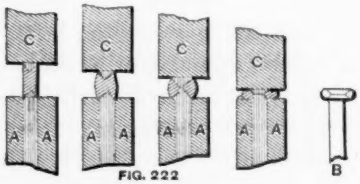


FIG. 222

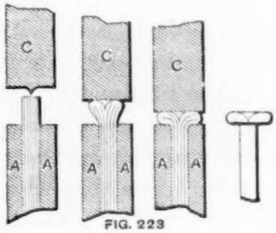


FIG. 223

will be all right. If they are a little too cold or too hot when cut, the nail is practically useless, and it is impossible to keep plates the same heat while being cut. If it is at the right temperature when they start, by the time they get the last nail cut off it is of too low a temperature. I took some of those nails and examined them carefully, and since this flow of metals has been discussed here, if you will allow me, I will show you what I think I have discovered in regard to the matter. I think it is all controlled by the flow of metals. By close examination I discovered marks like that on sketch of head of finished nail marked B, in Fig. 222, on almost every nail, especially those from which the heads came off so easily. In the process of manufacture the blank is first cut from the nail plate by the knives of the machine. The blank is then caught between the "gripping dies," marked A, and held firmly; the "header," marked C, then advances and crushes the protruding part of nail blank to the desired dimensions, thus forming the nail

head. In this operation of heading the nail there are two portions of this protruding material which, being supported, are practically undisturbed. This material is conical in shape, with base equal to the transverse area of the nail blank, and is as shown by Fig. 222. The remaining and largest proportion of the material is subjected to severe movement, causing a shearing action and a rupture between the parts as clearly shown.

Mr. Kent.—I also was in the southern part of Ohio last week and had some conversation about this nail business, and two weeks before I was in West Virginia and saw the nails made. The gentleman has given one explanation of this difficulty. There are three others. The nails are made out of strips of metal. These are piled together in front of the nail machine, which contains about fifty or sixty or more of these strips. As I passed through the mill I noticed that for larger-sized nails, 8 and 10 penny and larger, these bundles had been heated in a furnace very irregularly, so that the appearance of the pile was partly bright red, partly dark red, and partly black. There was scarcely one plate in the whole pile that was uniformly heated. The man began with his top plate. It took him an hour or more to get down to the bottom of the pile, so that there were hardly two nails alike in temperature. Many of the nails must have been cut while at the "critical temperature" of steel mentioned by Dr. Egleston at the New York meeting, at which temperature steel appears to become brittle. The other explanation of the flying off of the heads is a chemical one. Some specimens of nails were sent to Pittsburgh for analysis: some were good nails and some bad nails, and after a good deal of analyzing the chemist came to the conclusion that the heads of those nails that were low enough in carbon and phosphorus did not crack, and those high enough in phosphorus and carbon did crack.

Still another explanation of the case is the sharp corner under the head. You can make a piece of wrought iron of the shape of a nail, with a sharp corner, and there is not much danger of cracking; but as soon as you have a crystalline metal or granular metal like steel, it acts just like cast iron does in the foundry. You must have a fillet in the corner or you will have a strain.

So there are four explanations: the one given us by Mr. Hamilton, the chemical one, the question of the fillet, and the critical temperature. Which is the most correct is a matter for further research.

Mr. Hamilton.—I saw nails made from the very same plate. They kept the nails made from each plate separate. They investigated the matter very closely. They had their office full of nails of different kinds, made from different kinds of steels, and at various degrees of heat. Of course the lower in carbon the steel the better the nail; but from the same plate of steel one head would fly off like glass and another you could not hammer off, and I would say that in the steel nail the fillet that you talk about does not appear to make a particle of difference in practice. They have them with a large fillet and with a small fillet, and they know that it does not make any difference.

Mr. Dodge.—Was that little cross on those that broke?

Mr. Hamilton.—Yes, sir.

Mr. Dodge.—Not on the others?

Mr. Hamilton.—Sometimes on the others. When the steel plates from which nails are made are taken from the heating furnace and placed in the boxes to be used one by one by the nail feeder they are of uneven temperature. The workmen then resort to what they term "stuffing a pile" to equalize the heat; this is merely introducing cold plates between the plates that would be too hot to cut. This answers very well for iron, but is not satisfactory when steel is used. The placing of a small gas furnace has been suggested near each nail machine, the steel being first heated in the plate furnace, and placed in these small furnaces, when they can be kept at the critical temperature needed successfully to manufacture cut steel nails.

Mr. Hawkins.—It seems to me that if the metal flows as has been suggested—in the hypothesis to explain that cross in the nail head—that the metal should take the form of a cone, if anything—that is, that portion of the metal that does not flow. It cannot be conceived that it would not take the form of a wedge at all events, as the metal that does flow out is at liberty to flow in any direction, and consequently the remaining portion of the metal would be a cone: and there would not be any reason, therefore, why any cross mark should appear in one direction any more than another, or why there should not be any number of similar crosses all the way around it. It appears to me that the cross must come from some other cause, unless the metal itself moves simply as a wedge.

Mr. Durfee.—I do not know that what I am about to call the attention of the members to has anything to do with this cross on the head of the nail; still it may have. Any of you who have ever

seen an ingot of tool steel broken in two before there has been any hammer work done on it, have seen a crystallization very much as is represented in the sketch (Fig. 215). You will have seen a well-defined cross from angle to angle of the ingot, and all the crystals will be arranged perpendicularly to the exterior surfaces of the ingot. It occurred to me that possibly the cross described might have been produced by a similar crystalline arrangement of the particles in the head of the nail, as the head cooled. The crystals of metal arranging themselves as in the steel ingot, perpendicular to the cooling surfaces would produce just exactly the effect found in the nail head. The cross in the steel ingot is very sharply defined, and the arrangement of the crystals of steel is distinctly visible. It would almost seem as if they had been placed by some exact mechanical process.

FIG. 215

Mr. Oberlin Smith.—There is a gentleman over here inquiring about the cause of that wedge existing, instead of a cone. It occurs to me that such may possibly be the case on account of the different fibrous arrangement of the metal in the two different directions through the nail perpendicular to two of its adjacent sides. If this was a nail in which the fibers ran lengthwise this would not happen. Thus, if we were heading a wire nail and the metal was poor, the loosened part would probably be a cone. Obviously it is due to the friction against the header, and its base resting against the header under heavy pressure causes considerable resistance to its flowing out at that point. That other construction, with a pointed header, shown upon the blackboard, is of course intended to obviate such a difficulty.

Mr. Hawkins.—I presume that the section of the metal being square it might assume that form. It would necessitate, then, these four distinct crosses appearing at right angles to one another.

Mr. Smith.—I will recall what I said about the wedge. I thought the crosses were on the two sides. But as it is not a wedge, only a pyramid, that explanation about the fibers being arranged differently across the nail has nothing to do with it.

Mr. Hamilton.—There ought be a difference, it seems to me, in the action of metal whether it is made to flow under a number of blows and slowly, or whether it has to change its form very rapidly without giving it time to adjust itself. In the case of these nails,

they are headed four or five hundred a minute, and there is not much time for flow of metals.

Mr. A. H. Emery.—And yet there is time to flow. In the case of the hammer, it is a succession of little blows. There is no relief of the pressure in the case to a die, and it must flow, if it flows at all, under that resistance to flowing due to friction.

Mr. Dodge.—Will the flow go on—will it flow after it starts, even when it is not struck again immediately ?

Mr. Emery.—The flow is only when the pressure of the hammer is there, but the flow is slight because the flow is produced by a succession of blows. Whereas in the die, one continuous pressure exists until the whole head is formed.

Prof. Webb.—I can readily perceive how the flow could go on after the hammer had struck it.

Mr. Emery.—I could more readily conceive how it could not.

Prof. Webb.—I could more readily conceive how it could not, but I could conceive how it can. You have stored the force of the blow by compressing a particle of the iron ; why should not that push its way out and equalize itself.

Mr. Kent.—I have observed the flow of metals for half an hour after the pressure was relieved. A straight bar of metal, being put on a testing machine, was pressed down by transverse strain until it took a certain permanent set, so-called, and the load removed. During the next half hour and perhaps longer that bar kept straightening itself. That straightening was proved not only by measurements in thousandths of an inch—and it was a very considerable quantity, I think two or three hundredths of an inch—but on the platform scale as well, and after relieving the pressure and balancing the scale at zero, after a while the beam rose and we had to move the poise along to balance it again, and to move the poise continuously for half an hour. A description of the phenomena was published in the Transactions of the Civil Engineers by Professor Thurston. It happened about eight or nine years ago.

Mr. Albert Emery.—I have seen a great many cases where the strain exceeded the limit of elasticity and the metal yielded temporarily under that strain, and there is a constant effort by that metal to recover itself and to come back again. I remember a pressure gauge which I bought of the maker, on which I put the maximum load intended, nominally 600 pounds to the inch, but really, as I afterward found, only 525 lbs. per square inch. With this load the metal of the bent tube took a flow under the strain, which

exceeded the elastic limit slightly, and gradually got back to zero in a day and a half; but that flow was backward.

Mr. Babcock.—In regard to that cone matter, any of us who have witnessed the giving out of a brittle material like a piece of marble or stone under direct pressure have seen that action take place; the block yielding around the center and leaving a cone at the top and a cone at the bottom. I can conceive that such an action would take place in a metal. The tendency would be the same. A piece of steel of a brittle character would necessarily take that form, and might produce the effect the gentleman referred to.

Mr. Hawkins.—I would like to give an example that came to my notice a good many years ago. We used to have, in the construction of printing presses, a piece called a roller frame that consisted of a casting that had two members of about a quarter of an inch thick by an inch and three-quarters when finished, and they were held together by short cross-ribs so as to form a frame, within which to fit the rollers, and these frames were some of them as long as twelve or thirteen feet, and it was found very difficult to get them straight enough without casting them very thick. It was discovered at the time that we could take one of those castings when crooked and pene it at one side so as to bring it approximately straight. After we had pened it and got it under strain so as to bring it nearly straight, we would drop it gently upon an anvil or some solid substance so as to assist the strain in giving it a permanent set in the direction that would allow the strain to act upon it. Then we could plane that casting, and it would remain straight.

Mr. Couch.—Nearly every one who has made surface plates of any size, has found an advantage in rough-planing them, and then letting them lie for two or three weeks before giving them their final planing. After some metal has been removed from one side of a casting having considerable surface, a re-arrangement appears to go on for some time, which changes its form more or less.

Mr. Towne.—In connection with what Mr. Couch has said, I may mention that I recall the case of a large lathe-bed, the making of which came under my observation a number of years ago, of from twenty to thirty feet in length, the pattern for which was made with a camber of two or three inches, the camber being allowed not merely for the straightening of the casting in cooling, but also to allow for a further straightening expected to result from the finishing of the bed by planing.

Mr. A. H. Emery.—I would say in regard to this flow of metals,

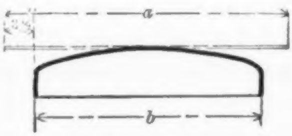
that it has seemed to me for many years, and I see no reason as yet to change my views, that this flow takes place only so far as to relieve the metal to this extent—to reduce the existing strain to some point somewhat below the limit of elasticity, or to the limit of elasticity, and it is not necessary to reduce it, it seems to me, substantially below that, to allow the metal then to remain in that permanent condition. As an instance of the strain remaining in the metal at all times, the Rodman guns were cast, and a heavy strain put upon the casting when cooling, and the Department was of opinion that the strain gradually relieved itself, so that in a few years it was quite gone. I told them that up to the limit of elasticity it remained forever, and I instanced the case of a steel spring which I had had confined under a strain of fifty or sixty thousand pounds to the square inch for ten years. As a proof that the strain remains in the casting up to the limit of elasticity or thereabouts, I would mention that three years ago last fall, at the South Boston Foundry, a fifteen-inch Rodman gun, which had been cast for the navy and had been fired 350 rounds, and had been condemned for powder-cutting around the vent, was cut up. Just in front of the trunnions a cut was made where the diameter of the gun was about forty-four inches, and the bore fifteen inches. When the cut had gone as far as two inches, it relieved the longitudinal strain of tension in the outer wall at that point, sufficiently to allow the longitudinal strains of compression on the interior of the gun to exert themselves, and to expand that part of the gun farther. This expansion of the interior of the gun longitudinally was sufficient to cause rupture of the exterior at the bottom of this cut, so that a large section of this gun, which from its area required five or six million pounds to rupture it, was ruptured, and the gun parted in the lathe clear through on one side with quite a noise. That allowed the other side to straighten itself and relieve itself from strain, so that only about two-fifths of the section of the gun was not parted by the rupture. The metal on test showed itself to be fully as good quality as when the gun was cast seventeen years previously, when cast and tested and previous to these firings.

Prof. Webb.—I would ask whether pickling removes enough of the scale to relieve the strain, and allow a casting to change a little in shape.

Mr. Walker.—I think it does. I have had castings break while they have been in pickle, and also after the pickle has been

washed off. I do not think it has been due altogether to the heat generated, though, no doubt, the chemical action of the acid had something to do with breaking the casting in question, as the breakage was in the opposite direction to that of the usual strain. Hence, I think that pickling does alter the surface part of a casting, and possibly its shape.

Most of you are aware what my sketch represents. Figure 229 is a good illustration of the "flow of metals" in the form of a steam dome or boiler head. The circular plate, a, represented by dotted lines, shows the original form, the full lines represented by diameter b, is the head after it is formed. The difference in diameter between a and b is seven inches, and hence there is a difference in circumference of about twenty-two inches. Now, as the flow of metals has been gone into, I would like to ask some one to explain where this difference of circumference goes to? This class of work is done in Europe. I had the pleasure of seeing a sample of this kind in 1875, at the office of the Manchester Boiler Inspection & Insurance Company, which had been formed cold.



TOP FOR STEAM DOME
FIG. 229.

Mr. Partridge.—The author says that this law of flow controls alike all substances. Mr. Babcock refers to the action of marble under compression as illustrating this. The action of marble under strains tending to produce bending appears to be similar to that of a metal. I have frequently seen a sheet of marble forming the front of a shelf over a steam table which has bent at its unsupported edge some two inches from the effect of the weight upon it and the heat of the steam table. The slab was about $\frac{3}{4}$ of an inch thick and 40 inches long. The weight on it would vary from 25 to perhaps 125 pounds. There was no sign of fracture in any part. It was replaced about two years ago by a new sheet, which I notice is already showing decided signs of sinking. The temperature is probably never less than 120 degrees, and in the summer much more.

In stamping a deep dish, the flow of metal evidently takes place in all directions. A part is crowded together while portions are spread out, and the result is an increase of length, making the perimeter greater than the diameter of the blank. If the press is suitably arranged it is possible, to a certain extent, to force the metal to flow back upon itself, and manifest its flow by thickening the

plate. Where a printed sheet of tin is used, the direction of the flow and the peculiar action of the metal can be seen very clearly. The depth to which deep stamping can be carried in tin is quite surprising. I have had samples of stamped work about $3\frac{1}{2}$ inches in diameter by 7 inches deep, and tapered like a tin lemonade-mixer.

Mr. Stratton.—I think the question of time has a great deal to do with this matter of the flow of metals, as has been already said. Some time since, in reading the pamphlet issued by the Hoopes & Townsend Company, of Philadelphia, I was quite impressed with the idea that it was all important to know in how short a space of time they concentrated this pressure on the punch and forced it

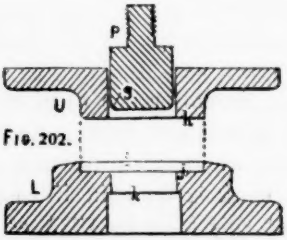


FIG. 202.

FIG. 203.

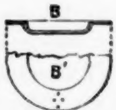


FIG. 204.

FIG. 206.

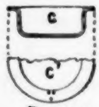


FIG. 205.

FIG. 207.

FIG. 208.

through the nuts, or how rapidly they turned them out. If Professor Thurston were here he would probably enlighten us on this subject, as he made an exhaustive investigation of it. I would recommend members of the Society to send for this pamphlet, as it contains much that is of interest on this subject. It is very fully and nicely illustrated. The cuts of the nuts show them as etched with acid, so that the flow of the metal is thoroughly shown.

Mr. Oberlin Smith.—I have done a good deal of work in "drawing" sheet metals—iron, steel, brass, copper, gold, not principally the latter, however, and tin plate. The process is probably known to a good many, but it may not be familiar to all. The upper die, *U*

(Fig. 202), descends first and the punch, *P*, with it, or approximately with it. If the dies are "combination dies" (that is to both cut and draw), the "blank" shown at *A* (Fig. 203) in cross-section and *A'* in top view, is cut, during the descent of *U*, from a sheet of metal laid upon the female cutting edge, *i*. If these dies are not to cut, the edge, *i*, is rounded off and need not be of steel, as it acts merely as a guide for the blank, which has been cut somewhere else, and is laid in upon the surface, *j*, of the lower die, *L*. As *U* completes its downward stroke its lower surface, *h*, just touches the metal blank—in practice it touches it very firmly and under a heavy pressure, to allow for the springing of the press. The die, *U*, now being stopped and remaining exactly there (actuated by a cam motion usually), the punch, *P*, descends and draws the metals in the successive forms shown at *B*, *C*, and *D* (Figs. 204, 205 and 206). Cylindrical work, like *D*, drops through the die, being prevented from rising with the punch by the sharp stripping edge, *k*.

Of course the action upon the metal, as Mr. Partridge says, is a flow in all directions. The particles at the outer edge all have a flow outward. The primal flow is circumferential, because, as the blank tends to get into a smaller diameter, the circumference becomes smaller. Thus, all the particles are crowded toward each other circumferentially and stretch apart radially to get out of the way. As Mr. Partridge stated, on tin plate or other metal which has been marked, it is very apparent how the metal has moved. I had some blanks that were marked off in lines $\frac{1}{16}$ inch apart at right angles, and it was very easy to see which way the particles of the metal had gone. This movement is shown in Figs. 203, 204 and 205, where certain four particles of metal in the blank are represented by four dots at the corners of a square upon *A'*. Upon *B'* and *C'* can be seen the successive changes in position of these dots as the square becomes a more and more elongated diamond.

I have a formula at home for determining the size of the blanks for given work according to its diameter when finished, its depth, and the amount of rounded corner. It is astonishing to a novice to see how a comparatively hard metal like tin plate will stand this process. Ordinary tin plate will draw to a depth of about half its diameter at one operation. Beyond that limit you cannot go far. When you use sheet iron and steel, the proportions are about the same as with tin plate. When you come to brass and copper, of course you can get a much greater depth at one operation. With gold I have done a little in the way of watch cases, etc., and it

works about the same as copper. Copper you can get deeper than brass. With all these metals it is necessary that surfaces *h*, *j* should come together with the proper pressure thereon; but if the pressure is too heavy, the friction becomes so great that the punch bursts through. If it is too little, the metal begins to wrinkle. The mean point between too great pressure and too little pressure prevents either wrinkling at *h*, *j*, or tearing through around the corner of punch at *g*. In a second operation, where deeper work is wanted, another pair of dies is used. The blank-holder is made thin enough to come down inside the first-made "cup," and with a ductile metal like copper you can continue this process three times or more without annealing. Then you anneal and do it again. With tin plate, which you cannot anneal, of course the difficulties are greater. You can draw that in about three operations. I have seen a tin-plate box twice its diameter in depth, but such proportions are rare. They draw teapots and pitchers and all such work as that, and they close them in afterward at the top by spinning. Such work, however, is usually drawn from black iron, so that it can be annealed, and it is tinned subsequently.

Mr. Kent.—How does mild steel compare with copper in its ability to be drawn?

Mr. Smith.—I have not made accurate experiments to compare them, but I have noted casually that it draws as well as sheet iron, but not so well as copper. I cannot, however, give figures showing the amount of difference.

Mr. Stratton.—Does it make any difference with what rapidity the die is worked in the drawing of these metals? If it is worked quickly, will it have a tendency to crack more than if it is moved slowly?

Mr. Smith.—Yes, sir. Iron will, however, stand a faster flow than brass or copper. It would seem that it should be the other way, but I believe the case to be as I have stated it. In all work of a kind like Fig. 207, *E*, the sides are wrinkled; it cannot be drawn smooth. This cylindrical work, *D*, can be made smooth, because its flange is held by the pressure almost up to the punch. But the conical work, *E*, is unsupported from where it leaves the surface, *j*, over to the smaller end of the punch. There is a little annular disk of metal there unconfined, consequently the wrinkle commences to form just as it passes down the corner of *j*, and continues down all the way. This defect has to be overcome by "roller-spinning." This is the way that dish-pans, wash-basins and such

things are all done. They are drawn in one operation, but roller-spun afterward. In practice, whenever we set our dies to the best advantage, so as not to wrinkle the metal nor tear it, the metal remains of the same thickness after drawing as it was in the original blank. In cartridge work the blank is made so much too thick for the die that there is not room for the metal, and in such case the work is drawn thinner, in a manner analogous to wire-drawing, but where the punch allows room for the original thickness of metal, there is of course no making thinner, neither is there making thicker—the fixed space between *h* and *j* not permitting. So our formula for determining the diameter of our blanks is based upon the fact that the total surface area of the drawn cup, or other work, is exactly the same as in the original blank. Sometimes where we have a new thing come to us (that we have not made before), of such a shape that it is hard to calculate the area, we take such a sample and weigh it and measure the thickness accurately. We know the weight of a square inch of such metal, and we can calculate its area from that, generally finding it come out about right. There is frequently a good deal of money spent in trying blanks for a new pair of dies. This process of determining the area—making the area of the blank just the same as the area of the work—is all that is required, unless, indeed, there is some deeply embossed contour made by the end of the punch, which necessarily stretches the metal thinner.

Prof. Webb.—Would Mr. Smith sketch some two or three of those tenth-inch squares. Perhaps he could just sketch on a circular plate the shape that they assume afterward.

Mr. Smith.—I do not exactly remember how they appear on the circular flange, where a flange remains on the work. Of course they are all this way [illustrating] on a side view of the cylinder—outside view. This first line, which was a radial one in the original blank, would run straight up, as an element of the cylinder, and the next one would run in toward it in this way. This next line would converge still more, especially at the top, and next one more yet, and so on.

Mr. Partridge.—I would like to ask Mr. Smith if he does not find that the metal flows around the circumference of one of those deep dishes, and there are points where the metal has been flowing horizontally, and there are points where its flow has been vertical.

Mr. Smith.—Yes, sir; slightly so—with very deep work considerably so. I attribute it to three causes—one is irregularity in the

die, there being a slightly less space in some places than in others; another analogous cause is extra thick places in the metal; and the third is harder or softer spots than the normal hardness—perhaps with irregularities in fibrous construction. It is chiefly due however to the irregularities of the dies—the surfaces not being perfect planes—and to the dies springing. If a die is made flat, it will often bend down in spots, and a wrinkled place will start even though the dies are perfectly true and parallel with each other. The secret of building successful drawing presses is to make the horizontal members of the presses enormously deep, so that their rigidity will prevent the dies from springing "out of flat."

Mr. Partridge.—I referred to a matter altogether different from that. From such measurements as I have been able to make on drawn work with pieces of tin lined in this way, and radially, I came to the conclusion that when it was drawn those lines instead of being equally disposed around this side would be far apart here, one going here and so around—a regular rhythmic series around the whole plate. In other words, that there were points determined by the grain of the metal where the metal will come together. There were other points where it flowed lengthwise, and that these printed patterns showed that that was the case even when the drawing was perfect.

Mr. Smith.—I would say that there is sometimes that irregularity, although I do not see why it should be in rhythmic form unless it is in the lathe leaving the die slightly but regularly corrugated. I have noticed that there is a difference in the grain of the metal. In tin plate there is not much difference, because it is rolled crosswise in both directions, but in long strips of brass the grain runs lengthwise only. There is a tendency for the blanks to stretch sideways the most, but the extra thickness of the sheets in the center (caused by the rolling-mill rolls springing) gives more pressure and consequent radial stretching, thus having a tendency to counteract the other evil. With regard to the surfaces h and j, we find that if made very flat and then oil-stoned in a radial direction the metal will stand considerable more pressure without tearing through than if they were polished.

Mr. Kent.—Has the radius of the fillet any influence?

Mr. Smith.—Yes, sir, assuming that by "fillet" you mean the rounded corner near j. In practice, for tin plate and brass work we make it from $\frac{1}{16}$ to $\frac{1}{8}$ inch, but more would be better as far as reducing the pull upon the metal is concerned. If, however, we

make it too great we lose so much of the bearing surface of *h* as to cause wrinkles. At Figs. 207 and 208 are shown forms which are very difficult to draw, because there is so much flange to *make flow* in proportion to the circumference of metal (at the end of the punch) that has to do all the *pulling*. In such cases the work is very apt to have its bottom torn out.

CLXXXIII.

REPORT OF A SERIES OF TRIALS OF A WARM-BLAST APPARATUS, FOR TRANSFERRING A PART OF THE HEAT OF ESCAPING FLUE GASES TO THE FURNACE.

BY J. C. HOADLEY, BOSTON, MASS.

THE experiments of which an account will be found in the following paper were begun in the summer of 1881, and, with the interruptions required for the modifications of the apparatus, occupied nearly a year. They were conducted at the chemical works of the Pacific Mills, Lawrence, Mass., by Mr. Fred. H. Prentiss, under the direction of the writer. Ever since the conclusion of the last weekly experiment, May 20, 1882, the apparatus has been in uninterrupted use, and appears to be still in good order, with fair indications of reasonable durability—a point to be settled only by continued use. A number of causes have delayed the publication of this report: the unusual scope of the experiments, the great length of the boiler tests—embracing nine full weeks—the number of subjects investigated, the attempt to ascertain everything which could affect the result—taking for granted nothing but the well-established physical laws concerned—have resulted in a large mass of notes which required much labor for their proper digestion. Its publication has been still further delayed—not unwisely, perhaps—in order to gain, by experience in practical use, some knowledge of the advantages and disadvantages of the apparatus, as time alone can reveal them.

The teachings of these experiments are little less valuable on their negative than on their positive side. It is hardly less worth while to know the absolute limitations of economy in coal combustion; to know what cannot be done, though quacks promise never so largely, as to learn by what means some part of the important loss of heat inevitable with existing arrangements, may be arrested and put to use at reasonable cost and without undue trouble or inconvenience.

On both these points, it is believed, some contributions of real value will be found in this paper. Much, perhaps most of it, is only confirmatory of facts previously known; but in some respects have been here based on broader, more complete and longer-continued experiments, with the aid of some new instruments. Single boiler tests,

as boiler tests are usually conducted, are of very limited value. Too many unfounded assumptions are usually made. "Coal" is taken as equal to something to be found in tables, sometimes even without allowing for surface moisture, which may be dried out; yet there is more difference in coal than there is in boilers, rejecting boilers notoriously defective, and surface water will range all the way from 0.5 per cent. to 8 per cent. "Steam" is taken as of fixed and standard quality, as if it were dry, saturated steam, which is possible only when no steam is drawn from the boiler, and when none has been drawn from it for some little time.

The hygrometric condition of the air is neglected, and its temperature and its barometric pressure, or, if observations are taken of the hygrometer, thermometer and barometer, the corrections these instruments would supply are rarely made. Steam-gauge pressures indicate different absolute pressures, and different quantities of heat, at varying barometric pressures.

Little attention is usually given to the question: How large a proportion of the air in the chimney gases really passes through the incandescent fuel on the grates? and how much infiltrates at cracked or ill-fitting doors, at cracks in the brick-work, and between the brick-work and arch front, or through the brick-work itself? Lastly, it is believed that this is the first serious attempt, outside of the technical school or laboratory, to carry out a thorough, continuous analysis of flue gases—by far the most important point of attack upon the difficult problem of coal combustion. Unless the composition of the escaping gases is known, nothing is known; this accurately ascertained, and their weight and temperature, almost everything which it is desirable to know is ascertainable.

Some of the instruments devised and constructed for these experiments, and used in carrying them on, will be found of interest. Such are the calorimeter, the water-platinum pyrometer, the two-fluid anemometer and the incased aneroid barometer.

This warm-blast apparatus seems to afford a means of securing a net saving of 10 to 18 per cent. over the best attainable practice with natural chimney draft and with air supplied to the furnace at usual external air temperatures; at least five times as much as can be saved by any and all other methods, save the Green Economizer, which is an analogous device, only available where large quantities of warm water are in constant demand; and should commend itself to the attention of all large consumers of coal, as soon as the durability of the apparatus is well established by sufficiently

protracted use. There are some incidental advantages, growing out of the more complete control of the rate of combustion ; and there is, it must be said, an offset to these advantages in the more rapid deterioration of fire grates, the importance of which can only be determined by prolonged experience.

The expense of these experiments, which grew out of a suggestion at the end of a pamphlet "On the Combustion of Fuel" * was borne by an association of mill owners and manufacturers. Their object may be stated as follows:

1. To ascertain how large a portion of the heat generated by the combustion of commercial coals, with the best attainable practice by natural chimney draft, escapes through the chimney, serving no useful purpose except in producing the draft.

2. To ascertain what portion of such escaping heat could practically be arrested and returned to the furnace in a warm blast, by means of an apparatus of admissible size and cost.

3. To determine the form and dimensions of apparatus sufficiently well adapted to this purpose.

4. To ascertain the cost of driving a blower to supplement the loss of chimney draft suffered in consequence of the reduced temperature of the finally-escaping flue gases.

5. To obtain by observation the data for striking a balance of advantages and disadvantages resulting from the use of such apparatus, as compared with natural draft, under conditions substantially similar; and

6. To obtain as much information as such experiments could be made to yield upon all questions relating to the economical combustion of coals and the generation of steam.

It will be apparent, on reflection, that the problem was far from simple, and by no means easy. It would not do to confine the experiments to a boiler with the warm-blast apparatus, and then to institute a comparison with alleged results obtained in ordinary practice, since there might easily be found in "ordinary practice" defects of care, skill or arrangement, which would make the comparison unduly favorable to the device. Again, the use of a blower, or exhaust-fan, by giving control of the draft, would give facility for more rapid combustion, and, consequently, for more rapid steam generation, which, unless guarded against or duly allowed for, might, by increased "priming"—water entrained with the steam but unevaporated—have given a deceptive appearance of ad-

* See Appendices III. and IV.

vantage arising from a positive loss; a favorite ruse of empirical boiler-improvers time out of mind.

It was therefore thought necessary to lay out a comprehensive series of experiments; *first*, with a boiler similar in form, dimensions and setting to all the fifty boilers of the Pacific Mills, in order to ascertain just how near to theoretically perfect conditions we could bring that boiler, in actual practice, week by week; and *secondly*, just what proportion of the inevitable loss of heat was suffered at the chimney, and what degree of efficiency was attainable.

This knowledge gained, as a secure basis of comparison, similar experiments, modified only so far as necessary to adapt them to the modified arrangement of the boiler setting, were carried out with the boiler fitted with the warm-blast apparatus: the two sets of experiments being designated, for distinction, "cold blast" (or Pacific) and "warm blast."

The observations covered the following points:

1. Coal.—Time of each firing and quantity fired; quality and condition; temperature; samples taken at every firing, and analysis of the daily samples.

2. Refuse.—Divided into "cinders," picked out by hand, yielding by analysis about 41 per cent. of carbon; partly burned coal, about 62 per cent. carbon: and ashes, of several grades, about 14 per cent. carbon. Several screens of different degrees of fineness were used, and the several grades were weighed, sampled and analyzed, for the few first weeks. But a very perfect check upon this work (which will be pointed out farther on), enabled us to dispense with these laborious and costly analyses of refuse.

3. Water.—Quantity fed into the boiler; time and weight noted every time a tank was emptied; height of water level in glass water-gauge attached to boiler—temperature and height noted every quarter of an hour.

4. Air.—Quantity, with cold-blast, deduced from the composition of the flue gases, determined by continuous analysis, together with the tension of these gases, and their temperature: the tension ascertained by means of a large aneroid barometer inclosed in an air-tight case, communicating through a tube with the flue, and, by a three-way cock, with the atmosphere; and the temperature by means of mercurial (chemical) thermometers, inserted in tubes filled with sperm oil, set in the flue. With the warm blast, in addition to the foregoing, a record was kept of the revolutions of a Root blower

of known measured capacity, and ascertained rate of leakage. The hygrometric state of the air was deduced by quarter-hourly notes of a hygrometer. The temperature of the external air and of the air of the boiler-room was regularly noted, and with the warm blast, the temperature on entering the "abstractor," to be warmed by the outflowing gases, and again on emerging from the abstractor, to enter the ash-pit.

There was also a hot-air flue for highly heating (at will) a part of the air, with provision for introducing it at a "split bridge," with dampers to regulate its admission, and provision for observing the temperature of such highly heated air.

5. Gaseous Products of Combustion.—Continuous analysis by the gravimetric method, each forenoon's and each afternoon's production by itself, with occasional special examinations of shorter periods, to observe the effect of modes of firing, of introducing hot air, and other variations from the usual conditions. The gases given off during the night from banked fires were also continuously analyzed and their volume determined, in the experiments with cold blast; but with the warm blast, the dampers were finally made so tight that no current could be detected, and the loss—whatever it was—could not be estimated. This gravimetric method of gas analysis, which is very interesting and not hitherto generally practiced, will be fully described in its proper place.

6. Steam.—Pressure recorded by an Edson pressure-recording gauge, and noted every quarter of an hour by a test gauge, known to agree with a mercurial column; supplemented by quarter-hourly readings of a signal service (mercurial) barometer; and its quality as to saturation, moisture or superheating ascertained. This was done with cold blast, in which case the boiler had no superheating surface; by a steam calorimeter, to be hereafter described; and with warm blast, in which case there was ample superheating surface and constant superheating in fact, by the thermometer.

7. Fire.—Temperature in center of incandescent coal, at bridge wall, and at the pier, where the gases are about to enter the boiler flues, taken by the water-platinum pyrometer.

8. Flue Gases.—Their temperature on emerging from the boiler flues, in smoke-box; on entering the abstractor; on emerging from the abstractor, and on passing to the exhaust blower.

9. Brick-work.—Radiation from its surface; conduction of heat from inside to outside.

For carrying out these experiments, several new instruments, or

new forms of old ones, were devised and constructed. The more important of these will be found described in the proper place.

It is obvious that these observations and experiments could not all be carried on simultaneously and kept up throughout the whole period covered by the tests, without a larger force of assistants than it would have been judicious to employ. Nor was this necessary. Calorimetric experiments on the quality of steam, for instance, which are delicate and laborious, demanding the closest attention of skillful observers, can be so timed with respect to the rate of steam generation and consumption as fairly to represent ordinary conditions. Such experiments were, in fact, confined to one week, July 11–16, 1881, when fourteen experiments, fully detailed in the appropriate place, were made and recorded.

Pyrometric experiments in the fire, at the bridge wall and at the pier, were chiefly directed to ascertaining the temperature of new fires, well-kindled fires, new, old, or spent fires, and banked fires, with anthracite and with bituminous coal.

Experiments on the radiation and conduction of brick-work were made as time and convenience would permit.

Valuable information was obtained on the necessity of carefully sampling the gaseous products of combustion, which exist in flues and chimneys in most heterogeneous mixtures, far from being equally diffused; and on practicable methods of satisfactory sampling, all of which are fully described.

The power consumed in driving the suction blower was carefully ascertained. Some curious experiments, not devoid of interest, were made to ascertain the quantity of solid carbon carried off in black smoke with the chimney gases from bituminous coal—a very small proportion of the carbon consumed.

Each one of the tests of evaporation here reported was carried on continuously during an entire week. Early on Monday morning, the boiler and the water it contained being cooled down nearly to the temperature of the boiler room, a wood fire was lighted and kept up until the steam gauge showed about 50 pounds pressure per square inch, whereupon the fire was drawn, and the furnace and ash-pit were cleaned out. A quantity of wood, usually about 260 pounds, weighed and sampled for analysis,* was then put on the fire grate for kindling, and coal was thrown on at the discretion

* Several analyses of the wood were made, but as these analyses are troublesome, as the quantity of wood used was small, and as dry wood is nearly uniform in composition, the usual ratio, 40 per cent. of coal, was adopted.

of the skillful and attentive fireman, and weighed at every firing. A platform scale, fitted with a box of plank, having sides and a back, but open in front, was kept exclusively for weighing coal. 500 pounds of coal filled the box conveniently full—the box itself being balanced by a counterpoise on the scale beam. The weight of a charge and the time of charging being noted, the weight was again taken and noted after each firing, and the time of opening and closing the fire door was also noted. The successive differences were the quantities thrown on the grate at the respective firings, and their sums were the total quantities fired during the period covered by the notes summed up.

The notes of each day's firing were plotted, graphically, on section paper, to guard against errors and omissions.

Near the close of the day, as early as the demand for steam would permit, the fire was "banked," all dampers were closed and so left till morning, when the dampers were opened, the fire was cleaned, and fresh coal was thrown on. It is, therefore, evident that all the fuel consumed during the week has been charged to the boiler, except the wood consumed on each Monday morning in raising steam to about fifty pounds pressure.

The actual pressure at starting the fire and at opening the dampers in the morning, was observed and recorded, together with the height of water in the boiler, this latter being taken from a scale attached to the glass water-gauge; and similar observations were noted at stopping, as well as every fifteen minutes of the day—and sometimes of the night—and the differences in height of water and in pressure of steam, between starting on Monday morning and stopping at noon on Saturday, were duly allowed for. A table will be found on a subsequent page giving the number of pounds of water contained in the boiler at each inch in height of the glass water-gauge, from 0 to 10 inches, and for pressures varying by 5 pounds, from atmospheric pressure to 80 pounds above, with differences for convenience of interpolation.

As to the omission of the quantity of wood consumed in raising steam on Monday morning, it is proper to forestall criticism by the remark that in no other way could the several trials be made so strictly comparable as by starting and stopping in each case, as nearly as possible, with steam at the same pressure and with water at the same level. The same method was pursued in every case, so that the comparison of one case with another is as just as it seems possible to make it without continuous uninterrupted firing.

The anthracite coal was Lackawanna, taken from "pockets" in Boston, egg size, very uniform, and of good quality and reasonably dry, the analysis showing only 2.78 per cent. of water.

The bituminous coal was Cumberland, kept under cover, and was also of good quality, and contained even less water than the anthracite.

Samples about as large as a coffee-bean were taken at each firing —averaging about one from every tenth lump (of the anthracite), each full day's samples filling a compartment three inches cube, in a box containing six such compartments; and all the samples of each week were pulverized and treated in the usual manner.

Two independent analyses were made of each week's samples, and sometimes, when there appeared to be too much difference, a third analysis was made for confirmation or correction. A considerable quantity of each of the pulverized samples, each separately bottled and labeled, is preserved for future verification, if desired.

A summary of the results of coal analysis is subjoined (Table I.), the anthracite used with cold blast being the mean of five weeks' firing.

TABLE I.

CONSTITUENTS OF COAL.	BOILER WITH COLD BLAST.		BOILER WITH WARM BLAST.	
	Anthracite.	Bituminous.	Anthracite.	Bituminous.
Carbon	82.43	81.03	81.51	81.71
Hydrogen	1.86	3.84	1.89	3.79
Ash	10.12	7.19	11.83	5.75
Water	2.78	.63	2.49	1.02
Oxygen		4.49		4.91
Nitrogen		2.00		2.00
Sulphur	2.81*	.82	2.28*	.82
	100.00	100.00	100.00	100.00

The two boilers with which experiments were made were precisely alike, and were substantially like all the boilers in use at the Pacific Mills, about fifty in number, some of which are a little less in length. They are of the class known as externally fired, return tubular boilers. The cylindrical shell, of flange iron 0.375 inches thick, is 60 inches in diameter outside of the small courses, double-riveted in the longitudinal seams, and 21 feet in extreme length,

* The Oxygen, Nitrogen and Sulphur not separated in the anthracite.

of the skillful and attentive fireman, and weighed at every firing. A platform scale, fitted with a box of plank, having sides and a back, but open in front, was kept exclusively for weighing coal. 500 pounds of coal filled the box conveniently full—the box itself being balanced by a counterpoise on the scale beam. The weight of a charge and the time of charging being noted, the weight was again taken and noted after each firing, and the time of opening and closing the fire door was also noted. The successive differences were the quantities thrown on the grate at the respective firings, and their sums were the total quantities fired during the period covered by the notes summed up.

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The anthracite coal was Lackawanna, taken from "pockets" in Boston, egg size, very uniform, and of good quality and reasonably dry, the analysis showing only 2.78 per cent. of water.

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Samples about as large as a coffee-bean were taken at each firing —averaging about one from every tenth lump (of the anthracite), each full day's samples filling a compartment three inches cube, in a box containing six such compartments; and all the samples of each week were pulverized and treated in the usual manner.

Two independent analyses were made of each week's samples, and sometimes, when there appeared to be too much difference, a third analysis was made for confirmation or correction. A considerable quantity of each of the pulverized samples, each separately bottled and labeled, is preserved for future verification, if desired.

A summary of the results of coal analysis is subjoined (Table I.), the anthracite used with cold blast being the mean of five weeks' firing.

TABLE I.

CONSTITUENTS OF COAL.	BOILER WITH COLD BLAST.		BOILER WITH WARM BLAST.	
	Anthracite.	Bituminous.	Anthracite.	Bituminous.
Carbon	82.43	81.03	81.51	81.71
Hydrogen	1.86	3.84	1.89	3.79
Ash	10.12	7.19	11.83	5.75
Water	2.78	.63	2.49	1.02
Oxygen		4.49		4.91
Nitrogen		2.00		2.00
Sulphur	2.81*	.82	2.28*	.82
	100.00	100.00	100.00	100.00

The two boilers with which experiments were made were precisely alike, and were substantially like all the boilers in use at the Pacific Mills, about fifty in number, some of which are a little less in length. They are of the class known as externally fired, return tubular boilers. The cylindrical shell, of flange iron 0.375 inches thick, is 60 inches in diameter outside of the small courses, double-riveted in the longitudinal seams, and 21 feet in extreme length,

* The Oxygen, Nitrogen and Sulphur not separated in the anthracite.

including the smoke-box cover; the smoke-box at the front end being 1 foot long, and the flues 20 feet. These are 3.5 inches in diameter outside, lap-welded iron tubes, set in squares and in straight rows both horizontally and vertically, 4.5 inches between centers, and therefore with 1 inch clear space between them.

They are arranged in 7 horizontal rows; 4 rows of 11 tubes each, one of 9, one of 7, and one of 5, making 65 tubes in all. The middle tube of the row next to the upper row, is in the center of the shell, which leaves at the bottom a space of 5.37 inches between the lower side of the flues and the inner side of the small courses, 4.87 inches between the flues and the rivet-heads, and at the nearest, 3.09 inches, radially, between the flues and the smaller courses, and 2.59 inches between the nearest tubes and the rivet-heads. The provision for water circulation is therefore sufficient, and is further aided by setting the smoke-box entirely forward of the arch front, so that a length of 12 inches of the water space at the front end, immediately back of the smoke-box, is embraced in the brick-work, and shielded from the direct action of the fire, which, it is believed, produces a downward current at that point, to supply the rapid evaporation directly over the fire-grates. These are 5 feet 2 inches long from the fire-brick lining of the arch front to the bridge wall.

The fire-grates of the original "Pacific" boiler, with which the cold-blast experiments were made, were 5 feet wide between the side walls; those of the new boiler, with hot-blast apparatus, 5 feet 4 inches wide. The side walls of the Pacific boiler are offset above the grates, until at the level of the bridge wall, they are 5 feet 6 inches apart, at which point they are 24 inches thick; and are closed over against the boiler at the middle of its height, where the space is 3 inches to the smaller courses, 2.62 inches to the larger courses, and 2.12 inches to the rivet-heads. The brick-work closing the space between the side walls and the boiler, is 9 inches in depth, and above it the right-hand side wall is carried up 3 inches above the top of the boiler, where the covering bars are laid on. The left-hand side is occupied by a horizontal brick flue, conveying the gases of combustion, received from the smoke-box through a plate-iron smoke bonnet, to the rear of the boiler setting, where a vertical brick flue, 16 × 36 inches, conducts them down below the floor of the boiler-house, to enter the side of an underground brick flue extending along in the rear of the boilers to the chimney, located just outside of the boiler-house, as seen in Fig.

161. The covering over the boiler is as follows: suitable cast-iron bars of ⊥ section are laid about 3 feet apart, across the boiler, supported by the side wall of the boiler-setting on one side, and by the wall of the flue on the other side. On the flanges of these bars were placed, at intervals of a brick's length—8 inches—smaller bars, of similar section, on the flanges of which bricks were placed; and on the covering so made two courses of brick were laid in mortar. The space between this covering and the boiler is left vacant. Suitable openings are left for access to the man-hole cover, safety-valve seat, and feed-water inlet. The side and end walls, reduced to 12 inches in thickness, are carried up two or three courses higher than the covering over the boiler, in the form of a low parapet.

The boiler is supported at the front end by the arch front, at the rear end by a massive piece of fire-brick, and on the side wall by strong lugs, two on each side, riveted to the boiler.

Feed water is supplied to the boiler at the top, near the rear end, through a nozzle provided for the purpose, through a pipe carried down to and into the water, and around the flues nearly to the bottom, to enable the feed water to acquire nearly the temperature of the water in the boiler before its discharge from the pipe.

All the experiments with cold blast, and with natural chimney draft, were made with this Pacific boiler and boiler setting, as above described. Subsequently, the warm-blast apparatus was placed on top of this boiler; but without any alteration of the boiler-setting, except to discontinue the use of the horizontal flue on top and the vertical flue in the rear; and to make flues on each side for the warm blast, from the front end of the abstractors to the ash-pit. It will be seen from this description that this boiler has no superheating surface, unless the two or three square feet above the water level in the smoke-box be so considered, and as the gases here are but a few degrees warmer than the steam in the boiler, this is too trivial to produce a sensible effect.

The top of the fire-grates is 20 inches below the bottom of the boiler; the top of the bridge wall, 12 inches above the grates, and the pavement back of the bridge wall, 22 inches below the top of the bridge wall, and 30 inches below the boiler. The whole—furnace and combustion chamber—is lined with fire-brick, all headers in the furnace; and the rear wall is brought over by offsets, nearly to contact with the end of the boiler, above the flues, large tile, 18 inches long, 12 inches wide, and 3 inches thick, being freely

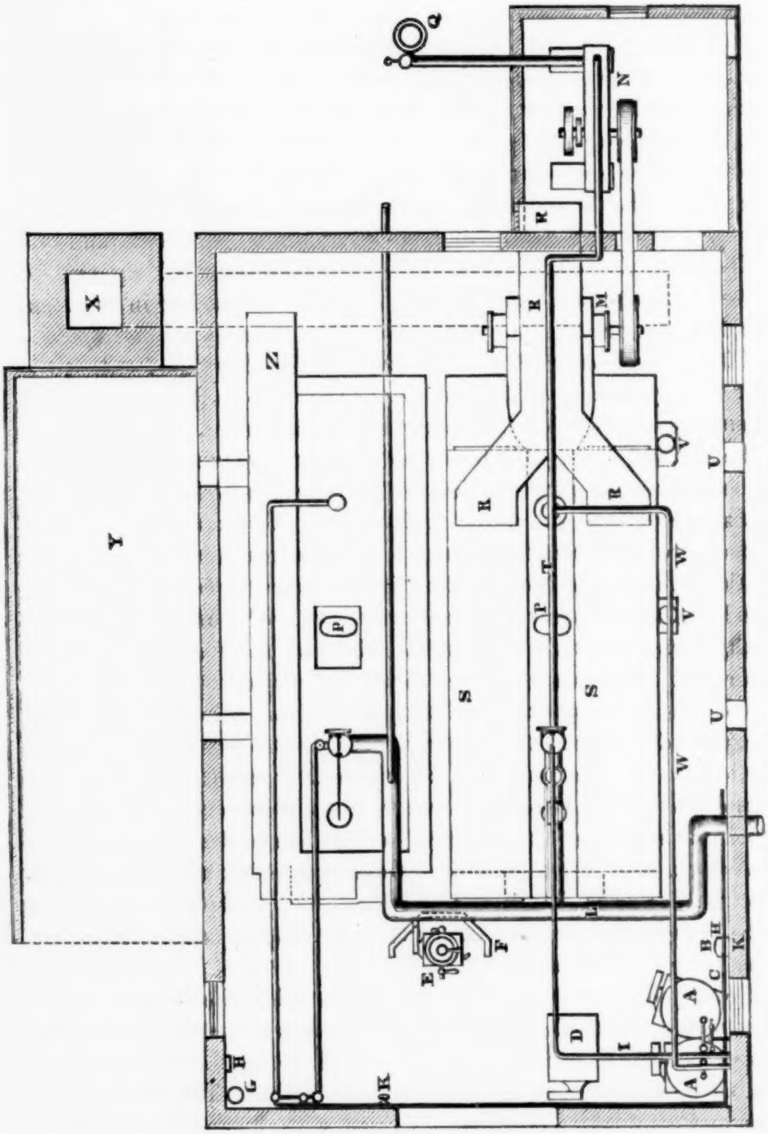


FIG. 161.—GROUND PLAN OF BOILER-HOUSE.

REFERENCES TO GROUND PLAN OF BOILER-HOUSE.

A, A, Water tank on platform scales.
B, Water meter.
C, Water pipes and valves for filling tanks.
D, Platform scales for weighing coal.
E, Calorimeter on its platform scale.
F, Screen to protect calorimeter from radiant heat.
G, Edson pressure recording gauge.
H, Test steam gauge.
I, Steam pipe to supply injector.
K, K, Pipe for direct water supply ; not used during these experiments.
L, Main steam pipe : wrapped with felt.
M, Root blower, for exhausting gases.
N, Steam engine to drive blower.
P, Man-hole of boiler.
Q, Condenser for ascertaining the quantity of heat rejected by the steam engine.
R, R, R, R, Cold-air boxes leading to abstractors.
S, S, Abstractors of boiler No. 1.
T, Steam pipe to supply steam engine N.
U, U, Small doors for inserting heat-carriers of pyrometer, at bridge wall and pier.
V, Shelf for pyrometer, at pier.
W, Water pipe from injector to boiler.
X, Chimney.
Y, Coal shed.
Z, Horizontal flue on top of boiler with cold blast : subsequently removed, when the second form of abstractor was applied to this boiler, converting it into warm-blast boiler No. 2.

used to give strength and stability to this overhanging wall; and for the same purpose the rear wall was made 3 feet 4 inches thick.

The boiler to be designated boiler No. 1, warm blast, was precisely similar to the "Pacific" boiler described above, but its setting was in some respects quite different.

The side walls are 33 inches thick, 9 inches being of fire-brick and 24 inches of red brick. This gives room for descending flues on each side, 8 inches in thickness, from the abstractors to the ash-pit, with 9 inches of fire-brick between them and the fire, and 16 inches of red brick outside. These walls are placed 5 feet 6 inches apart, and are plumb all the way up to 1 inch above the axis of the boiler, except a slight contraction of the space between them, of 1 inch on each side, at the fire grates, which are 5 feet 4 inches wide. From the top of these walls, a semicircular arch is sprung over the boiler, leaving a clear space between it and the smaller courses of the boiler of 3 inches at the sides, and 4 inches at the top, into which the hot gases could freely ascend, although no current could pass through. The temperature found in this space at the top—700° to 900° F.—gave at all times a slight degree of superheating, and care was taken to carry the water pretty high in the boiler, to avoid danger of injury to the plates.

At the close of the experiments with this boiler, before turning it over for regular use, large fire-brick tile—18 × 12 × 3 inches—were inserted, one by one, the whole length of the boiler on both sides, just below the arch, closing up the space between the side walls and the boiler, so that there was thereafter no superheating surface in this boiler. The reason for shutting off the superheating surface, when no longer required for experimental purposes, was to guard against overheating the plates.

The space between the rear end of this boiler and the rear wall is closed, or covered, above the flues, by a transverse arch of 5 feet span, 12 inches versed sine and 42 inches radius, resting on corbels brought out 3 inches on each side from the face of the side walls, at about the level of the axis of the boiler. This arch, composed, in fact, of a series of superimposed arches, one fire-brick (4.5 inches) in depth, was carried up even with the intrados of the arch over the boiler, which was continued on over it, to break the joint and to make the brick-work continuous; but was not built up close against the end of the boiler, a space of 0.75 inch being left for difference of expansion between the boiler and the brick-work.

One reason for arching over the boiler in the manner described,

was to obtain a secure foundation for the abstractors. These were placed on top, one at each side (Fig. 162), leaving a space of 3 feet in width between them, for access to the man-hole, safety valve and other attachments on top of the boiler. Side walls 8 inches thick, 32 inches apart, the face of each outside wall flush with the face of the side wall of the boiler setting, were covered over (after the tubes of the abstractor were put in), by supporting ⊥ bars 8 inches apart, and by 3 courses of brick resting on these bars. The flues for conveying the gases of combustion through the abstractors from

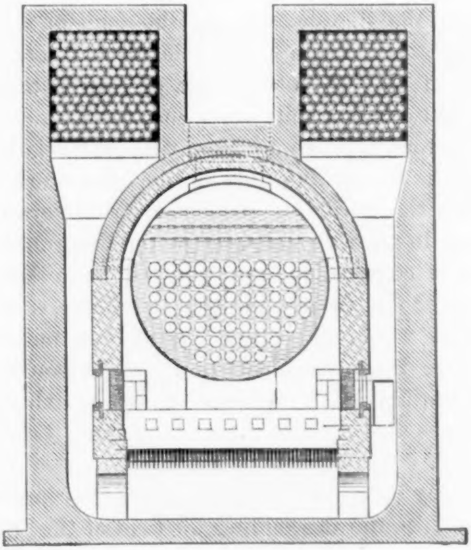


FIG. 162.

CROSS SECTION OF WARM-BLAST BOILER NO. 1, THROUGH FURNACE.

the smoke-bonnet to the blower at the rear of the boiler, are 240 in number—120 in each abstractor—of ordinary lap-welded tubes, 2 inches in diameter outside and 20 feet long, set by expanding their ends in cast-iron flue sheets provided with suitable flanges for fixing them securely in the brick-work. These 2-inch smoke-flues are set 3 inches apart, between centers, in 12 horizontal rows, 10 tubes in each row, in each abstractor, in equilateral triangles; and incased, each one in a 3-inch tube of thin iron, locked spirally, leaving between the smoke-flue and the incasing tube an annular space a little less than 0.5 inch in width radially, with pegs projecting from the inner flue to keep each tube and its casing in a

concentric position. The 3-inch tubes were bedded in mortar, and rested against each other at their lines of contact.

The 3-inch tubes were only 18 feet long—2 feet less than the 2-inch smoke-flues. As the air to pass through the 3-inch tubes, in the annular space between them and the 2-inch smoke-flues, was to be received at the rear end, at the top, and discharged at the front end at the bottom, the rear ends of the upper rows and the front ends of the lower rows of incasing tubes, were set 21 inches from the respective flue sheets; and the front ends of the upper rows, and the rear ends of the lower rows of these same tubes were set 3 inches from the flue sheets; and the ends of all intermediate rows at proportionate distances, in order to facilitate the admission and discharge of air. A cold-air box of thin plate iron, provided with an air-tight damper at its outer end, and branching equally to the two abstractors, supplies air from without the boiler-house at its rear end, and the descending flues in the brick-work already mentioned conduct the warm air from the abstractors to the ash-pit, through arches in the side walls, below the fire grates (Fig. 163).

The gases of combustion are conveyed from the smoke-box to the abstractors at the front end, by a branching smoke-bonnet of ample area (less would be better, causing less radiation), and at the rear they are drawn together again through converging flues to a single descending brick flue leading to the exhausting blower, which discharges directly into the under-ground brick flue leading to the chimney.

Tightly closing dampers are placed in the descending smoke-flue at the rear, and in the descending air-flues at the front, and regulating dampers, like throttle valves, are also placed in these latter. There is also a damper below the exhausting blower, and in line with the descending smoke-flue, which serves, when open, as a "by-pass" to permit the gases to flow to the chimney by natural draft when the blower is not in motion.

Small iron doors, 6 inches square, with isinglass panels, were set in the right-hand side wall, one opposite the bridge wall, the other opposite the pier at the rear end of the boiler, for the insertion and removal of the heat-carriers (platinum balls, in black-lead crucibles), of the platinum water pyrometers.

In addition to the necessary and proper arrangements heretofore described, there were certain contrivances, destitute alike of merit and novelty, introduced for reasons which it is not necessary to explain.

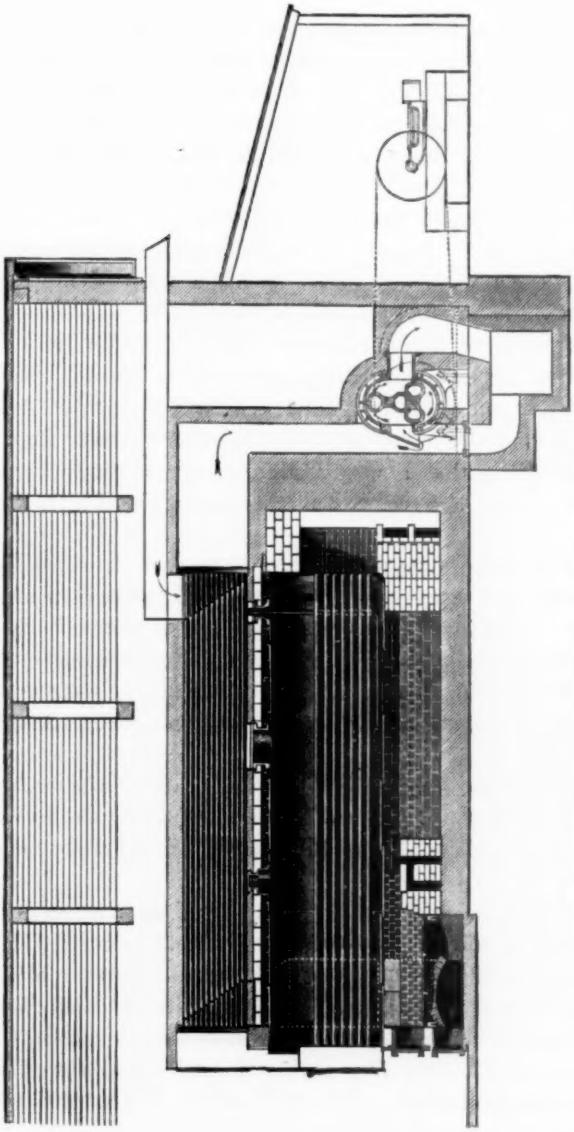


FIG. 163.—VERTICAL, LONGITUDINAL SECTION OF WARM-BLAST BOILER NO. 1.

One of these was a set of perforations in the side walls of the furnace, for the admission of warm air drawn from the descending air flues, *above* the incandescent fuel. Four tiles of fire clay, each 18 inches long, 12 inches wide, and 4 inches thick, each one pierced with 187 holes $\frac{3}{8}$-inch diameter in front and $\frac{5}{8}$-inch in rear, were set, two on each side, end to end, about in the middle of the length of the furnace, and exactly opposite the descending warm-air flues, with their lower edges 12 inches above the fire grates. Sliding "gridiron" dampers, with handles extending out through the arch front, were set behind these perforated tiles and a few inches from them; and great care was taken to make these dampers, when closed, as tight as possible, to reduce to a minimum the harm which the admission of air above the fuel could not fail to do. Careful and repeated experiments and observations proved that these dampers could never be opened without checking the draft through the fuel, and lowering the temperature of the fire; and it is not impossible that a very little leakage through the closed dampers may have lowered in a slight degree the efficiency of the boilers.

Another of these venerable contrivances, which is likely to be tried over and over again every few years till steam engines are no more, was a circuitous flue for heating air, or "superheating" it (whatever superheating may be supposed to mean when applied to a permanent gas), for admission to the combustion chamber through a channel and orifices in the bridge wall, technically known as a "split bridge."

This superheating flue was built entirely within the combustion chamber, back of the bridge wall, extending along the face of the side and rear walls, and was constructed in the following manner:

A wall of fire-brick, 3 inches thick, placed on their edges, three bricks high, was laid on the pavement back of the bridge wall, parallel to the side wall and to the rear end wall, at a distance of 4 inches from these walls. A course of headers 9 inches long was then laid, covering the flue, and bonded 2 inches into the walls, forming a flue 4 inches wide and 13.5 inches high, around the sides and rear of the combustion chamber. On top of this flue, another flue exactly similar was placed, both having their angles at the rear truncated a little, to diminish the resistance. The uppermost of these two flues was connected, just behind the bridge wall, with the vertical air flue on that side, through which warm air descended

from the abstractor to the ash-pit, admission of air to the superheating flue being regulated by a damper hinged at its lower edge, in such a manner that when open it extended obliquely into the vertical flue, so as to arrest a portion of the descending warm air, and to direct it into the superheating flue. At the opposite end of the bridge wall there was an opening through the horizontal partition between the upper and lower superheating flues, so that the air might descend to the lower flue and return to a point directly beneath the place where it entered the upper flue. Here was an opening into the split bridge; so that after twice making the circuit of the side and rear walls, the air, presumably considerably heated, might pass into the channel in the split bridge, and through small openings in its rear, to mingle with the hot gases flowing over the crest of the bridge wall. Subsequently, an additional wall of fire-brick was laid behind the bridge wall, to turn the superheated air upward.

No good was ever found to result from this system of flues; indeed, it is doubtful if any considerable quantity of air ever passed through the flues at all, although some must have flowed in when the dampers were opened, since the resistance of the open flue, circuitous as it was, could hardly have been so great as that of the coal on the fire grates.

But since the only combustible substance present in the smoke, carbon monoxide (CO), never, in the day-time, exceeded half of one per cent., and rarely exceeded half that small quantity when the dampers were open, for six weeks together, it was impossible that combustion could be sensibly promoted by the admission, at the rear of the bridge-wall, of a further supply of air, however much "superheated."

A certain very slight advantage resulted, indirectly, from the interposition of 3 inches more fire-brick, to check radiation where it was most active; but this device and that of perforated tile previously described are here given much in detail in order to show that their uselessness did not result from imperfect design, inadequate extent, or defective construction; but simply from the futility of attempting to burn over again the products of combustion already substantially complete, by the admission of air, however heated, where air, at the temperature of the hot gases themselves, is already in excess by 100 per cent., and most intimately intermixed.

It was assumed at the outset that cast-iron grates could not with-

stand the heat resulting from the introduction of warm blast, and a water grate was provided of a construction supposed to be safe and durable, although costly, consisting of a top and bottom plate, 3 inches apart, united at their edges by a hoop, and having a sufficient number of short tubes set through them for the admission of air. Provision was made for circulation, by a diaphragm in the middle of its width, connecting it with the water space below the flues, and dividing the furnace longitudinally into two equal parts. After repeated trials, this grate leaked so badly that it was discarded, and the ordinary long grates of the Pacific Mills were tried. These were cast two bars together, 5 feet 2 inches long, 0.75 inch thick on top, 0.50 inch at 0.62 below the top, and 0.31 inch at the lower edge; and 5.5 inches deep in the middle. The spaces between grate bars were 0.5 inch, so that the openings, without deduction for obstructions at the ends and at the two intermediate side supports, were equal to 40 per cent. of the whole grate area; and allowance made for all obstructions, the clear space was equal to 34 per cent. These grates, supported at their ends only, were not destroyed by three weeks' use with warm blast; but they gave evidence of injury in places, which made it plain that they might suddenly melt down at any time, and other grates, admitting of more support from below, were tried with entire success. These were the Williams rocking grates, supported on bearers in sections only about 15 inches long, and provided with a means of clearing them by shaking from below, through the ash-pit door. In firing with stationary grates, it was found necessary to keep the fire doors —one at least—open one hour out of ten hours' firing, to clean the fire and draw the clinker. This invariably lowered the temperature all the way from furnace to smoke-box, diminished the draft through the coal on the grates, produced an increased quantity of carbon monoxide (CO), in the chimney gases, and reduced the efficiency of the boiler. With the rocking grates, the time of slicing and cleaning the fire was reduced to ten minutes once a day, at 5.30 P. M. The short, sectional grate bars next to the bridge wall suffered pretty rapid deterioration, and in a less degree those at the front and sides. Some form of grate still better may be found; but it seems probable that the life of any grates, whatever their form, will be less with warm blast than with air taken in at the external temperature—unless, indeed, a water grate can be used. What the excess of cost for grates may be, to offset the gain by warm blast, can only be determined by experience of some duration.

After the conclusion of the experiments with Warm-Blast Boiler No. 1 (weeks G & H of Record, ending February 4 and 11, 1882), the original Pacific Boiler was converted into Warm-Blast Boiler No. 2, by the simple addition of the abstractors and the air passages and smoke flues necessary to convey the gases through the abstractors to the exhaust blower, and the air in the opposite direction from without the boiler-house to the ash-pit.

No alteration was made in the boiler; and none in the brick-work, otherwise than to build the two vertical flues near the front end, to conduct the warm blast down to the ash-pit, and into it by arches in the side walls below the fire grates.

These vertical flues occupied, in part, a space of 8 inches originally left between the side walls, and, on the left the wall of the boiler-house; on the right the side wall of Warm-Blast Boiler No. 1.

The mode of construction first adopted in Warm-Blast Boiler No. 1, has been fully described.

The principles involved in that construction were:

1. The division of the air passages into 240 annular channels of uniform cross-section between (*a*) lap-welded smoke flues 2 inches in diameter outside, and (*b*) spiral-locked sheet-iron tubes 3 inches in diameter outside, in order to give uniform velocity to the air, and equal exposure of the air to the warm surfaces.

2. The great addition to the warm surface made by these external tubes, which would be warmed by radiation from the 2-inch flues to almost uniform temperature with them, since air is not sensibly affected by radiant heat. There would therefore be two metallic surfaces in contact with the thin annular stream of air, the inner one 6 inches, the outer 9 inches in circumference, amounting to 300 square feet for each foot in length, and for the whole 20 feet to 6,000 square feet.

3. The skin friction of the 3-inch tube being greater than that of the 2-inch flue, not alone on account of its 50 per cent. greater area, but also on account of the slightly projecting spiral line of joint within, corresponding with the prominent spiral interlocked ridge on the outside, the air must acquire a rolling motion from within outward best calculated to bring all parts of the inflowing air into frequent contact with the warm surfaces, and to uniform temperature with those surfaces.

Much apprehension was felt that the smoke might pass in largest volume through the lower courses of 2-inch flues—those first en-

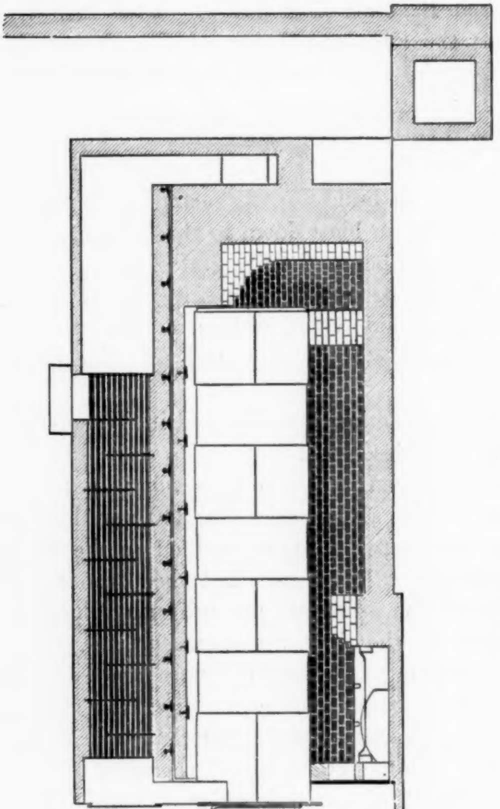


FIG. 164.—VERTICAL, LONGITUDINAL SECTION OF WARM-BLAST BOILER SETTING NO. 2, SHOWING THE DEFLECTORS OF THE ABSTRACTORS.

countered on leaving the smoke-box; and that the air, on the other hand, entering at the top, might flow in greatest volume through the upper annular passages, and that so the effect might be diminished. But extensive and patient probing with the water-platinum pyrometer—the platinum ball being held in the tongs to be described—proved that this apprehension was unfounded—that both air and smoke were at nearly uniform temperature at top and bottom of every cross section.

Yet the quantity of heat transferred from the escaping gases to the inflowing air did not answer expectations based on theoretical

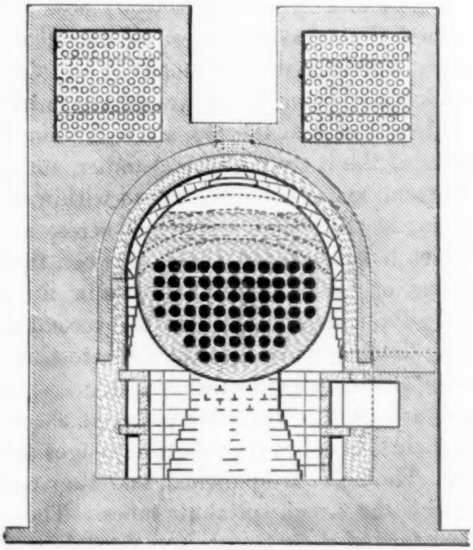


FIG. 165.

CROSS-SECTION OF WARM-BLAST BOILER NO. 1, AT PIER: BUT SHOWING THE ABSTRACTORS OF NO. 2.

considerations. The gases passed off at a temperature about 160° F. above that of the external air, carrying a large quantity of heat to waste. The obstruction sure to arise in time from accumulations of dust in the annular passages, was not lost sight of. The cost, too, of the double pipes, was very considerable. It was therefore decided, after due consideration, to adopt a new mode of construction.

A piece of 2-inch spiral-locked tube, 4 feet long, with brass ferrules soldered into its ends, having been put to severe tests and found air-tight, pipes of that description were adopted for the

smoke flues, at a saving of nearly one-half; and the 3-inch tubes were omitted, and replaced by deflectors, arranged as shown in Figs. 164 and 165. The 2-inch flues were made 18 feet long, of sheet steel, No. 26 American wire gauge (.018 inch thick), each tube formed of a single strip 35 feet 6 inches long and 3.5 inches wide. A ring or ferrule of copper, equal in thickness to the ridge formed by the locked joint (about .054 inch), with its ends cut to the obliquity of the spiral joint, so as to fit closely to it on both sides, made an outer surface at each end smooth and cylindrical, for expansion into suitable holes in the flue sheets; and internal thimbles of lap-welded pipe gave the degree of firmness necessary to hold the expansion.* Partitions of sheet iron, having holes for the 2-inch spiral flues corresponding in position with the holes in the flue sheets, were put into the brick chambers of the abstractors at intervals of about a foot; half of them closed at the top, and extending down to within 3 rows of flues of the bottom of the chamber, and the other half closed at the bottom, and extending up to within 3 rows of the top. Air entering at the top must descend across and among the 2-inch flues, which have spaces of 1 inch between them, pass under the first partitions, or "deflectors," then rise in the same manner across and among the flues to pass over the second deflectors, and so on, until on flowing over the last deflectors, it passes down through the vertical brick flues to the ash-pit.

The deflectors are plain rectangular pieces of sheet iron, No. 18 w. g., set with their side and top (or bottom) edges about 1 inch in the brick-work. The holes are punched, and they cost but a trifle in comparison with the 3-inch spiral air tubes. They also support the flues at every foot of their length, and they allow dust to collect to any probable extent in the corners at the bottom of the brick chambers without causing inconvenience. The deflectors, so far as they go, supply an additional surface warmed by conduction and radiation to impart heat to the air by contact; and the impact of the air in flowing transversely across the flues, although acting in each direction only on about one-half the circumference of each flue, may yet be counted on to give something of that increased effect due to impact of air upon warm surfaces which was first pointed out by Leslie.

For protection against loss by radiation, brick walls and brick covering were used as before; but in order to guard against cracks,

* A better method has since been devised by Mr. F. H. Prentiss, by means of external and internal malleable iron rings.

and to cut off leakage, the whole exterior of the brick-work of these abstractors was incased in thin galvanized sheet iron, locked and soldered.

The covering of the Pacific boiler, described previously, not being strong enough to bear the weight of the abstractors, bars of old rails, equal in length to the width of the boiler setting of warm-blast No. 1 (11 feet), were placed across at intervals of 2 feet, resting on the side walls, and projecting a few inches beyond them. Pieces of $\frac{1}{4}$-inch plate iron laid from bar to bar on their lower flanges supported the brick-work. After leveling up to the top of the bars, the sheet iron for the bottom of the casing was laid on, a hearth of three courses of brick was laid, the side walls were carried up, and, after the tubes and deflectors were all in proper place, the covering was put on, and the casing of sheet iron completed. A brick flue brought together the two streams of gases from the two abstractors, and conducted the united stream to the exhaust blower.

The results obtained with this apparatus (week T, ending May 20, 1882), were decidedly better than were obtained with the apparatus first tried. With higher temperature of external air, and a smaller quantity of air per pound of coal, the final temperature of escaping gases was reduced 20 degrees lower. Part of this gain may have been due to the manner in which the abstractors are supported, on bars, out of contact with the brick-work (which in the other case was very hot on account of the superheating arrangement), so that less heat was imparted by conduction to the abstractors, to be in part carried off by the smoke. But a part—probably the greater part—was due to the greater efficiency of surface impinged upon by air in motion, over similar surfaces along which air flows smoothly, without impact. Something may be due to the reduced thickness of the metal, but Péclet's formulæ do not lead us to expect a sensible effect from this cause.

The question of durability remains to be settled, but there is now reason, after nearly three years' use, to look for a favorable result. Both the air and the smoke are at a temperature so far above their dew-point—unless, indeed, the boiler leaks badly—that no moisture can be deposited on the flues, either within or without; and there is little danger to be apprehended from sulphur, which in the form of sulphurous acid (SO_2), is not actively corrosive, and of SO_3 (which condensed with water becomes H_2SO_4, or sulphuric acid), there is never much and seldom any. The anthracites contain but very little sulphur, and the Cumberland

bituminous coals only about 0.8 per cent. Pictou coal, it is true, sometimes contains sulphur, in the form of iron pyrites, in visibly large quantities.

The cost of the single tubes with deflectors is much less than that of the other form, with double tubes :—*First*, because the spiral steel tubes cost but little more than half as much as the lap-welded tubes of the same size; *second*, because they were reduced from 20 feet in length to 18 feet, yet seemed to be even more efficient; and *third*, because the deflectors cost much less than the 3-inch tubes which they replaced. As to the sheet-iron casing outside of the brick chamber, that was no less desirable with the first form than with the second.

It is probable that the quantity of air per pound of coal consumed was reduced by this air-tight casing, since much air infiltrates through brick-work. The extent of this infiltration is surprising. So great is it that the flame of a match is drawn to and into the interstices of an 8-inch brick wall—not alone at fine visible cracks, but at mortar joints apparently sound.

To cut off this harmful infiltration of air, the outside of the smoke-flues in the rear was coated with desiccated tar and shingled over with tarred cotton cloth. It might be worth while to leave off the outer 8 inches of brick-work of the boiler setting, all around, until the inner portion was complete, and then to cover the whole surface, sides, ends and top (and preferably the bottom also, to keep down moisture), with galvanized sheet iron, locked and soldered. At the arch front, where very pernicious leakage of air is too common, a tight joint could be made by means of a strip of sheet iron riveted to the back side of the arch front all around, to which the casing could be locked and soldered. If, now, the casing were covered with an inch of hair felt, and around and over all 8 inches of brick-work were laid, secured with binders, as usual, or more completely, an appreciable saving of heat would result, perhaps exceeding one per cent. An air space is sometimes left in the brick-work, for the purpose of reducing radiation. Breaking the continuity of the brick-work certainly impedes the outflow of heat, by interrupting conduction, and interposing the slower processes of radiation and absorption; but an air space as an interceptor of radiant heat is futile. Hardly any substance in nature is less useful for this purpose than air, which when dry is absolutely diathermanous. It answers well to build the walls of three successive, independent 8-inch walls, close together, but

not bonded, and free from mortar at their surfaces of contact. Conduction is thereby sensibly interrupted, some freedom is left for unequal expansion, the binders tie all firmly together, and cracks will be less numerous, less continuous, and less disastrous. But all cracks, large and small, should be sedulously stopped up. Very large cracks will often be found between the arch front and the brick-work. These should be stopped with putty. Smoke-box covers and doors, fire doors and ash-pit doors should be carefully fitted, and smoke-box doors and covers, which usually require to be opened but once a week, should be packed or puttied. Fire doors should be made with a groove all around them, to receive a packing of asbestos, and should have some provision for pressing them firmly against their seats on the arch front. However well fitted at first, or when cold, the heat warps them so that they often admit sufficient air to impair the draft where alone draft is useful—through the fuel on the fire grates. For the same reason, the fire door should be left open as little as possible. If the grates are stationary, it will be necessary, with combustion as rapid as 12 pounds of coal per square foot of fire-grate area per hour (counting all the time the draft is open), to clean the fire and draw out clinker as much as six times in ten hours, occupying ten minutes each time, equal to one hour in ten—a serious loss, which may be reduced five-sixths by the use of rocking grates, operated through the ash-pit door. But let no grate-vender quote me as authority for the stereotyped saving of "30 per cent. of 'the coal.'" I believe that an appreciable saving may be made by the use of good rocking grates, perhaps two per cent. In the aggregate, these small savings become important; but aside from the one conspicuous saving by returning to the furnace, in a warm blast, a part of the heat of the gases of combustion after they leave the smoke-box, in some such manner as that herein described, or by its substantial equivalent the Green Economizer, no gain of so much as five per cent. over reasonably good ordinary practice can be so much as fairly hoped for.

II.

Taking up now a second division of the subject, a description will be given of the instruments and apparatus for physical observation.

1. The Pyrometer.—The inner cell, or true containing vessel of this instrument, is 4.25 inches in diameter, and of equal height on the side, with a bottom in the form of the segment of a spherical surface of 4.25 inches radius, all of sheet brass 0.01 inch thick, nickel plated and polished outside and inside (Fig. 166). The outside case is 8 inches in diameter and 8.5 inches deep, of 16 oz.

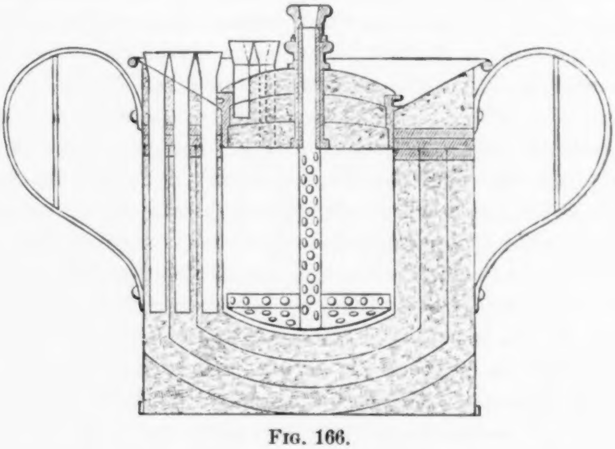


Fig. 166.

WATER-PLATINUM PYROMETER.

copper, nickel plated and polished on the inside but plain outside. There are two handles—on opposite sides—for convenience of rapid manipulation. The top, which is depressed conically like a hopper, is of the same copper as the sides and bottom, and is terminated at its outer edge with a strong wire, forming a lip all around for convenience of pouring. The central cell is connected with the outer case, only by three rings of hard rubber, each 0.25 inch thick; the middle ring completely insulating the cell from its continuation upward to the hopper-shaped top, by interposing its thickness between the flanges of these two parts. The joints formed by contact of these flanges with the hard rubber ring, which

would be likely to leak water into the spaces filled with eider down, were made tight by a coating of asphaltum varnish.

The lower hard rubber ring is, in fact, made up of three concentric rings, each one supporting the flange of a partition. These partitions are complete cups—sides and bottom—of sheet brass 0.01 inch thick, and, together with an additional spherical segment at the bottom, next to the outer case, are nickel plated and polished outside and inside. They divide the space between cell and case into three compartments, each about 0.625 inch in thickness, all filled as are all spaces everywhere, with eider down. All the four plates of the cover are of sheet brass, 0.01 inch thick, nickel plated and polished on both sides; and all are insulated from each other and from the vessel by a hard rubber ring, which forms the outer rim of the cover, and by a tube of hard rubber with a knob and shoulder above and a screw-thread at its lower end, by which the upper and lower plates are firmly held together, while the knob serves for lifting the cover.

Through this tube passes the hollow stem of the agitator, the upper part of it formed of hard rubber, terminated at top by a knob, with a taper hole for a cork, through which a thermometer passes down nearly to the bottom of the stem, the lower part of which is of brass tube. The agitator conforms to the spherical shape of the bottom of the cell, but does not touch it by about 0.25 inch; and has a rim turned up 0.25 inch all around. This agitator and the brass portion of its stem are freely perforated with holes 0.2 to 0.4 inch diameter, and nickel plated and polished. The spherical form of the agitator gives a radial direction to the streams of water passing through its holes when it is raised and lowered, so that very little motion up and down—not enough to lift the hard rubber stem out of its hard rubber incasing tube—suffices to mix the water perfectly.

A slight modification, to the form of a propeller, would enable it to give equally good mixing by rotatory motion, as strongly recommended by Berthelot, and avoid alternate withdrawal and re-immersion of any part of the stem.

Very careful and quite satisfactory determinations were made of the calorific value of the metals directly affected by the temperature of the contained water; and just sufficient water was weighed in, to amount, with the calorimetric value of the instrument, to two pounds of cold water.

This determination was in its general nature similar to the de-

termination of the heat capacity of the calorimeter for testing the quality of steam, fully described elsewhere; but as this instrument was designed to hold only about a quart of water, the method followed was much simpler, and susceptible of greater accuracy.

A tin dipper of about two quarts capacity was used to hold the hot water, which was poured directly into the pyrometer as quickly as possible, whereupon the cover was shut down and the agitator was put in motion. The examples given in the case of the steam-calorimeter will serve as a type of the experiments with the pyrometer; but in this latter case a special correction was demanded. The cooling of the hot water was augmented by pouring, in consequence of the exposure to the air of a large surface of water in a thin sheet. The effect of this exposure was ascertained in the following manner: The instrument was placed in a bath of tepid water, so as to bring the temperature of all the materials composing it exactly to the temperature of the water to be poured in. Thus, whatever diminution of temperature the latter might suffer, must be entirely due to the loss of heat by pouring. Four experiments, carefully made, gave the following results: Loss of temperature by pouring, at 170° F., 0.81°, 0.86°, 1.00°, and 1.07°; mean, 0.935° F.

The following six values of the calorific capacity of the metals of the pyrometer, which share directly the temperatures of the inclosed water, including also the thermometer used with the instrument, were found by experiment: 0.1048, 0.1052, 0.1077, 0.1008, 0.1028, 0.1104.

Mean,	0.1053,	=	0 lbs.	1 oz.	11 drms.
Add water,	1.8947,	=	1	14	5
	2.0000,	=	2	0	0

This mean was the value used. The instrument being put on delicate coin scales and counterbalanced, weights equal to 1.8947 pounds avoirdupois, = 1 lb. 14 oz. 5 drms., were added to the counterbalancing weights, and cold water was poured in until the scales again balanced.

The vessel and its contents were then just equal in heat capacity, while the temperature of the water was not above 38° F., to 2 pounds of cold water.

The heat-carriers were platinum balls, of three sizes:

1 of 4,200 grains	=	0.6 lb.	avoirdupois.
1 of 2,800 grains	=	0.4 lb.	"
1 of 1,400 grains	=	0.2 lb.	"

Two vessels exactly similar were provided, and when duplicate observations were made for mutual verification, the two smaller balls were placed in one crucible, and the larger one, equal in weight to the two smaller ones, in the other. As the two instruments were sensibly alike, simultaneous observations with similar exposure should give, as they usually did give, temperatures equal within the limit of error to be expected—less than 10° F.—and occasionally identical temperatures.

Sometimes one of the smaller balls was used alone, to avoid raising the water to a final temperature above the range of a delicate thermometer embracing only a few degrees, of half to five-eighths inch to 1 degree, graduated to 0.1 degree.

The scale of the pyrometer, for the first approximation, was for the larger ball (and for the two smaller balls together), 100° to 1°; for the middle sized ball, 150° to 1°; and for the smaller, 300° to 1°. In order to ascertain what correction, if any, should be made for cooling during the process of withdrawing the platinum balls from the fire and immersing them in the water of the pyrometer, several experiments were made upon the effect of cooling from 15 to 35 minutes. The fire-brick, charged with its two crucibles without the covering brick, but with the covers of the crucibles in place (Figs. 167 and 168), was withdrawn from the fire in the usual manner, and the ball from one of the crucibles was put as quickly as possible into one of the pyrometers, and the notes were taken. The other crucible was then permitted to stand in the fire-brick, with its cover on, but exposed to the air of the room, usually 15 minutes, sometimes 25 minutes, and in one case 35 minutes, when the balls from the crucible in question were put into the other pyrometer, and the notes were taken as before.

When the two crucibles were emptied of their balls into the two pyrometers as quickly as possible, there were often discrepancies of 10°, 25°, or 50°, although accordance within 10° or less was frequent. These discrepancies resulted partly from errors of observation, and partly, no doubt, from real difference of temperature; and the apparent differences resulting from difference in the time of exposure to the air were therefore mixed up with errors of observation, and with possible differences of original temperature. The mean cooling effect was 0.7° F. per minute, equal to 70° F. in the resulting temperature; and the range was from 1.2° to 0.2°, say 120° to 20° in the result. At all events it was small, and although most active at first, while the heat was greatest, the loss

was too small to require notice when the balls were immersed in the water of the pyrometer in 3 to 5 seconds from the time of opening the door to withdraw the fire-brick with its crucibles; as was usually the case when there was no accidental detention.

THE USE OF THE PYROMETER.

In using this instrument we have, in order to obtain the first approximation, to make two assumptions: 1st, That the specific heat of the water at the temperatures employed, will be constant, and equal to 1.00000; 2d, That the specific heat of the platinum balls employed will be constant, and equal to $0.0\dot{3}33\dot{3}$, that is, to $\frac{1}{30}$ that of the water.

Since the largest platinum ball weighs three-tenths (0.3), as much as the water (including in all cases the heat value of the instrument), it follows from the above assumptions that the heat capacity of the platinum ball will be one one-hundredth (0.01), of that of the water, including the inclosing vessel. Every degree, then, added to the temperature of the water indicates roughly 100° lost by the platinum ball. The error resulting from the inaccuracy of the first assumption is small, and may sometimes be neglected; but with high temperature, where the range of temperature in the water is considerable, and especially when the initial temperature of the water is as high as 40° F., it is better to eliminate the error by the use of the following table of temperatures and corresponding British thermal units. For instance, if the initial temperature be 61°, and the final, 83°, the number of British thermal units added to the water will be:

$$83.041 - 61.010 = 22.031;$$

and the loss of heat by the platinum ball, on the second assumption will be:

$$22.031^\circ \times 100^\circ = 2203.1^\circ \text{ F.}$$

TABLE NO. III.

Temperatures Fahrenheit, and corresponding number of British thermal units contained in water, from zero Fahrenheit.

Deg.	B. t. u.	Deg.	B. t. u.	Deg.	B. t. u.	Deg.	B. t. u.
32	32.000	57	57.007	82	82.039	107	107.101
33	33.000	58	58.007	83	83.041	108	108.104
34	34.000	59	59.008	84	84.043	109	109.107
35	35.000	60	60.009	85	85.045	110	110.110
36	36.000	61	61.010	86	86.047	111	111.113
37	37.000	62	62.011	87	87.049	112	112.117
38	38.000	63	63.012	88	88.051	113	113.121
39	39.001	64	64.013	89	89.053	114	114.125
40	40.001	65	65.014	90	90.055	115	115.129
41	41.001	66	66.015	91	91.057	116	116.133
42	42.001	67	67.016	92	92.059	117	117 137
43	43.001	68	68.018	93	93.061	118	118.141
44	44.002	69	69.019	94	94.063	119	119.145
45	45.002	70	70.020	95	95.065	120	120.149
46	46.002	71	71.021	96	96.068	121	121.153
47	47.002	72	72.023	97	97.071	122	122.157
48	48.003	73	73.024	98	98.074	123	123.161
49	49.003	74	74.026	99	99.077	124	124.165
50	50.003	75	75.027	100	100.080	125	125.169
51	51.004	76	76.029	101	101.083	126	126.173
52	52.004	77	77.030	102	102.086	127	127.177
53	53.005	78	78.032	103	103.089	128	128.182
54	54.005	79	79.034	104	104.092	129	129.187
55	55.006	80	80.036	105	105.095	130	130.192
56	56.006	81	81.037	106	106.098	131	131 197

The error arising from the inaccuracy of the second assumption is much more important, but is easily eliminated—at least approximately—by the use of Table IV., which is carried out for every 100° F., with certain intermediate points, for reference :—32° and 212°, for verification of the pyrometer by these standard temperatures—melting ice and boiling water.

At 446.2° F. the assumption of $0.033\dot{3}\dot{3}$ for the specific heat of platinum is correct. At lower temperatures the correction is *minus:* at all higher temperatures it is *plus.* The use of the table is obvious. Having found the approximate observed loss of temperature, corrected for variations in the specific heat of water, look for the nearest corresponding loss in column 6, "observed loss of temperature" etc., and if not found exactly, find the intermediate point by the aid of the proper difference in column 7. Opposite, in column 1, will be found the true loss of temperature by the heat-carrier, corresponding to the observed loss. For instance, having found an

observed loss of temperature, corrected for variation of specific heat of water, = 2203.1°, we find in column 6, 2152.1 which subtracted from 2203.1, leaves 51.0; and the tabular difference for 100° being 131.7 = 1.317 for 1°, 51 divided by 1.317, gives 38.7. Turning now to column 1, we find opposite 2152.1, 1900; and adding 35.1 we have 1938.1 as the (approximately) true loss of heat by the carrier in cooling from initial temperature to 83° F., and 1938 + 83 = 2021° F. as the initial temperature of the heat-carrier on its immersion in the water of the pyrometer.

The manner of manipulating the platinum balls as heat carriers, is plainly indicated in Fig. 167. In most cases the covering brick may be omitted; but it should be used whenever, on account of obstacles in the way of rapid manipulation, more than four or five seconds are required to remove the crucibles from the fire and to immerse the balls in the water.

For observations in the heart of the fire, the crucibles may be used without the brick. No bits of coal must be permitted to enter the crucibles; and this accident may be guarded against in some measure by binding on the covers with copper wire wound many times around. The wire will be speedily melted, but will endure long enough fairly to insert the crucibles and cover them with the glowing coal; and, with care, they may be taken out without disturbing their covers. Thermometers should be delicate, not less than 0.3 inch to 1°, and graduated to tenths of a degree.

They may then be read to hundredths, and temperatures may be determined within a very few degrees.

It will be apparent, on reflection, that these tables can give only approximate results, and could be exact only upon the impracticable condition that the final temperature of the water and heat carrier, after the immersion and cooling of the latter, should be, in every case, 32° F.

But since the initial temperature must always be above this point, and the final temperature several degrees higher still, while the tables are based on the mean specific heat of platinum, or, with the compound balls—platinum and iron—between 32° and the respective higher temperatures included in the table, an error will result from the use of the tables, varying in magnitude with the number of degrees between 32° and the final temperature. To be exact, the tables should be expanded so as to embrace specific heats between 32° and, say, 100° F., varying by single degrees, or by small intervals—3° or 5°—at the lower limit; and 100°, 200°, 300°,

etc., as in these tables, for the upper limit. Such tables would be cumbrous and inconvenient, and by no means worth while. The approximation given by the tables is pretty close, and may usually be made satisfactory. In most cases a rather closer approximation may be made by adding the number of degrees of final temperature of the water to the observed loss of heat by the heat-carrier, *before* correction by the table, as already described. Thus, 2203.1 + 83.0 = 2286.1, corresponding, in Table IV., to 2001.7°.

A still closer approximation may be made by subtracting 32° from the number of degrees of final temperature, and reducing the difference to *pyrometer degrees*, by multiplying it by the tabular difference, 0° to 100°, column 7, Table IV., which is 96.9 for 100°, = .969 per degree. The pyrometer degrees so found are to be added to the observed loss of heat by the heat-carrier, and the corresponding true loss is to be taken out of the table; and 32 added to this will give a close approximation to the true temperature of the hot ball.

Thus, 83 − 32 = 51, and 51 × .969 = 49.4, and 2203.1 + 49.4 = 2252.5.

The next smaller tabular number in column 6 is 2152.1, and 2252.5 − 2152.1 = 100.4, and 100.4 ÷ 1.317 (131.7 ÷ 100 = 1.317) = 76.23. The number in column 1, opposite 2152.1 is 1900, and 1900 + 76.23 + 32 = 2008.2° = the true temperature sought—to a close approximation. The respective values found by the three methods are 2021°, 2002°, and 2008°, showing an extreme range of variation at this high temperature, of less than 1 per cent.

Either method will usually be accurate enough. The first and second are equally easy of application, the third but little more laborious. Should more exact results be desired, the formula for specific heat may be used.*

* For a discussion of the specific heat of platinum and iron, at various temperatures, or, more properly, the mean specific heat of these metals from 32° F. to higher temperatures, see "Journal of the Franklin Institute," Vol. LXXXIV., third series, July–December, 1882, pp. 91, 169, and 252. Also, "Transactions of the American Society of Mechanical Engineers," Vol II., p. 42; and Vol. III., p. 174

TABLE IV.

Temperatures in deg. Fahr. corresponding with specific heats in column 2.	Mean sp. ht. of Platinum from 32° computed for each 100 deg. Fahrenheit.	Differences of sp. ht. for each 100° F.	Ratio of computed to assumed sp. ht.: viz. 1-30 water = 0.033333.	Differences of Ratios for each 100° F.	Observed loss of temperature by heat-carrier in cooling: at assumed ratio H_2O 30 to Pt 1.	Differences of observed loss per 100° Fahr.
1	2	3	4	5	6	7
0	.031983		.95950		0.0	
32	.032080		.96240		30.8	
		303		907		96.9
100	.032286		.96857		96.9	
		302		907		98.6
200	.032588		.97764		195.5	
212	.032624		.97873		207.5	
		303		908		100.5
300	.032891		.98672		296.0	
		302		908		102.3
400	.033193		.99580		398.3	
446.195	.033333		1.00000		446.2	
		303		909		104.1
500	.033496		1.00489		502.4	
		304		910		106.0
600	.033800		1.01399		608.4	
		303		910		107.8
700	.034103		1.02309		716.2	
		303		910		109.6
800	.034406		1.03219		825.8	
		304		911		111.4
900	.034710		1.04130		937.2	
		304		912		113.2
1000	.035014		1.05042		1050.4	
		304		912		115.1
1100	.035318		1.05954		1165.5	
		304		913		116.9
1200	.035622		1.06867		1282.4	
		305		913		118.7
1300	.035927		1.07780		1401.1	
		304		914		120.6
1400	.036231		1.08694		1521.7	
		305		914		122.4
1500	.036536		1.09608		1644.1	
		305		915		124.3
1600	.036841		1.10523		1768.4	
		305		915		126.1
1700	.037146		1.11438		1894.5	
		305		916		127.9
1800	.037451		1.12354		2022.4	
		306		917		129.7
1900	.037757		1.13271		2152.1	
		306		917		131.7
2000	.038063		1.14188		2283.8	
		305		917		133.4
2100	.038368		1.15105		2417.2	

TABLE IV.—*Continued.*

Temperatures in deg. Fahr. corresponding with specific heats in column 2.	Mean sp. ht. of Platinum from 32° computed for each 100 deg. Fahrenheit.	Differences of sp. ht. for each 100° F.	Ratio of computed to assumed sp. ht.: viz. 1-30 water = 0.03333.	Differences of Ratios for each 100° F.	Observed loss of temperature by heat-carrier in cooling: at assumed ratio H_2O 30 to Pt 1.	Differences of observed loss per 100° Fahr.
1	2	3	4	5	6	7
		306		918		135.3
2200	.038674		1.16023		2552.5	
		307		919		137.2
2300	.038981		1.16942		2689.7	
		306		919		139.0
2400	.039287		1.17861		2828.7	
		307		920		140.8
2500	.039594		1.18781		2969.5	
		306		920		142.7
2600	.039900		1.19701		3112.2	
		307		921		144.6
2700	.040207		1.20622		3256.8	
		307		921		146.4
2800	.040514		1.21543		3403.2	
		308		922		148.3
2900	.040822		1.22465		3551.5	
		307		923		150.1
3000	.041129		1.23388		3701.6	
		308		923		152.0
3100	.041437		1.24311		3853.6	
		308		923		153.9
3200	.041745		1.25234		4007.5	
		308		924		155.7
3300	.042053		1.26158		4163.2	
		308		925		157.6
3400	.042361		1.27083		4320.8	
		308		925		159.5
3500	.042669		1.28008		4480.3	
		309		926		161.3
3600	.042978		1.28934		4641.6	
		309		926		163.2
3700	.043287		1.29860		4804.8	
		309		927		165.1
3800	.043596		1.30787		4969.9	
		309		927		166.9
3900	.043905		1.31714		5136.8	
		309		928		168.8
4000	.044214		1.32642		5305.6	

Table V., which follows, constructed in the same manner as Table IV., but for iron instead of platinum, as a heat-carrier, requires no special explanation, as the use of the two tables is altogether similar. Iron can be used at all only at moderate temperatures, and the results obtained by its use must be crude, on account of its rapid change of weight by oxidation. Table VI. contains

three columns of corrections: first, for platinum, corresponding to column 6 of Table IV.; second, for iron, corresponding to column 8 of Table V.; and third, for a compound ball, composed of 700 grains of fine wrought iron encased in 700 grains of platinum, formed into a solid capsule, about 0.048 inch thick; the whole weighing 1,400 grains, with a heat capacity (at the assumptions for specific heat, for Pt, $0.03\dot{3}\dot{3}$, for Fe, $0.1666\dot{6}\dot{6}$), equal to that of 4,200 grains = 0.6 lb. of platinum. The assumed specific heat of Fe being five times that of Pt ($0.0\dot{3}\dot{3} \times 5 = 0.1\dot{6}\dot{6}$), the 700 grains

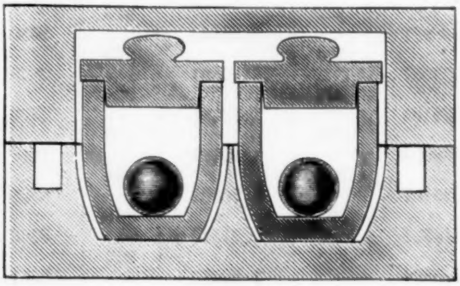


Fig. 167.

PLATINUM BALLS, CRUCIBLES AND FIRE-BRICK BED AND COVER, AS ARRANGED FOR USE WITH THE WATER-PLATINUM PYROMETER.

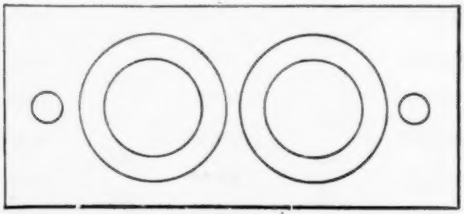


Fig. 168.

TOP VIEW OF LOWER FIRE-BRICK.

of Fe are equal to 3,500 grains of Pt, and 700 grains added for the Pt cover, the total is 4,200.

I had two of these compound balls, and often used them at moderate temperatures, 1,000° to 1,200° F., in direct comparison with solid platinum balls, without detecting any important discrepancies in the results. The advantage of the platinum cover is that the iron is protected from oxidation. The advantage of the iron is that there is great saving of cost.

TABLE V.

FOR IRON HEAT-CARRIER: ASSUMED SP. HT. $= 0.1\dot{6}\dot{6}$.

Temperatures in deg. Fahr., corresponding with specific heats in column 2.	Mean sp. ht. of Iron, from 32° F., computed for each 100° F.	Differences of sp. ht. for each 100°.		Ratio of computed to assumed sp. ht., viz.: $\frac{1}{6}$ water = $0.1666\dot{6}\dot{6}$.	Difference of ratio for each 100°		Observed loss of temperatures by heat-carrier at assumed ratio, H_2O to Fe, 6 to 1.	Differences of observed loss:	
		1st diff.	2d diff.		1st diff.	2d diff.		1st diff.	2d diff.
1	2	3	4	5	6	7	8	9	10
0	.10587			.63524			0.0		
32	.10687	328		.64122	1966		20.5	65.5	
100	.10915		47	.65490		287	65.5		4.5
		375			2253			70.0	
200	.11290		48	.67743		285	135.5		5.3
212	.11339	423		.68032	2538		144.2	75.3	
300	.11713		48	.70281		285	210.8		6.3
		471			2823			81.6	
400	.12184		47	.73104		287	292.4		7.1
		518			3110			88.7	
500	.12702		48	.76214		284	381.1		7.8
		566			3394			96.5	
600	.13268		47	.79608		287	477.6		8.9
		613			3681			105.4	
700	.13881		48	.83289		285	583.0		9.6
		661			3966			115.0	
800	.14542		48	.87255		285	698.0		10.6
		709			4251			125.6	
900	.15251		47	.91506		287	823.6		11.2
		756			4538			136.8	
1000	.16007		48	.96044		284	960.4		12.3
1082.5	.16667	804		1.00002	4822		1082.5	149.1	
1100	.16811		47	1.00866		287	1109.5		13.1
		851			5109			162.2	
1200	.17662		48	1.05975		285	1271.7		13.9
		899			5394			176.1	
1300	.18561		48	1.11369		285	1447.8		14.8
		947			5679			190.9	
1400	.19508		47	1.17048		287	1638.7		15.6
		994			5966			206.5	
1500	.20502		48	1.23014		284	1845.2		16.5
		1042			6250			223.0	
1600	.21544		47	1.29264		287	2068.2		17.4
		1089			6537			240.4	
1700	.22633		48	1.35801		285	2308.6		18.2
		1137			6822			258.6	
1800	.23770		48	1.42623		285	2567.2		19.1
		1185			7107			277.7	
1900	.24955		47	1.49730		286	2844.9		19.9
		1232			7394			297.6	
2000	.26187			1.57124			3142.5		

TABLE VI.

FOR HEAT-CARRIERS OF PLATINUM, IRON, ETC. (Pt, Fe).

True temperatures in deg. Fahr., corresponding with observed temperatures in columns 2, 5, and 8.	Observed loss of temperature by Platinum heat-carrier at assumed ratio of sp. ht. H_2O to Pt, 30 to 1.	Differences of observed loss for each 100° Platinum.		Observed loss of temperature by Iron heat-carrier, at assumed ratio of sp. ht. H_2O to Fe, 6 to 1.	Differences of observed loss for each 100° Iron.		Observed loss of temperature by comp'nd heat-carrier, at assumed ratios of sp. ht. Pt, $\frac{1}{30}$. Fe, $\frac{1}{6}$.	Differences of observed loss for each 100°. Pt and Fe.	
		1st diff.	2d diff.		1st diff.	2d diff.		1st diff.	2d diff.
1	2	3	4	5	6	7	8	9	10
0	0			0			0		
32	30.8	96.9		20.5	65.5		22.2	70.7	
100	96.9		1.7	65.5		4.5	70.7		4.1
		98.6			70.0			74.8	
200	195.5		1.9	135.5		5.3	145.5		4.7
212	207.5	100.5		144.2	75.3		154.8	79.5	
300	296.0		1.8	210.8		6.3	225.0		5.6
		102.3			81.6			85.1	
400	398.3		1.8	292.4		7.1	310.1		6.1
446.2	446.2	104.1			88.7			91.2	
500	502.4		1.9	381.1		7.8	401.3		6.9
		106.0			96.5			98.1	
600	608.4		1.8	477.6		8.9	499.4		7.7
		107.8			105.4			105.8	
700	716.2		1.8	583.0		9.6	605.2		8.3
		109.6			115.0			114.1	
800	825.8		1.8	698.0		10.6	719.3		9.1
		111.4			125.6			123.2	
900	937.2		1.8	823.6		11.2	842.5		9.7
		113.2			136.8			132.9	
1000	1050.4		1.9	960.4		12.3	975.4		10.5
1060	1119.5	115.1		1048.4	149.1		1060.2	143.4	
1082.5	1145.9			1082.5		13.1	1093.1		11.3
1100	1165.5		1.8	1109.5			1118.8		
		116.9			162.2			154.7	
1200	1282.4		1.8	1271.7		13.9	1273.5		11.8
		118.7			176.1			166.5	
1300	1401.1		1.9	1447.8		14.8	1440.0		12.7
		120.6			190.9			179.2	
1400	1521.7		1.8	1638.7		15.6	1619.2		13.3
		122.4			206.5			192.5	
1500	1644.1		1.9	1845.2		16.5	1811.7		14.0
		124.3			223.0			206.5	
1600	1768.4		1.8	2068.2		17.4	2018.2		14.9
		126.1			240.4			221.4	
1700	1894.5		1.8	2308.6		18.2	2239.6		15.4
		127.9			258.6			236.8	
1800	2022.4		1.8	2567.2		19.1	2476.4		16.2
		129.7			277.7			253.0	
1900	2152.1		2.0	2844.9		19.9	2729.4		17.0
		131.7			297.6			270.0	
2000	2283.8			3142.5			2999.4		

The apparatus for heating platinum heat-carriers for the pyrometer consists of two black-lead crucibles, 2 inches inside diameter at the top, and 3 inches deep, with suitable covers, as shown in Figs. 167 and 168, which are set into cavities in molded fire-brick, to avoid danger of accidental overturns. Platinum balls, each weighing 0.6 lb. avoirdupois (4,200 grains = 272.16 grammes), or one such ball, and two of 4,200 grains aggregate weight, are placed in the two crucibles—4,200 grains in each—covered up, and submitted to the heat in the desired exposure, long enough to insure uniform heating throughout. If the temperature fluctuates, as in most exposures will be likely to happen, the fire-brick and crucibles will in some degree integrate the fluctuations during the period of exposure.

For moderate temperatures, not exceeding a low red heat, and in situations not admitting of the use of crucibles, a pair of tongs was used, four or five feet in length, of steel, quite slender, with the extremities of their jaws concave, of suitable form and size to receive and cover the platinum ball. A link slipped over the handles held the ball securely, and permitted it to be put into places otherwise inaccessible, kept there until heated, and then withdrawn quickly and released to the water of the pyrometer. The temperature of flue gases, and of the warm blast, determined in that way, agreed substantially with the readings of mercurial thermometers, in cases where these could be satisfactorily used. The temperature of the brick deep in the wall—near the inside, where the heat was too great for glass thermometers—was obtained in this manner. Many temperatures ascertained by the use of this instrument, in the fire, at the bridge wall, at the pier, and in the arch over warm-blast boiler No. 1, will be found under the proper head.

2. The Calorimeter.—This instrument, shown in section in Fig. 169, was the result of much study, and answered fairly the expectations formed of it.

The lining, which is the true containing vessel, is of 24 oz. tinned copper, 17 inches in diameter and 32 inches deep, with a rim at the top 2.25 inches wide, of the same copper; and weighs, complete, 27 lbs. avoirdupois. This was surrounded, sides and bottom by a case of galvanized iron (Fe and Zn), 18.5 inches in diameter, 32.75 inches deep, No. 26 Birmingham wire gauge, weighing 15.5 pounds. A second case surrounds this 20 inches in diameter and 33.5 inches deep, of galvanized iron No. 26 w. g., weighing 16.1 pounds. An outside case surrounds all, 21.5 inches

in diameter and 34.25 inches deep, of galvanized iron No. 18 w. g., weighing, with its handles, 48 pounds. There are, therefore, three chambers, each 0.75 inch thick, all around, sides and bottom, the outer one of which is filled with hair felt, the two others with eider down. There is a cover, also in three compartments, filled in the same manner. The lower compartment of the cover is a little less than 17 inches in diameter, and freely enters the 17-inch inner chamber; the other two are 21.5 inches in diameter, and extend

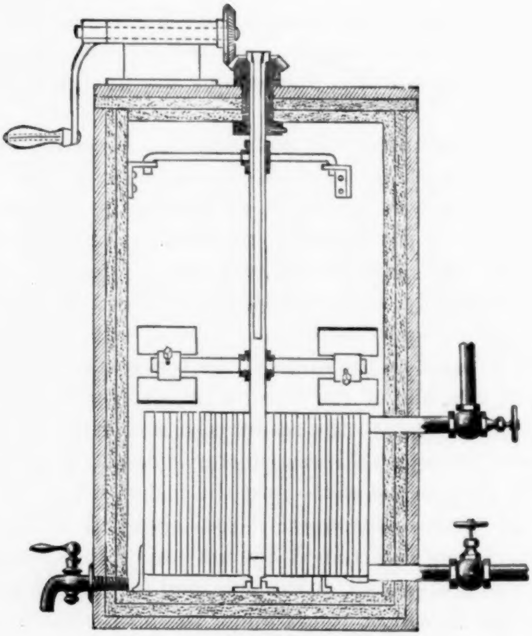


FIG. 169.

CALORIMETER.

out flush with the outside of the case. The lower plate of the cover, and the cylindrical band around the lower third, are of 24-ounce tinned copper, and weigh 3 pounds. This copper lining of the calorimeter and of its cover, is supposed to follow closely all the changes of temperature of the inclosed water, and to be to a considerable degree insulated from the exterior parts, and from the outer air.

A steam surface condenser, 15 inches in diameter and 12 inches high, is set inside, on short legs with broad, flat feet, near the

bottom of the 17-inch chamber; so that there is a space of 1 inch beneath it and all around it.

The cylindrical case of this condenser is of sheet copper, 0.18 inch thick, and the heads are of cast brazing copper of the same thickness, all united by brazing. There is a 2-inch brass tube in the middle; and 99 brass tubes, 0.5 inch outside diameter, are set by expanding, and sealed air-tight with soft solder, which is always, when the instrument is in use, immersed in cool water. This drum, or condenser, will safely bear an internal pressure of 200 pounds per square inch. Steam is admitted into it by a 0.75-inch brass pipe near its upper end; and water resulting from condensation of steam is drawn off by a similar pipe placed so low as to completely drain the lower head. Both of these pipes are made tight where they pass through the walls of the calorimeter, and each is provided with a screw-valve stop-cock. A 0.75-inch pipe, with a Bibb cock, is inserted in the barrel of the calorimeter, to draw the water out of it.

The agitator, for insuring uniformity of temperature throughout the contents of the calorimeter, is constructed as follows: A brass pipe 1 inch in diameter, about 34 inches long, freely perforated with holes 0.375 inch in diameter, having at the lower end a pivot to rest in a suitable step at the bottom of the barrel, passes down through the 2-inch tube of the condenser, and rises, when resting in its step, to about the level of the top of the cover when in place. A light three-legged spider, supported by light brass ears riveted to the lining of the barrel near the top, and having in the middle a short brass tube loosely fitting the tubular shaft, steadies the upper end of this shaft in an upright position when the cover is removed, and gives rise to no constraint when the cover is on.

The cover has a bushing in the center through which passes the hollow hub of a miter gear of 4 inches pitch diameter, fitted to slip over the upper end of the tubular shaft when the cover is placed on the barrel. This miter gear is held in its place in the cover by a collar on the lower end of its hub below the cover. It is loose on the tubular shaft—which it is designed to turn—but is at once locked to it by the insertion of a thermometer-case, the upper end of which is provided with a stopple fitting the tubular shaft, and fitted with two wings which pass through slots in the upper end of the tubular shaft, and engage with corresponding key-seats in the hub of the miter gear, locking gear and shaft together. A pipe box, having a stand and foot riveted to the cover, carries a short

horizontal shaft, on which, at the inner end, is a miter gear engaging with the one on the upright tubular shaft; and at its outer end a crank, by means of which it may be turned, giving motion to the tubular shaft.

A brass collar, fitted to slide on the shaft, carries two arms of brass tube, 0.625-inch diameter, screwed into the collar, serving as set-screws, to fix the collar at any desired height, and as supports for vanes to agitate the water. These arms are about 7 inches long, and extend to within about an inch of the lining of the barrel. The vanes have two blades, each secured to hubs which slide and turn freely on the arms, and may be set in any desired position, and at any desired angle, by set-screws. These vanes may, therefore, be considered as propeller blades, which in turning in the proper direction give an upward motion to the water at the outer part of the space it fills, accompanied, of course, by an equal downward motion in the middle; and produce a circulation most conducive to equalization of temperature, without any alternate withdrawal and re-immersion of any part of the apparatus, which must always be attended by some loss of heat. Some improvement could be made, particularly by reducing the weight of certain parts, such as the steam condenser, especially for low pressures—under 120 pounds per square inch; but the general principles are believed to be sound, and the operation was fairly satisfactory.

It should be mentioned that blocks of dry pine wood were placed in the spaces at the bottom, under the feet of the condenser, to support the heavy weight of this part; yet this weight and the feet which support it are a source of great anxiety in moving the instrument, especially in shipping it long distances by rail, lest the feet should cut through the light copper lining of the barrel. A better plan would be to support the condenser on molded blocks of hard rubber of sufficient size to distribute the weight.

It was necessary to ascertain the number of pounds of water which might be taken as the equivalent of so much of the metal of the instrument as must be assumed to follow promptly all changes of temperature in the contained water. Three methods were pursued for this purpose:

1st. By direct calculation from the known weight and specific heat of the metals so situated;

2d. By drawing into the calorimeter, cooled down to a low temperature, a weighed quantity of water of a known higher temperature, and observing the resulting temperature—the method of mixture;

3d. By condensing a weighed quantity of steam of known pressure and temperature, known also to be dry saturated steam, because drawn from a quiescent boiler; with a weighed quantity of water of known temperature in the barrel—again the method of mixture.

By the first method we obtain:

TABLE VII.

EFFECTIVE HEAT VALUE OF THE CALORIMETER.

NAMES OF METALS.	Weight Pounds.	Specific heat.	Effective heat value; B. t. u.
Copper	171.80	.095	16.32
Tin	1.25	.057	.07
Brass	8.31	.094	.70
Soft solder, Sn 2, Pb 1	2.09	.048	.10
Totals	183.45	.0937	17.19

The mean specific of the mass, at the usual temperatures, is therefore $\frac{17.19}{183.45} = .0937$.

By the second method:

We have first to ascertain the limits of error in drawing off and weighing water from the steam condenser, as follows:

FIRST TRIAL.

Weight of water poured in	lbs.	11.80176
Weight of water drawn out	"	11.82324
Apparent excess drawn out	"	0.02148
Ratio of excess	per cent.	0.1818

SECOND TRIAL.

Weight of water poured in	lbs.	11.82129
Weight of water drawn out	"	11.79785
Apparent deficit drawn out	"	0.02344
Ratio of deficit	per cent.	0.1985

THIRD TRIAL.

Weight of water poured in	lbs.	11.80566
Weight of water drawn out	"	11.82324
Apparent excess drawn out	"	0.01758
Ratio of excess	per cent.	0.1483

Combining the errors of the three trials, we obtain:

TABLE VIII.

ERRORS OF POURING IN, DRAWING OUT, AND WEIGHING.

	POUNDS.	OUNCES.	PER CENT.
First error.................................+	0.02184	0.349	0.1818
Second error.................................–	0.02340	0.374	0.1985
Third error.................................+	0.01758	0.281	0.1483
Means.................................	0.02092	0.335	0.1762

It will be seen that the errors are in opposite directions, and that they include errors of pouring in and of drawing out; and of two weighings. The error of drawing out and one weighing, is therefore less than one-third of an ounce, less than one-fiftieth of a

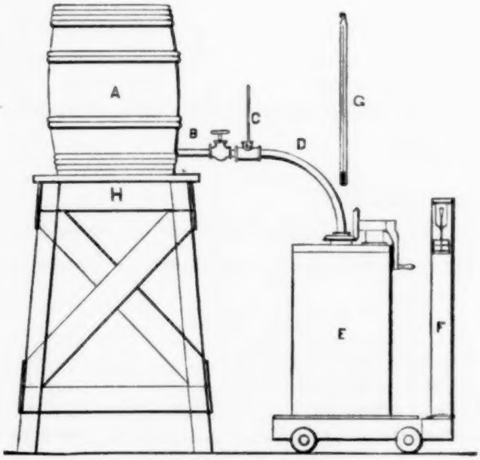


FIG. 170.

APPARATUS FOR TESTING THE HEAT CAPACITY OF THE CALORIMETER.

A, Cask containing about 300 pounds of warm water.
B, Iron pipe and stop-cock.
C, Thermometer in warm-water pipe.
D, Rubber hose for warm water.
E, Calorimeter.
F, Platform scales weighing to ounces.
G, Thermometer, graduated to $\frac{1}{5}$ degree F.
H, Stand to support cask, A.

pound, and less than one-sixth of one per cent. on quantities of 11 to 12 pounds. The calorimeter was accurately leveled before making the experiments.

In testing the heat capacity of the calorimeter by this second method, the apparatus represented in Fig 170 was used.

The calorimeter was brought to about the temperature of the room, in order to reduce to a minimum its changes of temperature by external influences; and after sufficient use of the agitator to bring the contained air to uniformity, readings of the central thermometer were taken once a minute during several minutes, in order to obtain the direction and rate of any change which might be observable. Meantime the cask A, of capacity considerably greater than that of the calorimeter (which is 200 lbs.), was filled with water by a hose, and warmed by steam, also by means of a hose, to 25° F. or more above the temperature of the room, only taking care to avoid a temperature so high as to cause rapid loss of heat by evaporation. After careful agitation with a stirring-stick, its temperature was taken, and a little drawn off, to waste, through the hose D (removed from the calorimeter), to bring the pipe and stop-cock B and the hose D to uniform temperature with the water. The stop-cock was then closed, and the end of the hose inserted in the upper end of the tubular shaft of the agitator, from which the thermometer and its case had been removed, as seen in Fig. 170; and the stop-cock was opened, and the water was allowed to flow from the cask into the calorimeter. Readings of the thermometer C were noted every 15 seconds during the filling, which takes 5 minutes. The temperature was pretty nearly uniform, and such variations as were observed, were due, it is probable, to imperfect mixing.

When the scales indicate that about the required quantity of water has flowed into the calorimeter, the stop-cock is closed, the hose is removed, the thermometer G, with its perforated tubular case, is replaced, the agitator is put in motion, and a set of readings of the thermometer G is taken at intervals of 1 minute (or less), for 5 minutes. The rate of cooling, which is regular, and very slow, being thus ascertained, it can be carried back to any desired point of time—in practice, to the time when the calorimeter was half full; and in a similar manner the slowly and regularly changing temperature can be brought forward to the same point of time. It is of course plain that the difference of these two temperatures at the same instant, will be the measure of the calorific capacity of the calorimeter.

It is necessary to compare the two thermometers, C and G, and to allow for any difference which may be found in their readings in identical circumstances. It was found that when both were immersed in water at 95° F. to the same extent as when used in these

determinations, the latter (G) read 0.3 deg. *lower* than the former (C The observations follow.

TABLE IX.

DETERMINATION OF THE HEAT CAPACITY OF CALORIMETER.

Time of readings by Auburndale horse-timer.		Readings of thermometer G when calorimeter is empty.	Readings of thermometer G after calorimeter is filled with water.	Readings of thermometer C in warm water while calorimeter is filling.
1		2	3	4
Min.	Sec.			
0	0	76.60°		
1	0	76.65°		
2	0	76.70°		
3	0	76.75°		
4	0	76.80°		
5	0	Began to fill calorimeter with warm water.		
6				101.6°
7				101.6°
7	30	76.975°	98.95°	
8				101.6°
9				101.6°
10		Filled.		101.6°
11				
12			98.725°	
13			98.675°	
14			98.625°	
15			98.575°	
16			98.525°	
17			98.500°	

By extending the range according to the ascertained rate of change of temperature, we obtain :

Change of empty calorimeter per minute, 0.05° during 3.5 minutes, 4 minutes to 7.5 minutes, and $.05 \times 3.5 = 0.175°$, to add to 76.8°, making this temperature when half the water had entered the calorimeter, 76.975°.

Change of water after filling the calorimeter, 0.05° per minute during 4.5 minutes, to be carried back 4.5 minutes, from 12 minutes to 7.5 minutes, and $.05 \times 4.5 = 0.225°$, to be added to 98.725°, making 98.950°.

Temperature of warm water before entering the calorimeter, 101.6°.

Weight of calorimeter and water, pounds	489.125
Weight of empty calorimeter, without thermometer and case	315.250
Weight of warm water put in....	173.875

To the temperature ascertained by thermometer G, we must add 0.3°, by which amount it read lower than thermometer C, making:

$$76.975 + 0.3 = 77.275, \text{ and}$$
$$98.950 + 0.3 = 99.250.$$

Corrected for the varying specific heat of water, the corresponding number of British thermal units is set opposite each temperature, respectively, in the following table:

TABLE X.

QUANTITIES OF HEAT IN BRITISH THERMAL UNITS.

	Degrees.	B. t. u.
Calorimeter, at half full	77.275	77.3056
Water, calorimeter half full	99.25	99.3278
Water before entering calorimeter	101.6	101.6848

Then:

$$x = \frac{(173.875 + 18.61) \times (101.6848 - 99.3278)}{101.6848 - 77.3056} =$$

$$\frac{129.485 \times 2.357}{24.3792} = 192.485 \times .09668 = 18.61.$$

The value of $x = 18.61$, the quantity sought, has to be found by a few trials, beginning with 17.19, found by the first method, which proves to be too small by 0.8 per cent. to satisfy the conditions of the equation.

Nine other similar determinations gave, with the foregoing, the ten following values: 18.09, 18.61, 18.50, 18.92, 19.06, 19.10, 19.20, 18.40, 18.55, 19.42.

The mean of all is	18.78
To this add for solder put on afterward, 2.09 lbs., sp. ht. = .048	.10
Calorific value of calorimeter as ascertained by the second method	18.88

This is nearly 10 per cent. more than was found by the first method, p. 719, which was 17.2.

By the third method:

At 5 hours 45 minutes P.M., when for an hour no steam had been drawn from the boiler, while steam pressure at about 51.6 pounds per square inch by a test guage had been steadily maintained, the steam must be considered as substantially "dry, satu-

rated steam." It could not be "superheated," because there was no superheating surface; and it could not contain much suspended water in liquid form, because there had been no ebullition for an hour, and the ebullition caused by drawing off to the calorimeter 45.4 cubic feet of steam—less than one-half the cubic contents of the steam space—through a $\frac{3}{4}$-inch pipe, must have been very slight.

Account of the experiment to determine by the third method the heat capacity of the calorimeter.

Height of mercurial barometer, inches	29.51
Atmospheric pressure; lbs. per sq. inch	14.494
Steam pressure by steam gauge	51.6
Steam pressure, absolute	66.094
Number of B. t. u. contained in one pound of steam of 66.094 lbs. per sq. inch pressure, absolute	1205.1060
Temperature to which water condensed from steam was reduced in the calorimeter	85.75°
Number of B. t. u. contained in water of temperature 85.75°	85.7965
Number of B. t. u. surrendered by each 1 lb. of steam on condensation and cooling to 85.75°, 1205.1060 − 85.7965 =	1119.3095
Quantity of water from condensed steam, drawn off and weighed on coin scales, lbs	7.238
Total number of B. t. u. given up by 7.238 pounds of steam; 1119.3095 × 7.238 =	8101.5622
Temperature of water in calorimeter before admitting steam, degrees F	48.45
Number of B. t. u. contained in each 1 lb. of this water	48.453
Number of B. t. u. gained by each 1 lb. of this water in rising from 48.45° to 85.75°, 85.7965 − 48.4530 =	37.3435
Total number of B. t. u. imparted to the water, divided by the number of B. t. u. gained by 1 lb., 8101.5622 ÷ 37.3435 =	216.9470
Weight of water in calorimeter	200.0000
Water value of calorimeter, lbs	16.9470

This result is only 1.47 per cent. less than the result obtained by the first method, and is strongly confirmatory of that result; especially in view of the fact that several experiments made in circumstances nearly similar, gave "saturated steam" when the value 17.2 was used for the calorimeter.

This third method is entitled to much weight, because it is precisely the method pursued in ordinary use. Yet it seems hardly probable that this value can be less than we have found it to be by direct calculation, by the first method (p. 719); and I have there-

fore adopted as the heat-equivalent of the calorimeter, in the following calorimetric work, in terms of water, 17.2 pounds.

The work done with this instrument, will be found in the sequel.

3. THE ANEMOMETER.—The germ, and perhaps something more than the germ, of this beautiful instrument, is to be found in Weisbach's *Lehrbuch der Ingenieur - und Maschinen-Mechanik*, Braunschweig, Friedrich Vieweg und Sohn, 1857, vol. 2, pp. 734, 735, under the name of the Wollaston Anemometer. In its present form, it is the joint production of Mr. F. H. Prentiss and the writer, although the principal share belongs to Mr. Prentiss.

Two glass tubes (Fig. 171), about 30 inches long, about 0.4 inch diameter inside and 0.7 inch outside, are connected at each end by means of stuffing-boxes, to suitable brass attachments, through which they are secured to a backing of wood.


FIG. 171.
ANEMOMETER.

These attachments, at top and bottom, have each a tubular opening, with a stop-cock in the middle of its length, which can be turned at will to establish a free communication between the glass tubes, or to shut off all communication. Directly over each tube a brass drum-shaped vessel is placed, 4.25 inches in length and of equal diameter. The heads of these drums, at both ends, are formed of plate glass, properly secured with screw-rings, and made tight with suitable packing. A tubular opening extends up from each glass tube to the drum above it, and there is a hole in each drum, directly in line with the axis of the glass tube, each fitted with a stop-cock and a nipple for attaching a flexible pipe. Two sliding scales are arranged between the glass tubes, to measure, the one depressions, the other elevations of the surface of a liquid filling the lower half of the tubes, indicated in the cut, Fig. 171, near the middle of the height. Both stop-cocks are represented in the cut as closed.

The lower one being opened the two tubes, in communication at their lower ends, are filled up to about the middle of their height with a mixture of alcohol and water, care being taken to avoid wetting the interior of the upper end of the tube poured through—the pouring being done through a small glass tube inserted through

the hole at the top of the drum, from which the stop-cock is removed for this purpose. The filling-tube is now to be raised so that its lower end is a little above the surface of the alcohol and water, the lower stop-cock is to be closed, and the upper one opened; and crude olive oil is to be carefully poured in until it fills the first tube up to the upper cross-tube into the second tube, and so finally fills both tubes and rises to about the middle of both drums.

The crude olive oil is of an olive-green color, and forms with the colorless alcohol and water a beautiful and very deep meniscus, if the tubes are clean, and the filling has been done with sufficient care, making the line of demarkation very distinct. Neither liquid discolors the glass, and if up-and-down motions are made cautiously and slowly, the liquids do not mix, and the common surfaces remain undisturbed. The specific gravity of the oil should be determined in advance, as it may vary a little, although we found it quite uniformly 0.916. The specific gravity of the alcohol and water may be made anything desired, between that of water, 1.000, and that of absolute alcohol, 0.813; but must always be made greater than that of the olive oil.

Where extreme delicacy is desired, the difference may be as small as 1 per cent.; that is, if the oil be as above, .916, the mixture of alcohol and water may be, .926. If the difference be much less than 1 per cent., the upper and lower liquids have a tendency to get into confusion, and do not constantly maintain a distinct line of demarkation at their common surface. For many purposes, a difference of specific gravity as great as 2 per cent. will give sufficient sensitiveness—fifty times as much range as a water column—and is more convenient to use.

The method of using this instrument to ascertain the force of chimney draft or other air current, is as follows: If both the stop-cocks between the tubes are opened, and both the small stop-cocks on the top of the drums are also opened, so that the surface of the oil in both drums alike is open to the air, *both* liquids will come to a level; the oil in the drums, very obviously, and the heavier mixture below the oil as certainly, if not quite as obviously; since if higher in one tube than in the other, the united weight of the two liquids in that tube must be greater than in the other, and must cause the liquid to sink down and flow into the other tube, raising the surface of the oil in the drum over that other tube, and causing it to flow across to the first tube, until both liquids are brought to a coincident height in the two tubes.

A slight difference will, however, commonly be found in the height of the lower liquid, owing to the unequal capillarity of the tubes, since these can rarely be obtained sufficiently near alike in caliber to avoid, when in equilibrium, a small, but sensible difference of level, which must be ascertained and allowed for.

If, now, the upper stop-cock between the tubes be closed, the lower one being left open, the surfaces of the two liquids will retain their respective heights in the two tubes, so long as the surface of the oil in the two drums remains subject to equal pressure. But if one drum be put in communication with a flue or chimney, by means of a flexible or other tube connected with the nipple of the small stop-cock—this stop-cock being open—while the other drum remains open to the air through its open stop-cock, the diminished pressure, due to chimney draft, upon the surface of the oil in that drum, will cause the oil to flow up into the drum, under the preponderating weight of the air on the surface of the oil in the other drum.

The surface of the oil in the drum is about 100 times as large as the inside cross-section of the glass tubes, and in the same proportion will the rise of the lower liquid on the one side, and its depression on the other, exceed the corresponding rise and depression of the upper surface of the oil.

If now, when equilibrium has been restored, the lower stop-cock be closed and the upper one opened, and the connection with the flue or chimney be severed—say by removing the flexible tube from the nipple—the lower liquid will be kept immovable, while the oil will flow through the upper cross-tube, and come to a common level in the two drums. On connecting the nipple again with the flue or chimney, and again closing the upper stop-cock and opening the lower one, a diminished repetition of the former action will take place; the lower liquid will rise a little in one tube and fall a little in the other, and the surface-level of the oil in the two drums will again become slightly unequal. This inequality, which will be much less than before, may be again removed by the same method; and a very few repetitions of this process will bring the difference in level of the surface of the lower liquid in the two tubes (corrected for inequality of capillary attraction, as explained above), to represent the entire difference in pressure on the surface of the oil in the two drums, due to the draft of the chimney; that is, a certain known height of column, filled, in one tube with a mixture of alcohol and water of specific gravity 0.926, with the flue-pressure resting on its surface, is just balanced by an equal

height filled with olive oil of specific gravity 0.916, with the pressure of the atmosphere resting on its surface. The differential column, therefore, represents a water column one-hundredth part as high, or a column of mercury $\frac{1}{1360}$ part as high. A draft which would be measured by 0.01 inch of mercury, or by 0.136 inch of water, would, on this anemometer, be measured by 13.5 inches of differential column. It is therefore a hundred times as sensitive as a water column, and more than 1,300 times as sensitive as a mercury column. If too sensitive, so that the required range would exceed the limits of the instrument, its sensitiveness can be reduced to any desired extent by a larger admixture of water, or by the use of pure water, as described by Weisbach, in which latter case the difference of specific gravity will be (1.000 − 0.916) = 0.084, and the sensitiveness 11.9 times as great as that of water alone, and 160 times as great as that of a mercury column.

This instrument with the respective specific gravities 0.937 and 0.916, difference, 0.21, equal to 2.1 per cent. was sensitive enough to show plainly the reduction of chimney draft caused by opening a sliding register in the fire-door for the admission of air above the fire, giving an aggregate open area of no more than six square inches. An instrument of such delicacy for determining pressures affords the best attainable data for estimating the velocity of air currents, far superior to the Casella revolving anemometer, or any other known to me. I will only add that after trying almost every applicable substance for packing the stuffing-boxes around the ends of the glass tubes, rings of cork, cut out of sheets of fine cork 0.25 inch thick, of suitable size to go tight over the tubes without bursting open, and to go easily into the stuffing-box, answered best; that is, they stopped all sensible leakage; but there was still a slow waste of alcohol—and perhaps of water also—by insensible leakage, and evaporation on coming to the air. The compression of the cork was very great; six rings of 0.25 inch each—1.5 inches in the aggregate, were compressed into a thickness of less than an eighth of an inch. Too much care cannot be taken to make all joints and stop-cocks tight. All passages through the brass should be drilled, and to this end, the turns at the lower end should be rectangular, instead of curved quarter-turns, as shown in Fig. 171.

4. The Incased Aneroid.—This is simply a fine aneroid barometer, 8.12 inches outside diameter, 2 inches in thickness, put into a brass case, resembling the case of a large steam gauge, fitted with a stout ring, by means of which a plate-glass cover is made air tight.

A 3-way cock at the bottom, connected by a flexible tube with a pipe inserted in a flue, affords facility for observing, alternately, and as often as desired, the difference between the barometric pressure of the external air, and the rarefied air within the brass case, outside of the aneroid, when the cock is open to the flue. Each inch of mercury is represented by an arc 2.21 inches in length, divided to tenths and fiftieths. Each of the smaller divisions is therefore equal to .044 inch, and to 1.12 millimeters, and is easily divisible by the practiced eye, to tenths, equal to .002 inch mercury, representing say 0.001 lb. pressure per square inch, and to .0277 inch of water. A hole 0.5 inch diameter, properly located in the back of the case, and stopped with an air-tight screw plug, gives access, on removing the plug, to the adjusting screw of the aneroid, so that the latter can be compared—and if it need be adjusted—by the mercurial barometer. This instrument was found convenient and useful.

5. THE MERCURIAL BAROMETER.—This was an ordinary "Signal Service" barometer, by J. & H. J. Green, New York, with freshly boiled mercury, and in all respects in good order. All observations were corrected for temperature, by attached thermometer. The floor of the boiler room is about 37 feet (11.28 meters), above mean tide in Boston harbor, and the barometer itself, as observed, 40 feet. This elevation is equal to 0.0456 inch of mercury and to 0.093 lb. per square inch. The mean weight of the atmosphere is therefore: $29.9218 - 0.0456 = 29.8762$ inches, and to $14.696 - 0.093 = 14.603$ lbs. per square inch. Lawrence is 26 miles N. by W. from Boston, in Lat. 42° 42′ 30″ N., Long. 71° 10′ 0″ W. The value of g (the force of gravity) is 32.163.

6. THE HYGROMETER.—This was the wet-and-dry bulb "Hygrophant," of J. S. F. Huddleston, Boston. The observations were reduced by Guyot's Tables, 3d edition, Smithsonian Institution, Washington, D. C.

7. THERMOMETERS.—These were many in number, and in considerable variety; all which were used for purposes requiring accuracy being tested by Mr. J. S. F. Huddleston, Boston. All were graduated to the Fahrenheit scale, some to half degrees, some to one-fifth degree, and some to one-tenth degree. One long and delicate thermometer, sole survivor of its class, is described as follows:

Whole length	31.5	inches.
Length of bulb (mean)	2.3	"
Diameter throughout	0.25	"

Length of graduated stem .... 28 inches
Graduated, range, 19° to 83° = 64°.
Graduated to $\frac{1}{10}$ degree.
Whole weight .... 1405 grains.
" glass, 80.5% .... 1132 "
" Hg, 19.5% .... 273 "
Sp. ht. glass, .1923 ; Hg, .0290, mean, .1705.

Heat value, $\frac{1405 \times .1705}{7000} = .034$ B. t. u.

This thermometer was used with the steam calorimeter, as were others similar to it, which were broken. Being accurately calibrated and carefully divided, .4375 inch to 1°, 0.437 = 1.11 millimeters to 0.1°, it may be read with much confidence to 0.01°. It is not to be supposed that actual temperatures, above 0° F. could be determined by it to this degree of accuracy; but differences of temperature, within a moderate range, as in calorimetric experiments, may be considered as correct within less than 0.01° at each reading, say 0.02° in the observed range. On a range of 10°, as in pyrometry, this would be 0.2%. On a range of 50°, as in calorimetry, 0.04%.

8. The Winckler Apparatus.—This clever and convenient instrument for ascertaining, approximately, the quantity of carbon dioxide (CO_2) in flue gases, by the volumetric method, will not be described, as it exists, so far as I know, in only one form, and is to be obtained from dealers in chemical instruments, apparatus and materials. It is not delicate enough to determine the quantity of carbon monoxide (CO), but will detect traces of it when present.

But this gas will *never* be found in more than very minute quantities in any reasonably well-managed fire, and for the convenient and expeditious determination of the CO_2, and consequently of the quantity of atmospheric air per pound of carbon (and per pound of coal, if the composition of the coal is known), the Winckler apparatus is very valuable. It was used in these experiments only as an auxiliary—the CO_2 and CO being ascertained throughout by the more accurate gravimetric method.

9. The Geissler Bulbs.—These are used in determining both the CO_2 and the CO in flue gases, by the gravimetric method, and are seen suspended from the scales, and also in place in the apparatus, in Fig. 172 (to be introduced in the sequel). I remark only that they should be of large size, in order to deal with considerable quantities of the absorbed gases.

10. The Chemical Balance.—This, also, should be of large size,

to weigh 200 milligrammes without injury to the scales; and of the best quality obtainable. The one used in these experiments, was made by H. Troemner, Philadelphia, weighing up to 200 grammes, = 3086.5 grains, or about 0.44 lb. av., divided to weigh to tenths of a milligramme, but capable, by skillful manipulation, of weighing to twentieths of a milligramme (.00005 gramme).

11. The Steam Gauge.—A 10-inch Bourdon steam gauge, made by the American Steam-Gauge Company, compared with a mercury column—as often before—both before and after the experiments, was put into a position where it could not be affected by heat from the boilers, and kept shut off except at the time of quarter-hourly readings. Every opening of the stop-cock, to let the pressure come to this gauge, produced an instantaneous lowering of the pressure in the small pipe leading from the boiler, and recorded itself by a slight mark on the trace of the Edson recording pressure gauge—a very satisfactory check upon the accuracy of the readings in point of time.

12. The Edson Pressure-Recording Gauge.—Continuous tracings from this instrument—night and day—were taken, and integrated, for comparison with the record of the test gauge.

A set of these tracings, for one week, will be found reproduced hereafter.

Some other minor pieces of apparatus will be briefly described, so far as necessary, in connection with the account of their use.

III.

It is proposed next to present a general summary of results, followed by a condensed record of the weekly experiments.

GENERAL SUMMARY OF RESULTS.

PACIFIC BOILER: Cold Blast, Natural Draft.
WARM-BLAST BOILER NO. 1: Abstractors with double tubes.
WARM-BLAST BOILER NO. 2: Abstractors with deflectors, applied to the "Pacific Boiler."

The results with anthracite, in the Pacific boiler, are the means for five weekly trials; all the others are for single weekly trials.

	Anthracite.	Bituminous.
Coal consumed, net, per week:		
Pacific Boiler	16264	12890
Warm Blast No. 1	20368	15184
Warm Blast No. 2	16740	
Water evaporated per week:		
Pacific Boiler	147039	121590
Warm Blast No. 1	180542	145075
Warm Blast No. 2	157483	
Pounds of water per pound of coal:		
Pacific Boiler	9.04	9.43
Warm Blast No. 1	8.86	9.55
Warm Blast No. 2	9.41	
Mean temperature of feed water:		
Pacific Boiler (Fahr.)	71.90°	72.40°
Warm Blast No. 1	38°	36°
Warm Blast No. 2	49°	
Mean temperature of external air, days:		
Pacific Boiler (Fahr.)	78.3°	71°
Warm Blast No. 1	34°	34.2°
Warm Blast No. 2	49°	
Steam gauge pressure above atmosphere, pounds per square inch:		
Pacific Boiler	47.54	47.30
Warm Blast No. 1	54.40	64.40
Warm Blast No. 2	42.50	
Mean barometric pressure, pounds per square inch:		
Pacific Boiler	14.47	14.61
Warm Blast No. 1	14.64	14.66
Warm Blast No. 2	14.70	
Steam pressure, absolute:		
Pacific Boiler	62.01	61.91
Warm Blast No. 1	69.04	79.06
Warm Blast No. 2	57.20	

	Anthracite.	Bituminous.
Pounds of water evaporated from and at 212° F., per pound of coal, days and nights:		
Pacific Boiler	10.51	10.58
Warm Blast No. 1	10.81	11.54
Warm Blast No. 2	11.12	
Water evaporated from and at 212° F. by day, per pound of coal burned during days and nights:		
Pacific Boiler	9.34	9.22
Warm Blast No. 1	10.00	10.72
Warm Blast No. 2	10.77	
Evaporative power of coal:		
Pacific Boiler	13.56	14.27
Warm Blast No. 1	13.45	14.30
Warm Blast No. 2	13.61	
Efficiency, days and nights, per cent.:		
Pacific Boiler	77.48	76.73
Warm Blast No. 1	80.37	80.70
Warm Blast No. 2	81.74	
Efficiency, days, per cent.:		
Pacific Boiler	79.96	76.53
Warm Blast No. 1	87.05	84.21
Warm Blast No. 2	87.76	
Efficiency: water, days; coal, days and nights, per cent.:		
Pacific Boiler	68.87	64.61
Warm Blast No. 1	74.35	74.96
Warm Blast No. 2	79.20	
Losses: per cent., complement of efficiency: water, days only; coal, days and nights:		
Pacific Boiler	31.13	35.39
Warm Blast No. 1	25.65	25.04
Warm Blast No. 2	20.80	
Losses, per cent., at chimney, by radiation from brick-work, and by imperfect combustion, = CO:		
Pacific Boiler, chimney	17.75	17.03
Radiation	2.64	3.39
CO	2.13	2.85
	22.52	23.27
Warm Blast No. 1 chimney	15.00	14.24
Radiation	4.00	4.00
CO	0.63	1.06
	19.63	19.30
Warm Blast No. 2, chimney	12.83	
Radiation	4.00	
CO	1.43	
	18.26	

	Anthracite.	Bituminous.
Temperature of smoke-box, Fahr.:		
Pacific Boiler	368.3°	376.9°
Warm Blast No. 1	396.9°	397.4°
Warm Blast No. 2	377°	
Temperature of air supplied to furnace :		
Pacific Boiler	78.3°	71°
Warm Blast No. 1	337.7°	349.5°
Warm Blast No. 2	334°	
Temperature of escaping gases.		
Pacific Boiler	368.3°	376.9°
Warm Blast No. 1	189°	196°
Warm Blast No. 2	164°	
Gases cooled by abstractors :		
Pacific Boiler	0°	0°
Warm Blast No. 1	207.9°	201.4°
Warm Blast No. 2	213°	
Air warmed by abstractors :		
Pacific Boiler	0°	0°
Warm Blast No. 1	303.7°	315.5°
Warm Blast No. 2	285°	
Temperature of steam, days :		
Pacific Boiler	297.5°	297.3°
Warm Blast No. 1	361.1°	322.6°
Warm Blast No. 2	291.2°	
Difference of temperature, boiler and gases :		
Pacific Boiler, gases *above* boiler	70.8°	79.6°
Warm Blast No. 1, " *below* "	127.1°	126.6°
Warm Blast No. 2, " *below* "	127.2°	
Difference of temperature, boiler and air supply :		
Pacific Boiler, air *below* boiler	219.2°	226.3°
Warm Blast No. 1, " *above* "	21.6°	26.9°
Warm Blast No. 2, " *above* "	42.8°	
Pounds of flue gases per pound of coal, days :		
Pacific Boiler	22.39	25.23
Warm Blast No. 1	23.49	28.37
Warm Blast No. 2	24.17	
Pounds of water equivalent in heat capacity to flue gases per pound of coal ; sp. heat of gases = 0.238.		
Pacific Boiler	5.33	6.00
Warm Blast No. 1	5.59	6.75
Warm Blast No. 2	5.75	
British thermal units carried off in gases per pound of coal, days :		
Pacific Boiler	1576	1835
Warm Blast No. 1	866	1092
Warm Blast No. 2	661	

	Anthracite.	Bituminous.
Efficiency *corrected* for difference in temperature of external air, and difference in time of banking fires:		
Pacific Boiler........................ per cent.	68.87	64.61
Warm Blast No. 1...........................	78.18	77.59
Warm Blast No. 2...........................	81.43	
Difference of Efficiency : Points gained by warm blast, over Pacific Boiler, cold blast :		
Warm Blast No. 1...........................	9.31	12 98
Warm Blast No. 2...........................	12.56	
Ratio of gain to the larger quantity ($\frac{9.31}{78.18}$ = 11.9% etc.)		
Warm Blast No. 1.......................per cent.	11.9	16.7
Warm Blast No. 2...........................	15.4	
Ratio of gain to the smaller quantity ($\frac{9.31}{68.87}$ = 13.5% etc.):		
Warm Blast No. 1...........................	13.5	20.1
Warm Blast No. 2...........................	18.2	

The power consumed in driving the blower is about 1 per cent. of the whole power produced by the boiler in combination with a good steam engine.

It therefore appears that the net saving effected by the warm blast was from 10.7 to 15.5 per cent. of the fuel used with cold blast, which is the same thing as to say that discontinuing the warm blast would cause an increased consumption of fuel equal to from 12.3 to 18.9 per cent. of the quantity used with hot blast. Broadly stated, the gain is 10 to 15 per cent.

IV.

CONDENSED RECORD OF WEEKLY EXPERIMENTS.

The following tables are greatly condensed, embodying, as they do, the summing up of more than 1,250 pages of notes taken during the tests, and the results of very laborious calculations. Table XI., occupying eleven pages, is progressive, the successive sections, numbered at the left hand 1 to 38, requiring for their full explanation only preceding sections. Observe, that the line "Mean, for anthracite," gives for the Pacific Boiler (cold blast), the means of the first 5 weeks, A, B, C, D, E; and for the Warm-Blast Boiler, the means of the first and third week, G and I—the single weeks, F and H, are to be compared with the corresponding means.

TABLE XI.—CONDENSED RECORD OF WEEKLY EXPERIMENTS.

KIND OF QUANTITY.	PACIFIC BOILER. For the week 1881.	ending,	QUANTITIES.	WARM-BLAST BOILER. For the week 1882.	ending,	QUANTITIES.
1. Pounds of coal thrown on the fire grate during the week: all anthracite except for weeks F & H, which are bituminous; and except 1326 pounds used in week I, for banking	A	July 16	16251.5	G	Feb. 4	21009
	B	" 23	16949	H	" 11	15842
	C	" 30	18652	I	May 20	Anthr. 15591
	D	Aug. 6	17951.5			Bitu. 1326
	E	" 13	15321			
	F	" 20	12836.5			
Mean, with anthracite			17025			18963
Mean of all			16326.9			17923
2. Pounds of dry wood burned during the week for kindling—Monday morning	A	July 16	271.5	G	Feb. 4	292
	B	" 23	261.5	H	" 11	280
	C	" 30	251.5	I	May 20	261
	D	Aug. 6	288.5			
	E	" 13	272			
	F	" 20	223			
Mean, with anthracite			269			276
Mean of all			261.3			278
3. Pounds of coal equivalent to wood burned during the week, being 0.4 of the wood burned	A	July 16	108.5	G	Feb. 4	116
	B	" 23	104.6	H	" 11	112
	C	" 30	100.5	I	May 20	104
	D	Aug. 6	115.4			
	E	" 13	109			
	F	" 20	89.2			
Mean, with anthracite			107.6			110
Mean of all			104.5			111

4. Pounds of ashes and residue accumulated during the week	A	July 16	2158	G	Feb. 4	3102
	B	" 23	2628	H	" 11	1539
	C	" 30	2809	I	May 20	1888
	D	Aug. 6	2432.5			
	E	" 13	2163.25			
	F	" 20	947			
Mean, from anthracite			2438.2			2495
Mean of all			2189.6			2176.3
5. Pounds of carbon in ashes and residue for the week	A	July 16	514	G	Feb. 4	617
	B	" 23	928	H	" 11	628
	C	" 30	923	I	May 20	287
	D	Aug. 6	572.8			
	E	" 13	631			
	F	" 20	29			
Mean, for anthracite			713.8			452
Mean of all			599.6			510.7
6. Pounds of coal equivalent to the carbon in ashes and residue for the week	A	July 16	623	G	Feb. 4	757
	B	" 23	1133	H	" 11	768
	C	" 30	1117.1	I	May 20	346
	D	Aug. 6	696.4			
	E	" 13	760			
	F	" 20	39			
Mean, for anthracite			865.3			551.5
Mean of all			727.6			623.7
7. Pounds of coal consumed during the week, allowing for wood consumed, and for coal equivalent to carbon in ashes and residue. Weeks F & H bituminous; all others anthracite	A	July 16	15737	G	Feb. 4	20368
	B	" 23	15921	H	" 11	15184
	C	" 30	17618.8	I	May 20	16740
	D	Aug. 6	17370.5			
	E	" 13	14670			
	F	" 20	12890			
Mean of anthracite			16263.5			18554
Mean of all			15701.2			17430.7

TABLE XI.—CONDENSED RECORD OF WEEKLY EXPERIMENTS.—*Continued.*

KIND OF QUANTITY.	PACIFIC BOILER. For the week 1881.	ending,	QUANTITIES.		WARM-BLAST BOILER. For the week 1882.	ending,	QUANTITIES.	
8. Mean temperature of feed water during the week, degrees Fahrenheit........................	A	July 16	72.56°		G	Feb. 4	38°	
	B	" 23	72.16°		H	" 11	36°	
	C	" 30	71.40°		I	May 20	49°	
	D	Aug. 6	71.28°					
	E	" 13	72.08°					
	F	" 20	72.40°					
Mean, with anthracite.........			71.90°				43.5°	
Mean of all...................			71.98°				41	
			Days.	Nights.			Days.	Nights.
9. Steam pressure by steam gauge; means for days and nights separately: days, left-hand column; nights, right-hand column..	A	July 16	51.20	34.90	G	Feb. 4	54.4	
	B	" 23	46.75	48.36	H	" 11	64.4	
	C	" 30	47.65	55.34	I	May 20	42.5	
	D	Aug. 6	51.18	53.76				(Not recorded.)
	E	" 13	40.92	50.57				
	F	" 20	47.30	54.10				
Mean, with anthracite.........			47.54	48.59			48.45	
Mean of all.........			47.50	49.50			53.77	
10. Pounds of water evaporated during the week while the dampers were open; days..................	A	July 16	124899		G	Feb. 4	167072	
	B	" 23	126687		H	" 11	134976	
	C	" 30	145234		I	May 20	152683	
	D	Aug. 6	137922					
	E	" 13	119844					
	F	" 20	105605					
Mean, with anthracite........			130917				159878	
Mean of all			126699				151577	

11. Pounds of water evaporated during the week while the dampers were closed; nights..............	A	July 16	15435	G	Feb. 4	13470
	B	" 23	18815	H	" 11	10100
	C	" 30	14673	I	May 20	4800
	D	Aug. 6	15361			
	E	" 13	16324			
	F	" 20	15984			
Mean, for anthracite..........			16122			9135
Mean of all..................			16099			9457
12. Total number of pounds of water evaporated during the entire week; days and nights...........	A	July 16	140334	G	Feb. 4	180542
	B	" 23	145502	H	" 11	145076
	C	" 30	159907	I	May 20	157483
	D	Aug. 6	153283			
	E	" 13	136167			
	F	" 20	121590			
Mean, for anthracite..........			147039			169013
Mean of all..................			142797			161034
13. Number of British thermal units apparently imparted to the water during the week, without correction for entrained water or superheating; days and nights..........	A	July 16	158 778 610	G	Feb. 4	212 652 655
	B	" 23	164 542 800	H	" 11	169 278 881
	C	" 30	181 224 054	I	May 20	181 676 279
	D	Aug. 6	173 745 190			
	E	" 13	154 147 537			
	F	" 20	137 580 746			
Mean, for anthracite..........			166 487 638			197 164 467
Mean of all..................			161 669 823			187 869 272
14. Number of British thermal units apparently imparted to the water during the week, while the damper was open, without correction for entrained water or superheating; days....................	A	July 16	141 377 751	G	Feb. 4	196 742 777
	B	" 23	143 231 922	H	" 11	157 362 369
	C	" 30	164 570 872	I	May 20	176 082 479
	D	Aug. 6	156 331 366			
	E	" 13	135 654 432			
	F	" 20	119 461 303			
Mean, for anthracite..........			148 233 269			186 412 628
Mean of all..................			143 437 941			176 729 208

TABLE XI.—CONDENSED RECORD OF WEEKLY EXPERIMENTS.—*Continued.*

KIND OF QUANTITY.	PACIFIC BOILER.			WARM-BLAST BOILER.		
	For the week 1881.	ending,	QUANTITIES.	For the week 1882.	ending,	QUANTITIES.
15. Correction, in British thermal units, for entrained water, = 1.04 per cent., except for weeks G & H, when the steam was superheated; and except all nights, when there was neither	A	July 16	1 470 329	G	Feb. 4	+ 1 399 078
	B	" 23	1 489 612	H	" 11	+ 906 333
	C	" 30	1 711 537	I	May 20	− 1 831 258
	D	Aug. 6	1 625 846			
	E	" 13	1 410 806			
	F	" 20	1 242 398			
Mean, for anthracite			1 541 626			− 216 090
Mean of all			1 491 755			+ 158 051
16. Total number of British thermal units imparted to the water during the week while the dampers were open; corrected for entrained water and for superheating; days	A	July 16	139 907 422	G	Feb. 4	198 141 855
	B	" 23	141 742 310	H	" 11	158 268 702
	C	" 30	162 859 335	I	May 20	174 251 221
	D	Aug. 6	154 705 520			
	E	" 13	134 243 626			
	F	" 20	118 218 905			
Mean, for anthracite			146 691 643			186 196 538
Mean of all			141 946 186			176 887 259
17. Total number of British thermal units imparted to the water during the week while the dampers were closed; no entrained water, and no superheating; nights	A	July 16	17 400 859	G	Feb. 4	15 909 878
	B	" 23	21 310 878	H	" 11	11 916 512
	C	" 30	16 653 182	I	May 20	5 593 800
	D	Aug. 6	17 413 824			
	E	" 13	18 493 105			
	F	" 20	18 119 443			
Mean, for anthracite			18 254 376			10 751 839
Mean of all			18 231 882			11 140 063

18. Total number of British thermal units imparted to the water during the entire week, corrected for entrained water and superheating; days and nights	A	July 16	157 308 281		G	Feb. 4	214 051 733	
	B	" 23	163 053 188		H	" 11	170 185 214	
	C	" 30	179 512 517		I	May 20	179 845 021	
	D	Aug. 6	172 119 344					
	E	" 13	152 736 731					
	F	" 20	136 338 348					
Mean, for anthracite			164 946 012				196 948 377	
Mean of all			160 178 068				188 027 323	
19. Equivalent evaporation from and at 212° F.; pounds of water per pound of coal, corrected for ashes and residue, for wood, and for entrained water and superheating; days and nights	A	July 16	10.35		G	Feb. 4	10.81	
	B	" 23	10.61		H	" 11	11.54	
	C	" 30	10.55		I	May 20	11.12	
	D	Aug. 6	10.26					
	E	" 13	10.78					
	F	" 20	10.95					
Mean, for anthracite			10.51				11.03	
Mean of all			10.58				11.20	
20. Equivalent evaporation from and at 212° F.; pounds of water per pound of coal burned; days and nights; days, left-hand column; nights, right-hand column			Days.	Nights.			Days.	Nights.
	A	July 16	10.85		G	Feb. 4	11.71	5.79
	B	" 23	10.80	7.67	H	" 11	12.04	11.41
	C	" 30	10.86	7.47	I	May 20	11.94	10.32
	D	Aug. 6	10.82	7.55				
	E	" 13	10.92	7.84				
	F	" 20	10.92	10.50				
Mean, for anthracite			10.85	7.63			11.83	8.06
Mean of all			10.86	8.21			11.90	9.17
21. Equivalent evaporation from and at 212° F.; days only, per pound of coal consumed days and nights. Weeks F & H are bituminous; all others anthracite. Pounds of water per pound of coal	A	July 16	9.21		G	Feb. 4	10.00	
	B	" 23	9.22		H	" 11	10.72	
	C	" 30	9.58		I	May 20	10.77	
	D	Aug. 6	9.23					
	E	" 13	9.47					
	F	" 20	9.22					
Mean, for anthracite			9.34				10.39	
Mean of all			9.32				10.50	

TABLE XI.—CONDENSED RECORD OF WEEKLY EXPERIMENTS.—*Continued.*

KIND OF QUANTITY.	PACIFIC BOILER.				WARM-BLAST BOILER.			
	For the week		QUANTITIES.		For the week		QUANTITIES.	
	1881.	ending,			1882.	ending,		
22. Number of pounds of water that one pound of coal, by analysis, would evaporate from and at 212° F., if there were no loss of heat: weeks F & H bituminous, all the rest anthracite	A	July 16	13.55		G	Feb. 4	13.45	
	B	" 23	13.50		H	" 11	14.30	
	C	" 30	13.58		I	May 20	13.61	
	D	Aug. 6	13.55					
	E	" 13	13.64					
	F	" 20	14.27					
Mean, for anthracite			13.56				13.53	
Mean of all			13.68				13.79	
			Per cent.				Per cent.	
23. Ratio of heat utilized to the full heating power of the coal: per cent; days and nights; weeks F & H bituminous, all the rest anthracite.	A	July 16	76.38		G	Feb. 4	80.37	
	B	" 23	78.60		H	" 11	80.70	
	C	" 30	77.69		I	May 20	81.74	
	D	Aug. 6	75.72					
	E	" 13	79.03					
	F	" 20	76.73					
Mean, for anthracite			77.48				81.06	
Mean of all			77.36				80.94	
			Days.	Nights.			Days.	Nights.
24. Ratio of heat utilized to the full heating power of the coal consumed, days and nights separately: weeks F & H bituminous; days, left-hand column; nights, right-hand column	A	July 16	80.11		G	Feb. 4	87.05	43.05
	B	" 23	80.06	56.82	H	" 11	84.21	79.79
	C	" 30	80.03	54.99	I	May 20	87.76	75.80
	D	Aug. 6	79.85	55.71				
	E	" 13	79.74	57.48				
	F	" 20	76.53	73.60				
Mean, for anthracite			79.96	56.25			87.41	59.43
Mean of all			79.39	59.72			86.34	66.21

25. Ratio of heat utilized during days only, to the full heating power of the coal consumed during days and nights; steam, days only; coal, burned days and nights			Per cent.				Per cent.	
	A	July 16	67.97		G	Feb. 4	74.35	
	B	" 23	68.30		H	" 11	74.96	
	C	" 30	70.55		I	May 20	79.20	
	D	Aug. 6	68.12					
	E	" 13	69.43					
	F	" 20	64.61					
Mean, for anthracite..........			68.87				76.78	
Mean of all			68 16				76.17	
26. Mean temperature of the smoke-box, deg. F., quarter-hourly observations; days, left-hand column; nights, right-hand column ..			Days.	Nights.			Days.	Nights.
	A	July 16	384.9°		G	Feb. 4	396.9°	(Not recorded.)
	B	" 23	372.9°	250.0°	H	" 11	397.4°	
	C	" 30	371.1°	257.1°	I	May 20	377.0°	
	D	Aug. 6	364.8°	247.8°				
	E	" 13	348.0°	249.8°				
	F	" 20	376.9°	252.5°				
Mean, for anthracite..........			368.3°	251.0°			387.0°	——
Mean of all			369.8°	251.4°			390.4°	
27. Mean temperature of the boiler-room, deg. F., quarter-hourly observations; days, left-hand column; nights, right-hand column ..			Days.	Nights.			Days.	Nights.
	A	July 16	85.2°		G	Feb. 4	68.7°	(Not recorded.)
	B	" 23	75.1°	72.1°	H	" 11	72.2°	
	C	" 30	76.0°	75.5°	I	May 20		
	D	Aug. 6	81.4°	79.6°				
	E	" 13	79.1°	75.6°				
	F	" 20	76.2°	73.3°				
Mean, for anthracite..........			79.4°	75.7°			——	——
Mean of all			78.8°	75.2°				
28. Mean temperature of external air, deg. F., quarter-hourly observations; days, left-hand column; nights, right-hand column			Days.	Nights.			Days.	Nights.
	A	July 16	81.0°		G	Feb. 4	34.0°	(Not recorded.)
	B	" 23	76.1°	66.0°	H	" 11	34.2°	
	C	" 30	74.9°	67.7°	I	May 20	49.0°	
	D	Aug. 6	82.3°	76.8°				
	E	" 13	77.0°	73.1°				
	F	" 20	71.0°	64.9°				
Mean, for anthracite....			78.3°	70.9°			41.5°	——
Mean of all			77.1°	69.7°			39.1°	

TABLE XI.—CONDENSED RECORD OF WEEKLY EXPERIMENTS.—*Continued.*

KIND OF QUANTITY.	PACIFIC BOILER. For the week 1881.	ending,	QUANTITIES.		WARM-BLAST BOILER. For the week 1882.	ending,	QUANTITIES.	
			Days.	Nights.			Days.	Nights.
29. Number of pounds of air, by analysis of flue gases, per pound of coal consumed; days, left-hand column; nights, right-hand column..	A	July 16	20.04		G	Feb. 4	22.67	(Not ascertained.)
	B	" 23	21.26	34.63	H	" 11	27.55	
	C	" 30	18.92	33.80	I	May 20	23.34	
	D	Aug. 6	23.37	27.43				
	E	" 13	24.14	30.72				
	F	" 20	24.42	42.74				
Mean, for anthracite..........			21.55	31.65			23 00	——
Mean of all			22.02	33.86			24.52	
			Days.	Nights.				
30. Number of pounds of flue gases, by analysis, per pound of coal consumed; days, left-hand column; nights, right-hand column	A	July 16	20.86		G	Feb. 4	23.49	
	B	" 23	22.08	35.45	H	" 11	28.37	
	C	" 30	19.74	34.62	I	May 20	24.17	
	D	Aug. 6	24.19	28.25				
	E	" 13	25.06	31.54				
	F	" 20	25.23	43.55				
Mean, for anthracite..........			22.39	32.46			23.83	——
Mean of all			22.86	34.68			25.34	
			Inches Hg.	Lbs. per sq. in.			Inches Hg.	Lbs. per sq. in.
31. Mean weekly barometric pressure, corrected; inches of mercury in left-hand column; pounds per square inch in right-hand column.	A	July 16	29.438	14.41	G	Feb. 4	29.81	14.64
	B	" 23	29.492	14.48	H	" 11	29.86	14.66
	C	" 30	29.509	14.49	I	May 20	29.93	14.70
	D	Aug. 6	29.413	14.45				
	E	" 13	29.544	14.51				
	F	" 20	29.752	14.61				
n. for anthracite.........			29.479	14.47			29.87	14.67
Mean of all.....			29.525	14.49			29.87	14.67

			Days.	Nights.			Days.	Nights.
32. Mean ratio of heat lost at chimney to the full heating power of the coal; mean loss by radiation being deducted, per cent.; days, left hand column; nights, right-hand column	A	July 16			G	Feb. 4	8.95	52.95
	B	" 23	17.13	39.18	H	" 11	11.79	16.21
	C	" 30	17.16	41.01	I	May 20	8.24	20.20
	D	Aug. 6	17.34	40.29				
	E	" 13	17.45	38.52				
	F	" 20	20.66	22.40				
Mean, for anthracite			17.27	39.75			8.59	36.57
Mean of all			17.95	36.28			9.66	29.79
			Chimney.	Radiation.			Chimney.	Radiation.
33. Mean ratio of heat lost at chimney and by radiation, to the full heating power of the coal, per cent.; chimney, left-hand column; radiation, right-hand column	A	July 16			G	Feb. 4	15.63	4.00
	B	" 23	20.00	1.40	H	" 11	15.30	4.00
	C	" 30	19.21	3.10	I	May 20	14.26	4.00
	D	Aug. 6	19.53	4.75				
	E	" 13	19.58	1.39				
	F	" 20	19.27	3.39				
Mean, for anthracite			19.58	2.64			14.94	4.00
Mean of all			19.52	2.81			15.06	4.00
			Per cent.				Per cent.	
34. Mean ratio of sums of losses at chimney and by radiation, to the full heating power of the coal; complement of heat utilized (No. 23), per cent	A	July 16	22.66		G	Feb. 4	19.63	
	B	" 23	21.40		H	" 11	19.30	
	C	" 30	22.31		I	May 20	18.26	
	D	Aug. 6	24.28					
	E	" 13	20.97					
	F	" 20	23.27					
Mean, for anthracite			22.24				18 94	
Mean of all			22.32				19.06	
35. Number pounds of coal, corrected for wood and ashes, burned per week while the dampers were open; days	A	July 16	13352		G	Feb. 4	17522	
	B	" 23	13590		H	" 11	13612	
	C	" 30	15529		I	May 20	15112	
	D	Aug. 6	14806					
	E	" 13	12730					
	F	" 20	11210					
Mean, for anthracite			14001				16317	
Mean of all			13536				15415	

TABLE XI.—CONDENSED RECORD OF WEEKLY EXPERIMENTS.—*Continued.*

KIND OF QUANTITY.	PACIFIC BOILER.				WARM-BLAST BOILER.			
	For the week		QUANTITIES.		For the week		QUANTITIES.	
	1881.	ending,	Open.	Banked.	1882.	ending,	Open.	Banked.
36. Number of hours and minutes per week the dampers were open, left-hand column; number of hours and minutes per week the fires were banked, right-hand column........	A	July 16	65:15	60:19	G	Feb. 4	48:57	75:48
	B	" 23	67: 1	59:26	H	" 11	52:36	70:21
	C	" 30	72:20	54:25	I	May 20	64:22	60:51
	D	Aug. 6	69:17	57:58				
	E	" 13	66:27	57:30				
	F	" 20	63:55	61:29				
Mean, for anthracite...			68: 4	57:56			54:40	68:20
Mean of all			67:22	58:31			55:18	69: 0
37. Number of pounds burned per square foot of fire-grate area and per hour, weekly means	A	July 16	7.92		G	Feb. 4	12.99	
	B	" 23	7.85		H	" 11	9.39	
	C	" 30	8.31		I	May 20	9.09	
	D	Aug. 6	8.27					
	E	" 13	7.42					
	F	" 20	6.79					
Mean, for anthracite..			7.95				11.04	
Mean of all			7.76				10.49	

The line "Mean of all," in each section, has not much significance, especially in sections relating to fuel, but may be found convenient in a general way.

Table XII. is the result of duplicate analyses, with repetitions in cases where the duplicate results appeared to be too discrepant. The anthracites were remarkably uniform, as, indeed, were the bituminous samples. The marked character of each kind of coal will be noticed.

In Table XIII. the hygrometric observations were reduced by Guyot's tables, each by itself, and a mean was taken of the results.

Tables XIV. and XV. are the result of continuous duplicate analyses of the flue gases, through each forenoon, each afternoon (except Saturday P.M.), and each night. Bottled samples were also taken simultaneously, for verification of results in cases of too great discrepancy between the two simultaneous duplicates.

Observe that, in the middle division of these tables, the sums of the figures in lines 1, 2, make the quantities in line 3, and that these correspond to the first line in the upper division; the sums of the figures in lines 4, 5, make the quantities in line 6, corresponding to line 2, upper division; the sums of the figures in lines 7, 8, make the quantities in line 9, corresponding to the third line in the upper division; and that the sums of the figures in lines 2, 5, 9, 10, make the quantities in line 11. Finally, the figures in wide-face type, lines 3, 6, 9, 12, make 100.00.

In the lower division, the figures in lines 1, 2, those in lines 4, 5, and those in lines 6, 7, added together, make in each case 100.00. The quantities, or ratios, in line 8, are simply 100 times the quotient of the numbers in line 7, divided by those in line 6.

In line 10, the O combined with hydrogen in the coal, disappears in desiccating the gases, and does not appear in the dry gases.

All necessary details concerning the manner of arriving at the several values inserted in these tables, will be found in the sequel.

TABLE XII.—CONDENSED RECORD OF WEEKLY EXPERIMENTS.

ANALYSES OF COALS FOR THE WEEKS DESIGNATED,		PACIFIC BOILER.							WARM-BLAST BOILER.			
		A	B	C	D	E	F	1881. Anthracite. Mean.	G	H	I	1882. Anthracite. Mean.
Ending,		July 16.	July 23.	July 30.	Aug. 6.	Aug. 13.	Aug. 20.		Feb. 4.	Feb. 11,	May 20.	
Hydrogen	H,	1.84	1.87	1.85	1.87	1.85	3.84	1.86	1.89	3.79	1.80	1.84
Carbon	C,	82.41	81.90	82.64	82.24	82.96	81.03	82.43	81.51	81.71	82.92	82.22
Water	H_2O,	3.40	3.25	2.37	2.55	2.32	0.63	2.78	2.49	1.02	2.39	2.44
Ash		10.12	10.03	10.11	10.36	9.99	7.19	10.12	11.83	5.75	9.76	10.80
Sulphur	S,						0.82			0.82		
Oxygen	O,	2.23	2.95	3.03	2.98	2.88	4.49	2.81	2.28	4.91	3.13	2.70
Nitrogen	N,						2.00			2.00		
		100.00	100.00	100.00	100.00	100.00	100.00	100.00	100.00	100.00	100.00	100.00

NOTE.—S, O and N are not separately determined in anthracite.

TABLE XIII.—ATMOSPHERIC AIR.

Grains per cubic foot, O and N	491.4	494.6	499.0	484.0	497.8	506.3	Mean. 495.5	565.0	553.0		Mean. 559.0
Grains per cubic foot, Water	6.5	5.7	6.6	7.6	5.7	5.5	6.3	2.09	2.00		2.04
Ratio of water to air; per cent	1.28	1.15	1.30	1.54	1.15	1.07	1.25	0.37	0.36		0.37

TABLE XIV.—CONDENSED RECORD OF WEEKLY EXPERIMENTS.

ANALYSES OF DRY FLUE GASES WHILE THE DAMPERS WERE OPEN; FOR THE WEEKS DESIGNATED.		PACIFIC BOILER.							WARM-BLAST BOILER.			
		A	B	C	D	E	F	1881. Anthracite, Mean.	G	H	I	1882. Anthracite, Mean.
Days,	Ending,	July 16.	July 23.	July 30.	Aug. 6.	Aug. 13.	Aug. 20.		Feb. 4.	Feb. 11.	May 20.	
1. Carbon dioxide	CO_2,	14.00	13.19	14.38	12.13	11.82	11.22	13.10	12.61	10.39	12.27	12.44
2. Carbon monoxide	CO,	.30	.25	.51	.20	.22	.36	.30	.08	.12	.18	.13
3. Oxygen uncombined	O,	11.20	11.89	10.73	12.81	13.06	12.96	11.94	12.45	13.89	12.73	12.59
4. Nitrogen	N,	74.50	74.67	74.38	74.86	74.90	75.46	74.66	74.86	75.60	74.82	74.84
Total		100.00	100.00	100.00	100.00	100.00	100.00	100.00	100.00	100.00	100.00	100.00
1. Carbon combined in CO_2	C,	3.82	3.60	3.92	3.31	3.22	3.06	3.57	3.44	2.83	3.35	3.40
2. Oxygen combined in CO_2	O,	10.18	9.59	10.46	8.82	8.60	8.16	9.53	9.17	7.56	8.92	9.04
3. Carbon dioxide	CO_2,	**14.00**	**13.19**	**14.38**	**12.13**	**11.82**	**11.22**	**13.10**	**12.61**	**10.39**	**12.27**	**12.44**
4. Carbon combined in CO	C,	.13	.11	.22	.09	.10	.15	.13	.03	.05	.08	.06
5. Oxygen combined in CO	O,	.17	.14	.29	.11	.12	.21	.17	.05	.07	.10	.07
6. Carbon monoxide	CO,	**.30**	**.25**	**.51**	**.20**	**.22**	**.36**	**.30**	**.08**	**.12**	**.18**	**.13**
7. O free, but required by CO	O,	.18	.14	.29	.12	.13	.20	.17	.05	.07	.10	.08
8. Oxygen surplus	O,	11.02	11.75	10.44	12.69	12.93	12.76	11.76	12.40	13.82	12.62	12.51
9. Oxygen uncombined	O,	**11.20**	**11.89**	**10.73**	**12.81**	**13.06**	**12.96**	**11.93**	**12.45**	**13.89**	**12.73**	**12.59**
10. O combined with hydrogen	O,	.70	.68	.74	.62	.59	1.22	.67	.69	1.07	.59	.64
11. O total, combined and free.	O,	22.43	22.30	22.22	22.36	22.37	22.54	22.30	22.35	22.58	22.37	22.36
12. Nitrogen = (77 + 23) O	N,	**74.50**	**74.67**	**74.38**	**74.86**	**74.90**	**75.46**	**74.66**	**74.86**	**75.60**	**74.82**	**74.84**
Total		100.00	100.00	100.00	100.00	100.00	100.00	100.00	100.00	100 00	100.00	100.00
1. Proportion of C in the CO_2.	%	96.71	97.04	94.69	97.35	96.99	95.33	96.56	99.14	98.22	97.67	98.40
2. Proportion of C in the CO.	%	3.29	2.96	5.31	2.65	3.01	4.67	3.44	.86	1.78	2.33	1.60
3. Loss of heat by CO	%	2.07	1.83	3.36	1.60	1.81	2.85	2.13	0.63	1.06	1.43	1.03
4. Proportion of O combined.	%	49.66	46.68	51.71	42.71	41.62	42.50	46.48	44.30	38.53	43.00	43.65
5. Proportion of O free	%	50.34	53.32	48.29	57.29	58.38	57.50	53.52	55.70	61.47	57.00	56.35
6. Proportion of O required	%	50.47	47.31	53.02	43.25	42.20	43.43	47.25	44.52	38.81	43.45	43.98
7. Proportion of O surplus	%	49.53	52.69	46.98	56.75	57.80	56.57	52.75	55.48	61.19	56.55	56.02
8. Ratio of O surpl's to O req'd	%	98.13	111.38	88.62	131.23	136.97	130.24	113.27	124.61	158.46	130.14	127.36

TABLE XV.—CONDENSED RECORD OF WEEKLY EXPERIMENTS.

ANALYSES OF DRY FLUE GASES WHILE THE DAMPERS WERE CLOSED; FOR THE WEEKS DESIGNATED.		PACIFIC BOILER.							WARM-BLAST BOILER.			
		A	B	C	D	E	F	1881. Anthracite, Mean.	G	H	I	1882. Anthracite, Mean.
Nights,	Ending,	July 16.	July 23.	July 30.	Aug. 6.	Aug. 13.	Aug. 20.		Feb. 4.	Feb. 11.	May 20.	
1. Carbon dioxide	CO_2,	(Not recorded.)	5.09	4.99	5.69	5.62	6.23	5.35	(Not recorded.)	(Not recorded.)	(Not recorded.)	
2. Carbon monoxide	CO,		2.15	2.38	3.18	2.57	.37	2.57				
3. Oxygen uncombined	O,		17.21	17.21	16.06	16.48	17.29	16.74				
4. Nitrogen	N,		75.55	75.42	75.07	75.33	76.11	75.34				
Total			100.00	100.00	100.00	100.00	100.00	100.00				
1. Carbon combined in CO_2	C,		1.39	1.36	1.55	1.53	1.70	1.46				
2. Oxygen combined in CO_2	O,		3.70	3.63	4.14	4.09	4.53	3.89				
3. Carbon dioxide	CO_2,		**5.09**	**4.99**	**5.69**	**5.62**	**6.23**	**5.35**				
4. Carbon combined in CO	C,		.92	1.02	1.36	1.10	.16	1.10				
5. Oxygen combined in CO	O,		1.23	1.36	1.82	1.47	.21	1.47				
6. Carbon monoxide	CO,		**2.15**	**2.38**	**3.18**	**2.57**	**.37**	**2.57**				
7. O free, but required by CO.	O,		1.23	1.36	1.82	1.47	.21	1.47				
8. Oxygen surplus	O,		15.98	15.85	14.24	15.01	17.08	15.27				
9. Oxygen uncombined	O,		**17.21**	**17.21**	**16.06**	**16.48**	**17.29**	**16.74**				
10. O combined with hydrogen	O,		.42	.34	.40	.47	.70	.41				
11. O total, combined and free.	O,		22.57	22.53	22.42	22.50	22.73	22.50				
12. Nitrogen (77 ÷ 23) O	N,		**75.55**	**75.42**	**75.07**	**75.33**	**76.11**	**75.34**				
Total			100.00	100.00	100.00	100.00	100.00	100.00				
1. Proportion of C in CO_2	%		60.17	57.16	53.24	58.19	91.46	57.19				
2. Proportion of C in CO	%		39.83	42.84	46.76	41.81	8.54	42.81				
3. Loss of heat by CO.	%		25.37	27.27	29.68	26.50	5.07	27.20				
4. Proportion of O combined.	%		23.72	23.61	28.35	26.78	23.93	25.61				
5. Proportion of O free	%		76.28	76.39	71.65	73.22	76.07	74.39				
6. Proportion of O required	%		29.17	29.67	36.46	33.29	24.86	32.15				
7. Proportion of O surplus	%		70.83	70.33	63.54	66.71	75.14	67.85				
8. Ratio of O surpl's to O req'd	%		241.34	237.28	174.28	200.40	302.30	213.32				

PYROMETRIC MEASUREMENTS OF TEMPERATURES.—All temperatures ascertained by the use of the water-platinum pyrometer, heretofore described, are embodied in the following tables, and these tables, in turn, are graphically represented in the accompanying diagrams. Temperatures were taken at both boilers, but the greater number, probably, at Warm-Blast Boiler No. 1, since special provision was made in the setting of that boiler for convenient use of the pyrometer. The high temperatures in Table XVI. were taken at Warm-Blast Boiler No. 2 (which was "Pacific Boiler" remodeled to warm blast), more than two months after the close of the last weekly experiment, ending May 20, 1882. My assistants went to Lawrence on a morning train, took matters just as they found them in the regular daily use of the boiler, and obtained the results in this table—partly melting the platinum balls in experiment 6—probably the result of some slight impurity in the platinum. I have used, for the most part, the temperatures obtained by the first (and simplest) method of reducing the pyrometric observations to degrees F., partly because that method gives a result a little too high, in most cases—not more than 1 per cent. too high—and we are sure that the heat-carrier can never be hotter than the flame or other source of heat to be measured, and may be a little cooler.

TABLE XVI.

PYROMETRIC OBSERVATIONS OF TEMPERATURES AT WARM-BLAST BOILER NO. 2, JULY 28, 1882. OBSERVATIONS NOS. 1, 2, 3 AND 4, AT BRIDGE WALL. NOS. 5 AND 6 IN THE HEART OF THE FIRE.

NO. OF OBS.	Temperature of water in pyrometer, 2.1053 lbs.	Number of British thermal units in water above 0° F.	HEAT-CARRIER.		Observed loss of temperature by heat-carrier at assumed ratio of sp. ht. for Pt, 30 to 1, for Fe, 6 to 1. See Table VI.	True loss of temperature and true temperature of heat-carrier when taken from the fire.
			Kind of Metal.	Ratio of water to heat carrier.		
1	2	3	4	5	6	7
1	96.65 81.20	96.71995 81.23740	Pt.	105.265	1629.8	1488.3 96.7
	15.45	15.48255				1585.0
2	99.3 83.4	99.3779 84.4418	Pt.	105.265	1677.5	1526.9 99.3
	15.9	15.9361				1626.2
3	102.51 85.40	102.59753 85.44580	Pt & Fe.	105.265	1805.5	1496.8 102.5
	17.11	17.15173				1599.3
4	103.02 86.16	103.10906 86.20732	Pt & Fe.	105.265	1779.16	1483.1 103.0
	16.86	16.90174				1586.1
5	110.30 81.54	110.41090 81.57808	Pt.	105.265	3035.1	2546.0 110.3
	28.76	28 83282				2656.3
6	113. 81.	113.121 81.037	Pt.	107.7	3455.4	2835.2 113.0
	32.	32.084				2948.2

$$\text{Mean of 1 and 2} = \frac{1585.0 + 1626.2}{2} = 1605.6.$$

$$\text{Mean of 3 and 4} = \frac{1599.3 + 1586.1}{2} = 1592.7.$$

$$\text{Mean of 1, 2, 3, 4} = \frac{1605.6 + 1592.7}{2} = 1599.15.$$

$$\text{Mean of 5 and 6} = \frac{2656.3 + 2948.2}{2} = 2802.25.$$

About one-sixth of the platinum was fused in observation 6, and cooled in drops, like shot; and one drop adhered to the lip of the pyrometer, and did not enter the water at all—a circumstance which raised the "ratio" to 107.7.

TABLE XVII.

TEMPERATURES DEDUCED FROM PYROMETRIC OBSERVATIONS IN TABLE XVI., BY THE SECOND AND THIRD METHODS, AS DESCRIBED ON p. 720. THE THIRD METHOD IS A LITTLE THE MOST ACCURATE.

No. of obs.	Second method.		Third method.				
	Observed loss, plus final temperature of heat-carrier.	True temperatures by second method, deg. Fahr.	Final temperatures minus 32° F. in deg. F.	Ratio pyr. to Fahr. deg.	Final temp. minus 32° F., reduced to pyrometer deg.	Observed loss, plus final t. above 32° in pyrometer deg.	True temperatures by third method, deg. Fahr.
1	2	3	4	5	6	7	8
1	1629.80		96.65			1629.80	1538.9
	96.65		32.			62.65	32.0
	1726.45	1566.3	64.65	× .969	= 62.65	1692.45	1570.9
2	1677.50		99.3			1677.50	1579.3
	99.30		32.			65.21	32.0
	1776.80	1606.7	67.3	× .969	= 65.21	1742.71	1611.3
3	1805.50		102.51			1805.50	1530.1
	102.51		32.			68.32	32.0
	1908.01	1546.6	70.51	× .969	= 68.32	1873.82	1562.1
4	1779.60		103.02			1779.16	1517.6
	103.02		32.			68.82	32.0
	1882.62	1534.1	71.02	× .969	= 68.82	1847.98	1549.6
5	3035.10		110.30			3035.10	2599.1
	110.30		32.			75.87	32.0
	3145.40	2623.0	78.30	× .969	= 75.87	3110.97	2631.1
6	3455.40		113.0			3455.40	2888.1
	113.00		32.0			78.49	32.
	3568.40	2911.3	81.0	× .969	= 78.49	3533.89	2920.1

The "true temperatures," in columns 3 and 8, are found by the use of Table VI. (except Nos. 5 and 6, which go too high for Table VI., and are obtained from Table IV.), in the manner explained on p. 711. Observe that in Nos. 1, 2, 4 and 6, the platinum heat-carrier was used; and Nos. 3 and 4, the compound, Pt. Fe, heat-carrier. The three methods do not give results very discrepant. The first method gives temperatures a little too high; the second a little too low; the third, usually a little nearer correct. The greatest differences occur with the Pt, Fe heat-carrier.

TABLE XIX.

TEMPERATURES AT BRIDGE WALL, ASCERTAINED BY THE USE OF THE WATER-PLATINUM PYROMETER. DEGREES FAHRENHEIT.

DATE 1881.	TIME. h.	TIME. m.		Temperature, deg. F.	DATE 1881.	TIME. h.	TIME. m.		Temperature, deg. F.	DATE 1881.	TIME. h.	TIME. m.		Temperature, deg. F.
July 8	10	30	A.M.	787	July 11	11	30	A.M.	1431	July 14	4	40	P.M.	1445
	10	30		808		12	31	P.M.	1536		4	50		1419
	11	40		1097		12	31		1427		5			1279
	11	40		1153	12	4	35		1363		5	10		1332
9	9	50		1095		4	45		1381		5	20		1262
	9	50		991		4	55		1251		5	20		1056
	11			735		5	15		1249	15	11	35	A.M.	993
	11			770		5	30		1185		11	50		883
	11	11		985	13	3	20		1339		12	5	P.M.	915
	11	11		953		3	30		1266		3	40		1327
	11	11		1018		3	40		1322		4			1258
	11	11		1045		3	50		1377		4	15		1017
	12		M.	1112		3	55		1236		4	25		728
	12			1129		4	7		1222		4	40		799
	12			1023		4	15		1186		5			894
	12			1105		4	25		1154		5	25		862
	1	45	P.M.	1342	14	9	20	A.M.	1056		5	45		741
	1	45		1345		9	30		1056		5	45		653
	1	45		1322		9	40		1026	18	10	5	A.M.	1216
	1	45		1296		9	50		1205		10	25		1406
	4			1386		10			1259		10	45		1376
	4			1382		10	10		1172		11	15		1296
	4			1324		10	20		1208		11	50		1474
	4			1305		11	20		1472	19	1	25	A.M.	*526
	4	55		894		11	35		1239		1	25		535
	4	55		974		11	45		1320		10	20		1260
	5	55		1024		11	55		1329		11	5		1381
	5	55		1050		12	5	P.M.	1418		11	20		1305
*	8	30	A.M.	1431		12	15		1259		11	35		1222
*	8	30		1310		2	25		1438	21	12	30	A.M.	*653
*	8	30		1303		3	45		1447		12	30		*674
*	8	30		1366		3	55		*1611	22	12	30	A.M.	*537
11	10	30		1409		4	5		1404		12	30		*556
	10	30		1479		4	20		1289	23	1	30	A.M.	*537
	11	30		1557		4	30		1318		1	30		*556

* Date not recorded. * Perhaps 100° too high. * Fires banked.

This table is represented graphically, as a profile in Fig. 173, the temperatures being represented as ordinates at equal distances, but in the same order as in this table. The temperatures for July 14 are represented graphically in Fig. 174, with the ordinates properly spaced to represent the respective times at which they were taken.

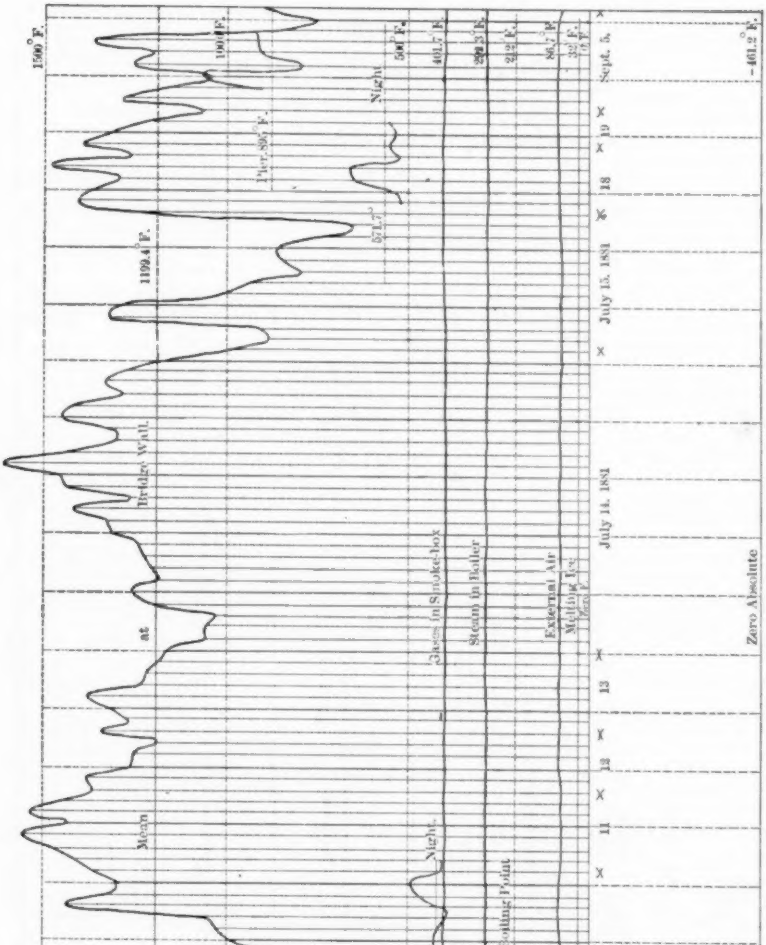


FIG. 173.—GRAPHIC REPRESENTATION OF TABLE XIX., P. 754.

Fig. 174.

GRAPHICAL REPRESENTATION OF PART OF TABLE XIX.

PACIFIC BOILER: JULY 14. 1881.

SCALES: HORIZONTAL ⅝ IN.=1 HOUR: VERTICAL 500°=1 INCH.

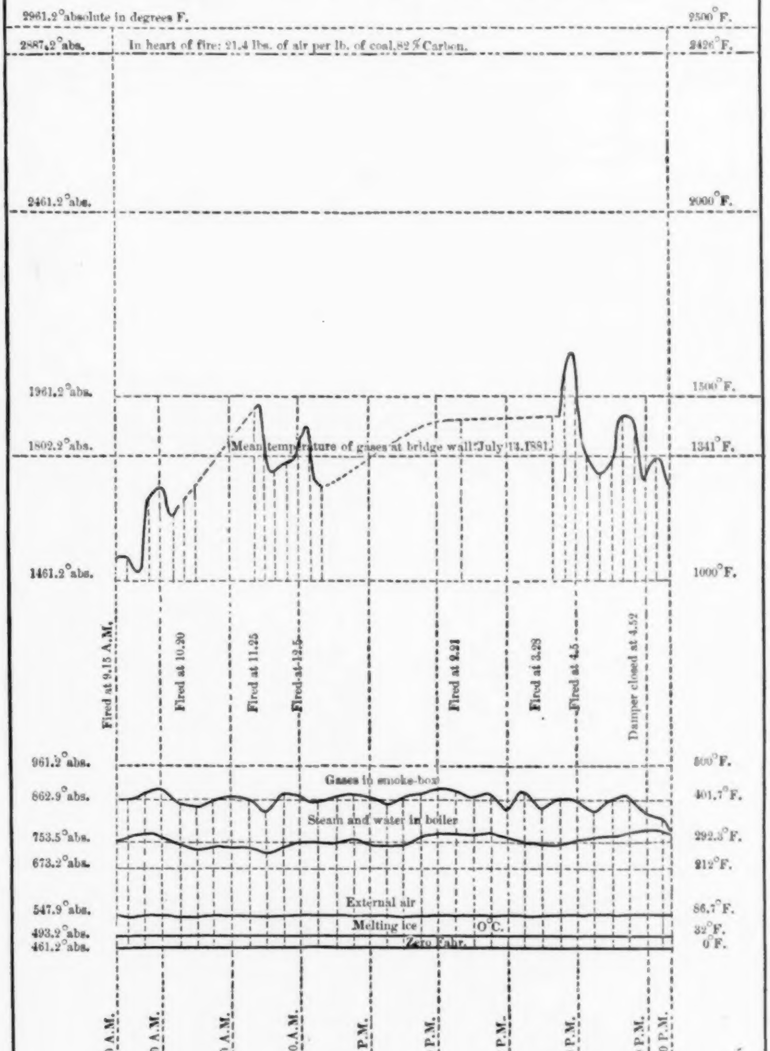


TABLE XX.

PYROMETRIC MEASUREMENTS OF TEMPERATURE IN ARCH OVER WARM-BLAST BOILER NO. 1; FEBRUARY 13, 1882.

TIME. h. m. Part of Day.	TEMPERATURE OF WATER. At immersion of heat-carrier.	After cooling of heat-carrier.	INCREASE OF HEAT. In degrees F.	In British thermal units.	HEAT-CARRIER. Kind of metal.	Ratio assumed of water to heat-carrier.	RESULTING TEMPERATURES. Lost by heat carrier in cooling.	Temperatures sought in degrees Fahr.
1	2	3	4	5	6	7	8	9
8:22 A.M.	71.3	77.7	6.4	6.4098	Pt.	100	630.2	707.9
8:45	77.6	83.8	6.2	6.2114	Pt.	100	611.8	695.6
2: 0 P.M.	77.22	83.17	5.95	5.9609	Pt.	100	588.4	671.6
2:34	82.8	87.8	5.0	5.0100	Pt.	100	498.8	586.6
3: 3	86.9	93.6	6.7	6.7134	Pt.	100	658.4	752.0
3:33	92.6	99.4	6.8	6.8180	Pt.	100	668.1	767.5
4:27	80.6	89.0	8.4	8.4164	Pt.	100	811.2	903.2

The highest observed temperature of superheated steam in the boiler, was 344° F., and the highest temperature of the iron must have been about midway, say, $\frac{344^\circ + 903^\circ}{2} = 624^\circ$, or perhaps a little higher.

TABLE XXI.

COMPARISON OF TEMPERATURES FOUND WITH PACIFIC AND WARM-BLAST BOILERS.

LOCATION OF TEMPERATURES.	TEMPERATURES: DEGREES FAHR. Pacific Boiler.	Warm-Blast Boiler.	Difference.
In heart of fire	2426°	2796°	370°
At bridge wall	1341°	1599°	258°
At pier		895°	
In smoke-box	368°	377°	9°
Air admitted to furnace	78°	334°	256°
Steam and water in boiler	292°	300°	8°
Gases escaping to chimney	368°	164°	204°
External air	78°	34°	44°
Gases cooled, Warm-Blast Boiler			213°
Air warmed, Warm-Blast Boiler			300°

Before any just or useful comparison can be instituted between the several figures in Table XXI. it will be necessary, or at least convenient, to reduce them all to a common basis—1° temperature

of external air. This will affect many of the other figures. For simplicity and convenience, we will reduce this, in both cases, to 0° C., = 32° F., which will reduce the temperature in the case of the Pacific Boiler, 78° − 32° = 46°, and in the case of the Warm-Blast Boiler, 34° − 32° = 2° F. A corresponding reduction would result in the temperature of the fire; but here another equalization is required.

The temperature of the heart of the fire is affected chiefly by two causes, namely: *First*, the quantity of air passing through the fire per pound of coal burned; and, *second*, the temperature of this air. For the latter, we merely subtract, as above mentioned, 46° from the temperature in the case of the Pacific Boiler, and 2426° − 46° = 2380°; and in the case of the Warm-Blast Boiler, 2796° − 2° = 2794°.

But these temperatures were found in different quantities of air: 21.28 pounds of air per pound of coal, in the former case, and in the latter, 20.36 pounds. Taking this last quantity in both cases, and assuming the anthracite coal to be in such a state of ignition that the hydrogen it may have contained has all been consumed, and neglecting the moisture in the air, we have, 0.238 being the specific heat of air, and also the gases of combustion; 0.82 the proportion of carbon in the coal, and 14,544 B. t. u. the full heating power of 1 pound of carbon:

$$\frac{14544 \times 0.82}{20.36 \times .238} = \frac{11926.08}{4.84568} = 2461$$

This will be the increment of heat, in degrees F., in passing through the fire in both cases; to be added in the one case to 32°, and in the other case to 332°, the difference, 300°, being due to heat derived, in the abstractor, from the outflowing gases, in the Warm-Blast Boiler. Then:

Pacific Boiler	2461° + 32° = 2493°
Warm-Blast Boiler.................... ...	2461° + 332° = 2793°
Difference..............................	2793° − 2493° = 300°

There will be less difference at the bridge wall, as the temperature tends to equalize itself with that of the boiler, and this tendency is the more rapid the greater the difference between the fire and hot gases on the one hand, and the boiler and its contents on the other. I arrive at the following mean temperatures, under equal conditions:

AT THE BRIDGE WALL.

Pacific Boiler	1340° F.
Warm-Blast Boiler	1600° F.
Difference, 1600° − 1340° =	260° F.

The temperature at the pier, we found 895° (Fig. 173), and the corresponding temperature for the warm blast is 1050°. We then have:

FLUE GASES:

At the pier, about to enter flues,

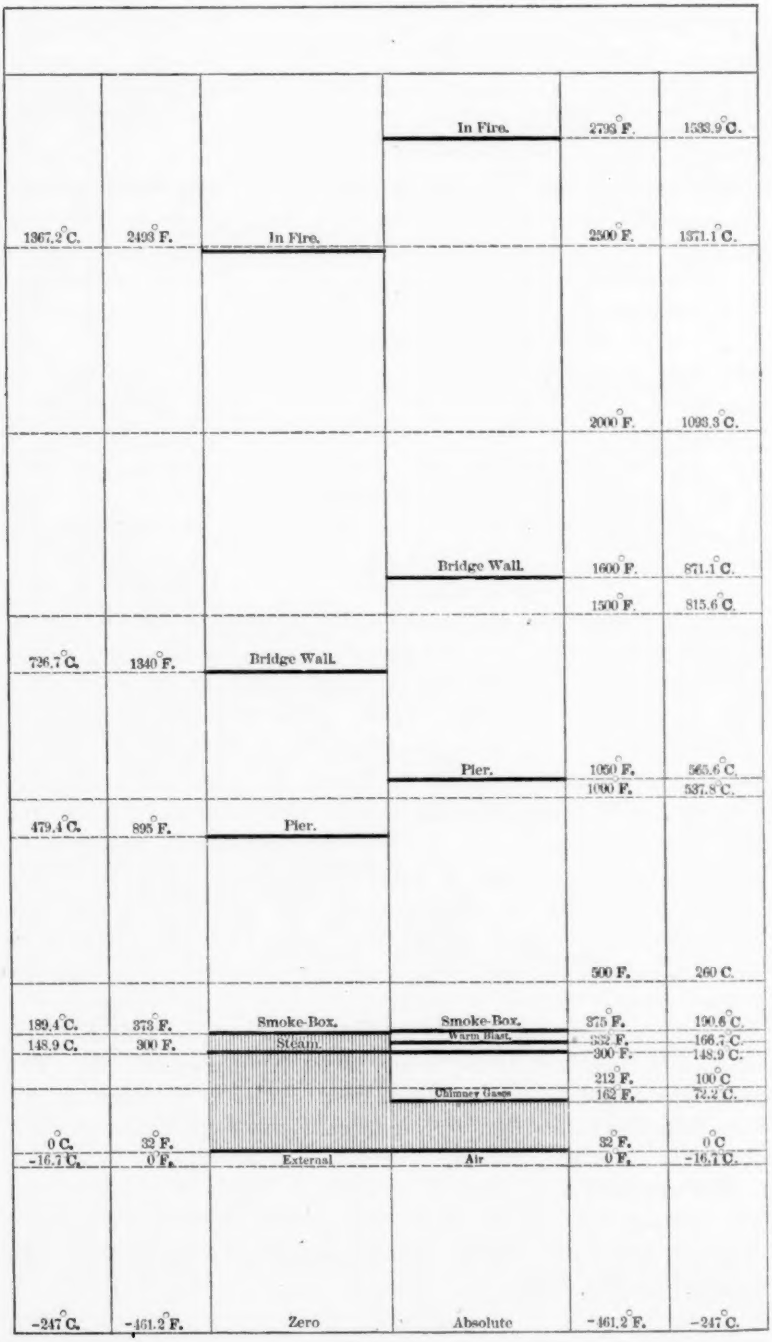
Warm-Blast Boiler	1050° F.
Pacific Boiler	895° F.
Difference, 1050° − 895° =	155° F.

Discharged to chimney,

Pacific Boiler	373° F.
Warm-Blast Boiler	162° F.
Difference, 373° − 162° =	221° F.

The temperature at the smoke-box will depend chiefly on that of the steam and water in the boiler; and that, in turn, depends in great measure on the rapidity with which steam is drawn off. We will assume the temperature of the steam to be 300° F., which is not very far from the mean, corresponding to 67.2 pounds pressure per square inch, absolute, and to about 52.5 pounds steam-gauge pressure. (The mean, for the Pacific Boiler, was 47.50, and for the Warm-Blast Boiler, 53.77.) The corresponding temperature in smoke-box, we have found to be 377° for external air at 34° (Table XXI), and for 32° we may properly call it 375° F. We have found the temperature in smoke-box, Pacific Boiler, to be 368° with 47.5 lbs. the square inch mean steam pressure, corresponding to a temperature of 285° in the boiler. Adding 5°, to bring it up to our assumed temperature, 300°, we have 368° + 5° = 373° F.; that of the Warm-Blast Boiler being, as we have seen, 375° F. The gases discharged from the Warm-Blast Boiler to the chimney, we have found to be at 164°, with external air at 34°, and we may call them, for air at 32°, 162° F.

We may now reconstruct our table, on a basis of equal temperature of external air, and throw its numbers into the form of a diagram, fairly representing the comparative temperatures in the two boilers (Fig. 175).



In Fire.
2793° F.
1533.9° C.
1367.2° C.
2493° F.
In Fire.
2500° F.
1371.1° C.
2000° F.
1093.3° C.
Bridge Wall.
1600° F.
871.1° C.
1500° F.
815.6° C.
726.7° C.
1340° F.
Bridge Wall.
Pier.
1050° F.
565.6° C.
1000° F.
537.8° C.
479.4° C.
895° F.
Pier.
500 F.
260 C.
189.4° C.
373° F.
Smoke-Box.
Smoke-Box.
375 F.
190.6° C.
148.9 C.
300 F.
Steam.
Warm Blast.
332 F.
166.7 C.
300 F.
148.9 C.
212 F.
100° C
Chimney Gases
162 F.
72.2 C.
0° C.
32° F.
32° F.
0° C
-16.7 C.
0° F.
External
Air
0 F.
-16.7 C.
-247° C.
-461.2° F.
Zero
Absolute
-461.2° F.
-247° C.

FIG. 175.

TABLE XXII.

COMPARATIVE TEMPERATURES—PACIFIC AND WARM-BLAST BOILERS UNDER EQUAL CONDITIONS; 20.36 POUNDS OF GASES OF COMBUSTION—IN THE FIRE—PER POUND OF ANTHRACITE COAL, 82 PER CENT. CARBON, COMPLETELY BURNED TO CO_2: EXTERNAL AIR AT 32° FAHR., STEAM PRESSURE, 52.5 POUNDS PER SQUARE INCH ABOVE THE ATMOSPHERE—TEMPERATURE OF STEAM, 300° F.

LOCATION OF TEMPERATURES.	TEMPERATURES: DEGREES FAHR.		
	Pacific Boiler.	Warm-Blast Boiler.	Difference.
In heart of fire	2493°	2793°	300°
At bridge wall	1340°	1600°	260°
At pier	895°	1050°	155°
In smoke-box	373°	375°	2°
Air admitted to furnace	32°	332°	300°
Steam and water in boiler	300°	300°	0°
Gases escaping to chimney	373°	162°	211°
External air	32°	32°	0°
Gases cooled, Warm-Blast Boiler			213°
Air warmed, Warm-Blast Boiler			300°

It will be observed that the air entering the furnace is warmed 300°, while the gases are cooled only 213°. This difference, or something like it, was constantly observed, and may be explained by two causes: *First*, the weight of the gases was about one-twentieth greater than that of the incoming air, by reason of the carbon carried off as CO_2, and (the specific heat of the gases and of air being sensibly alike—0.238), this circumstance alone would bring the cooling of the gases down from 300° to 285°; *second*, the whole mass of brick and iron composing the abstractors was kept at a pretty high temperature by conduction from the boiler setting.

This would tend, of course, to raise the mean between the outgoing gases and the incoming air; that is, to aid the warming of the air, and to retard the cooling of the gases. The mean temperature of the air in abstractor was (32° at entering, 332° at leaving), $\frac{32° + 332°}{2} = 187°$. The mean temperature of the gases in abstractor was (375° at entering, 162° at leaving), $\frac{375° + 162°}{2} = 268.5°$; and $268.5° - 187.0° = 81.5°$.

When the air enters at 32°, the gases are leaving at 162°; and $162° - 32° = 130°$.

When the air leaves, to enter the furnace, at 332°, the gases are entering from the smoke-box, at 375°, and $375° - 332° = 43°$. The mean $\frac{130° + 43°}{2} = 86.5°$, is about the difference to be ex-

pected between two fluids on opposite sides of iron plates, the one imparting heat to the other, at the rate of conduction necessary in steam boilers. It may, perhaps, be reduced to 75°, but it is probable that the enhanced cost of the apparatus would be out of proportion to the gain.

Table XXII. is graphically represented in Figs. 175 and 176. The former sufficiently explains itself, as the several temperatures in Table XXII. are merely located at their proper respective positions, according to the scale chosen.

The base line is the absolute zero of temperatures, 461.2° F. below zero Fahrenheit, equal to 274° C. below zero centigrade. The spaces shaded by heavy vertical lines, represent the respective quantities of heat carried off by the chimney.

Fig. 176 represents the same temperatures as they stand related to the surfaces of the shell and flues of the boiler, and to the flues of the abstractors, by means of which heat is withdrawn from the gaseous products of combustion, and imparted to the water in the boiler.

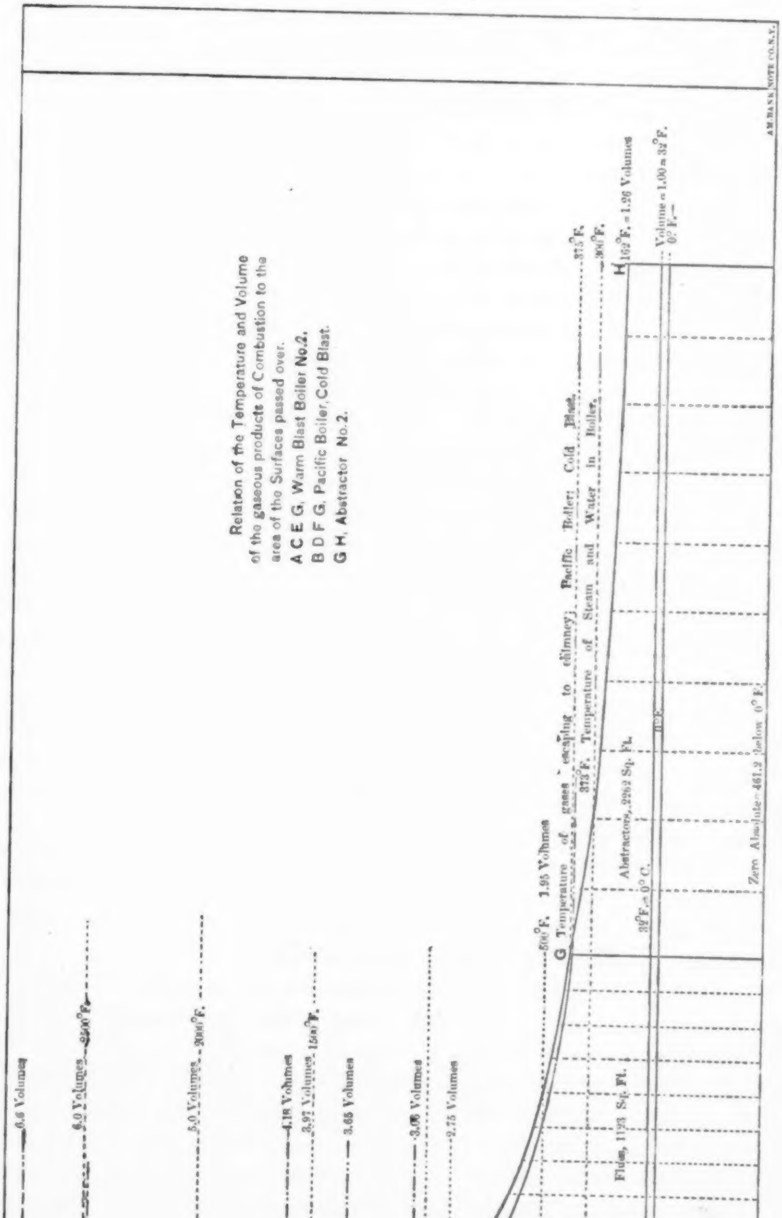
The diminishing rate of absorption with reduction of temperature, as the gases approach the temperature of the absorbing surfaces, is clearly shown.

The gases are, in fact, cooled by the air in the abstractor 138° F. below the temperature of the steam in the boiler, but a very large area is required to do this.

Incidentally, Fig. 176 shows the relative volume of the gases of combustion at successive points. Calling the volume at the temperature of external air (32° F.), equal to 1, it is 6 to 6.6 in the heart of the fire, 3.65 to 4.18 at the bridge wall, 2.75 to 3.06 at the pier, on entering the boiler-flues; 1.69 at the smoke-box, and 1.26 at the blower, where it is discharged to the chimney.

These two diagrams, Fig. 175 and Fig. 176, are a complete summary of the experiments recorded in these pages, so far as they relate to the two modes of boiler setting, with cold blast and warm blast, applied to boilers otherwise exactly alike, under equal conditions.

Analysis of Coals.—The manner of obtaining and preserving samples of coal has been already described. A suitable portion of each sample to be analyzed, separated from the rest with the precautions usual in assaying to insure a fair representation of the whole sample in the part selected, was put into a platinum "boat," weighed, inserted in a glass tube about $\frac{5}{8}$ inch caliber and 24 inches long, and kept at a gentle heat—a little above 100° C—in the furnace seen in Fig. 172, with a stream of air passing through the tube, to desiccate the coal, until after repeated trials, it came to


Relation of the Temperature and Volume of the gaseous products of Combustion to the area of the Surfaces passed over.
A C E G, Warm Blast Boiler No.2.
B D F G, Pacific Boiler, Cold Blast.
G H, Abstractor No.2.
A 2783°F. 6.6 Volumes
B 2482°F. 6.0 Volumes 2500°F.
5.0 Volumes 2000°F.
C 1600°F. 4.18 Volumes
3.97 Volumes 1500°F.
D 1340°F. 3.65 Volumes
E 1050°F. 3.06 Volumes
F 898°F. 2.75 Volumes
500°F. 1.95 Volumes
G Temperature of gases escaping to chimney; Pacific Boiler; Cold Blast. 373°F.
373°F. Temperature of Steam and Water in Boiler. 300°F.
H 162°F. = 1.26 Volumes
Volume = 1.00 = 32°F.
0°F.
Over Fire 47. Sq. Ft.
Bridge Wall to Pier 102. Sq. Feet
Flues, 1193 Sq. Ft.
Abstractors, 2262 Sq. Ft.
32°F. = 0° C.
0°F.
Zero Absolute = 461.2 below 0°F.
AM. BANK NOTE CO. N.Y.

constant weight, when it was supposed to be dry. The tube was then connected with a can containing compressed oxygen, the heat was increased, by means of the fifteen Bunsen burners of the furnace, to a moderate red heat, and a stream of oxygen was passed through the tube, until after repeated trials, the boat and its contained coal again came to constant weight—the carbon (and any other combustible substances which may have been present), having been oxidized, leaving in the boat ash only.

The use of oxygen instead of atmospheric air facilitates the oxidation, greatly shortens the process, and not only saves the time of the assistant, but, most important of all, lessens in a still greater degree the danger of losing an analysis through the premature breaking of a tube—a circumstance happening with vexatious frequency when air is used. All analyses were made in duplicate, and in case of suspicious difference, or of accident to one boat, they were repeated until satisfactory agreement was reached.

Passing, after leaving the tube, through a calcium chloride tube and a set of Geissler bulbs, the oxygen leaves its water, derived from oxidation of the hydrogen, in the former, and its CO_2, derived from the oxidation of the carbon, in the latter. The four chief ingredients of the coal—carbon, water, ash, and hydrogen, being thus determined directly, by weight, the remaining possible ingredients—oxygen, sulphur, and nitrogen are, in the anthracites left undistinguished, as a residuum, small in amount, only about 2.8 per cent. in the aggregate. In the bituminous coals, the determination of the sulphur, less than 1 %, and of the oxygen, 4.5 to 5% (in one case), leaves the nitrogen as a residuum, 2 %.

The continuous reservation of samples—at every firing—the systematic preservation of these samples, their uniform treatment, and the great number of duplicate analyses, give reason for considerable confidence in the final mean results.

Calorimetric observations to determine the quantity of entrained water in the steam.—The full notes of all experiments with the calorimeter, made during the entire week, July 11–16, 1881, are subjoined, together with the calculations of results. These experiments were made at various stages of the fire, and under varying conditions of demand for steam, and of rising and falling, and stationary pressure, and are supposed to represent fairly the usual operation of the Pacific Boiler in this respect. In a few instances, noticeably in the three observations on July 14, there is a slight irregularity in the first reading of the thermometer, "after admitting steam," column 7, due, perhaps, to imperfect mixing; but subsequent readings are clear.

TABLE XXIII.—CALORIMETRIC OBSERVATIONS, JULY 11, 1881, A.M. (1)

CALORIMETRY: QUALITY OF STEAM.

TIME OF READINGS.	WEIGHT OF CALORIMETER AND CONTAINED WATER.		WEIGHT OF CONDENSED STEAM.		TEMPERATURE OF WATER.		DIFFERENCE OF TEMPERATURE.	INTERVAL OF TIME.	STEAM-GAUGE PRESSURE.
	Before admitting steam.	After admitting steam.	By difference.	By wt., after drawing off.	Before admitting steam.	After admitting steam.			
1	2	3	4	5	6	7	8	9	10
A.M.	Lbs. av.	Lbs. av.	Lbs. av.	Lbs. av.	Deg. F.	Deg. F.	Deg. F.	Seconds.	Lbs. per sq. in.
11:48	515.625	521.0625	5.4375	5.6094					
0					57.85°				
1					57.875°				
2					57.9°				
3:05	Steam	let on.			57.925°				
3:50	Steam	shut off.			57.95°	86.575°	28.625°	45	39.9
5						86.55°			
6						86.525°			
7						86.5°			
8						86.475°			

TABLE XXIII.—CALORIMETRIC OBSERVATIONS, JULY 11, 1881, P.M.—*Continued.* (2)

CALORIMTERY : QUALITY OF STEAM.

TIME OF READINGS.	WEIGHT OF CALORIMETER AND CONTAINED WATER.		WEIGHT OF CONDENSED STEAM.		TEMPERATURE OF WATER.		DIFFERENCE OF TEMPERATURE.	INTERVAL OF TIME.	STEAM-GAUGE PRESSURE.
	Before admitting steam.	After admitting steam.	By difference,	By wt., after drawing off.	Before admitting steam.	After admitting steam.			
1	2	3	4	5	6	7	8	9	10
P.M.	Lbs. av.	Lbs. av.	Lbs. av.	Lbs. av.	Deg. F.	Deg. F.	Deg. F.	Seconds.	Lbs. per sq. in.
2:20	517.0625	525.625	8.5625	8.7793					
0					54°				
1					54.0125°				
2					54.025°				
3					54.05°				
4					54.075°				
4:30]	Steam	let on.							
5:10	St am	shut off.			54.1°	98.55°	44.45°	40	75.5
6						98.5°			
7						98.45°			
8						98.4°			
9						98.35°			
10						98.3°			

TABLE XXIII.—CALORIMETRIC OBSERVATIONS, JULY 12, 1881, A.M.—*Continued.* (3)

TIME OF READINGS.	WEIGHT OF CALORIMETER AND CONTAINED WATER.		WEIGHT OF CONDENSED STEAM.		TEMPERATURE OF WATER.		DIFFERENCE OF TEMPERATURE.	INTERVAL OF TIME.	STEAM-GAUGE PRESSURE.
	Before admitting steam.	After admitting steam.	By difference.	By wt., after drawing off.	Before admitting steam.	After admitting steam.			
1	2	3	4	5	6	7	8	9	10
A.M.	Lbs. av.	Lbs. av.	Lbs. av.	Lbs. av.	Deg. F.	Deg. F.	Deg. F.	Seconds.	Lbs. per sq. in.
9:05	517.0625	526.75	9.6875	9.7168					
0					44.775°				
1					44.8°				
2					44.825°				
3					44.85°				
4					44.875°				
4:08	Steam	let on.			44.8875°	94.4°	49.5125°	60	54.2
5:08	Steam	shut off.							
6						93.325°			
7						93.275°			
8						93.225°			
9						93.175°			
10						93.125°			

CALORIMETRY: QUALITY OF STEAM.

TABLE XXIII.—CALORIMETRIC OBSERVATIONS, JULY 12, 1881, P.M.—*Continued.* (4)

CALORIMETRY : QUALITY OF STEAM.

TIME OF READINGS.	WEIGHT OF CALORIMETER AND CONTAINED WATER.		WEIGHT OF CONDENSED STEAM.		TEMPERATURE OF WATER.		DIFFERENCE OF TEMPERATURE.	INTERVAL OF TIME	STEAM-GAUGE PRESSURE.
	Before admitting steam.	After admitting steam.	By difference.	By wt., after drawing off.	Before admitting steam.	After admitting steam.			
1	2	3	4	5	6	7	8	9	10
P.M.	Lbs. av.	Lbs. av.	Lbs. av.	Lbs. av.	Deg. F.	Deg. F.	Deg. F.	Seconds.	Lbs. per sq. in.
3:15	516.5625	529.25	12.6875	12.7324					
0					38.175°				
1					38.225°				
2					38.275°				
3					38.325°				
4					38.375°				
4:30	Steam	let on.			38.4°				
5:30	Steam	shut off.			38.45°	102.625°	64.175°	60	75.5
6						102.6°			
7						102.55°			
8						102.5°			
9						102.45°			
10						102.4°			

TABLE XXIII.—CALORIMETRIC OBSERVATIONS, JULY 13, 1881, A.M.—*Continued.* (5)

CALORIMETRY: QUALITY OF STEAM.

TIME OF READINGS.	WEIGHT OF CALORIMETER AND CONTAINED WATER.		WEIGHT OF CONDENSED STEAM.		TEMPERATURE OF WATER.		DIFFERENCE OF TEMPERATURE.	INTERVAL OF TIME.	STEAM-GAUGE PRESSURE.
	Before admitting steam.	After admitting steam.	By difference.	By wt., after drawing off.	Before admitting steam.	After admitting steam.			
1	2	3	4	5	6	7	8	9	10
A.M.	Lbs. av.	Lbs. av.	Lbs. av.	Lbs. av.	Deg. F.	Deg. F.	Deg. F.	Seconds.	Lbs. per sq. in.
9:35	518.8125	523.625	4.8125	4.8437					
0					46.475°				
1					46.5°				
2					46.525°				
3	Steam	let on.			46.55°				
4:15	Steam	shut off.			46.575°	71°	24.425°	75	17.5
5						71°			
6						71°			
7						71°			
8						71°			
9						71°			

TABLE XXIII.—CALORIMETRIC OBSERVATIONS, JULY 13, 1881, P.M. (a).—*Continued.* (6)

CALORIMETRY: QUALITY OF STEAM.

TIME OF READINGS.	WEIGHT OF CALORIMETER AND CONTAINED WATER.		WEIGHT OF CONDENSED STEAM.		TEMPERATURE OF WATER.		DIFFERENCE OF TEMPERATURE.	INTERVAL OF TIME.	STEAM-GAUGE PRESSURE.
	Before admitting steam.	After admitting steam.	By difference.	By wt., after drawing off.	Before admitting steam.	After admitting steam.			
1	2	3	4	5	6	7	8	9	10
P.M.	Lbs. av.	Lbs. av.	Lbs. av.	Lbs. av.	Deg. F.	Deg. F.	Deg. F.	Seconds.	Lbs. per sq. in.
3:05	517.5	524.25	6.75	6.8984					
0					51.5°				
1					51.5°				
2					51.25°				
3	Steam	let on.			51.55°				
3:50	Steam	shut off.			51.575°	86.4°	34.825	50	41.3
5						86.4°			
6						86.35°			
7						86.35°			
8						86.3°			
9						86.3°			

TABLE XXIII.—CALORIMETRIC OBSERVATIONS, JULY, 13, 1881, P.M. (b).—*Continued.* (7)

CALORIMETRY: QUALITY OF STEAM.

TIME OF READINGS.	WEIGHT OF CALORIMETER AND CONTAINED WATER.		WEIGHT OF CONDENSED STEAM.		TEMPERATURE OF WATER.		DIFFERENCE OF TEMPERATURE.	INTERVAL OF TIME.	STEAM-GAUGE PRESSURE.
	Before admitting steam.	After admitting steam.	By difference.	By wt., after drawing off.	Before admitting steam.	After admitting steam.			
1	2	3	4	5	6	7	8	9	10
A.M.	Lbs. av.	Lbs. av.	Lbs. av.	Lbs. av.	Deg. F.	Deg. F.	Deg. F.	Seconds.	Lbs. per sq. in.
5:15	518.25	525.50	7.25	7.1445					
0					51.225°				
1					51.25°				
2					51.275°				
3	Steam	let on.			51.325°				
3:45	Steam	shut off.			51.35°	87.575°	36.225°	45	50.7
4						87.55°			
5						87.525°			
6						87.5°			
7						87.475°			
8						87.45°			
9						87.425°			

TABLE XXIII.—CALORIMETRIC OBSERVATIONS, JULY 14, 1881, A.M.—*Continued*. (8)

CALORIMETRY: QUALITY OF STEAM.

TIME OF READINGS.	WEIGHT OF CALORIMETER AND CONTAINED WATER. Before admitting steam.	WEIGHT OF CALORIMETER AND CONTAINED WATER. After admitting steam.	WEIGHT OF CONDENSED STEAM. By difference.	WEIGHT OF CONDENSED STEAM. By wt., after drawing off.	TEMPERATURE OF WATER. Before admitting steam.	TEMPERATURE OF WATER. After admitting steam.	DIFFERENCE OF TEMPERATURE.	INTERVAL OF TIME.	STEAM-GAUGE PRESSURE.
1	2	3	4	5	6	7	8	9	10
A.M.	Lbs. av.	Lbs. av.	Lbs. av	Lbs. av.	Deg. F.	Deg. F	Deg. F.	Seconds.	Lbs. per sq. in.
9:30	517.625	525.3125	7.6875	7.7812					
0					37.9°				
1					37.925°				
2					37.95°				
3	Steam	let on.			37.975°				
3:50	Steam	shut off.			38.°	77.65°	39.65°	50	49.1
4									
5						78°(?)			
6						77.525°			
7						77.5°			
8						77.475°			
9						77.45°			
10						77.425°			
11									

TABLE XXIII.—CALORIMETRIC OBSERVATIONS, JULY 14, 1881, A.M. (a).—*Continued.* (9)

CALORIMETRY: QUALITY OF STEAM.

TIME OF READINGS.	WEIGHT OF CALORIMETER AND CONTAINED WATER		WEIGHT OF CONDENSED STEAM.		TEMPERATURE OF WATER.		DIFFERENCE OF TEMPERATURE.	INTERVAL OF TIME.	STEAM-GAUGE PRESSURE.
	Before admitting steam.	After admitting steam.	By difference.	By wt., after drawing off.	Before admitting steam.	After admitting steam.			
1	2	3	4	5	6	7	8	9	10
P.M.	Lbs. av.	Lbs. av.	Lbs. av.	Lbs. av.	Deg. F.	Deg. F.	Deg. F.	Seconds.	Lbs. per sq. in.
3:45	515.5625	525.25	9.6875						
0					43.5°				
1					43.525°				
2					43.55°				
3					43.575°				
4	Steam	let on.			43.6°				
5:30	Steam	shut off.			43.625°	93.075°	49.45°	90	36.6
6									
7				9.6718		93.45° (?)			
8						93.°			
9						92.975°			
10						92.925°			
11						92.9°			
12						92.875°			

TABLE XXIII.—CALORIMETRIC OBSERVATIONS, JULY 14, 1881, P.M. (b).—*Continued.* (10)

CALORIMETRY : QUALITY OF STEAM.

TIME OF READINGS.	WEIGHT OF CALORIMETER AND CONTAINED WATER.		WEIGHT OF CONDENSED STEAM.		TEMPERATURE OF WATER.		DIFFERENCE OF TEMPERATURE.	INTERVAL OF TIME.	STEAM-GAUGE PRESSURE.
	Before admitting steam.	After admitting steam.	By difference.	By wt., after drawing off.	Before admitting steam.	After admitting steam.			
1	2	3	4	5	6	7	8	9	10
P.M.	Lbs. av.	Lbs. av.	Lbs. av.	Lbs. av.	Deg. F.	Deg. F.	Deg. F.	Seconds.	Lbs. per sq. in.
5:30	515.875	525.625	9.75	9.7695					
0					35.35°				
1					35.375°				
2					35.4°				
3					35.425°				
4	Steam	let on.			35.45°				
4:45	Steam	shut off.			35.475°	86.2°	50.725°	45	65.0
5									
6						86.1° (?)			
7						86.05°			
8						85.975°			
9						85.95°			
10						85.9°			

TABLE XXIII.—CALORIMETRIC OBSERVATIONS, JULY 15, 1881, A.M.—*Continued*. (11)

CALORIMETRY : QUALITY OF STEAM.

TIME OF READINGS.	WEIGHT OF CALORIMETER AND CONTAINED WATER.		WEIGHT OF CONDENSED STEAM.		TEMPERATURE OF WATER.		DIFFERENCE OF TEMPERATURE.	INTERVAL OF TIME.	STEAM-GAUGE PRESSURE.
	Before admitting steam.	After admitting steam.	By difference.	By wt., after drawing off.	Before admitting steam.	After admitting steam.			
1	2	3	4	5	6	7	8	9	10
A.M.	Lbs. av.	Lbs. av.	Lbs. av.	Lbs. av.	Deg. F.	Deg. F.	Deg. F.	Seconds.	Lbs. per sq. in.
9:10	517.375	526.875	9.5	9.496					
0					39.95°				
1					39.975°				
2					40°				
3	Steam	let on.			40.025°				
3:45	Steam	shut off.			40.05°	88.425°	48.375°	45	67.8
4									
5									
6						88.375°			
7						88.35°			
8						88.325°			
9						88.3°			
10						88.275°			

TABLE XXIII.—CALORIMETRIC OBSERVATIONS, JULY 15, 1881, P.M. (a).—*Continued.* (12)

CALORIMETRY: QUALITY OF STEAM.

TIME OF READINGS.	WEIGHT OF CALORIMETER AND CONTAINED WATER.		WEIGHT OF CONDENSED STEAM.		TEMPERATURE OF WATER.		DIFFERENCE OF TEMPERATURE.	INTERVAL OF TIME.	STEAM-GAUGE PRESSURE.
	Before admitting steam.	After admitting steam.	By difference.	By wt., after drawing off.	Before admitting steam.	After admitting steam.			
1	2	3	4	5	6	7	8	9	10
P.M.	Lbs. av.	Lbs. av.	Lbs. av.	Lbs. av.	Deg. F.	Deg. F.	Deg. F.	Seconds.	Lbs. per sq. in.
1:40	518.5	528.4375	9.9375	9.7617					
0					41.825°				
1					41.85°				
2					41.875°				
3					41.9°				
4	Steam	let on.							
5:30	Steam	shut off.			41.95°	91.8°	49.85°	90	75.0°
6									
7									
8						91.65°			
9						91.6°			
10						91.55°			
11						91.5°			
12						91.45°			

TABLE XXIII.—CALORIMETRIC OBSERVATIONS, JULY 15, 1881, P.M. (b).—*Continued.* (13)

CALORIMETRY: QUALITY OF STEAM.

TIME OF READINGS.	WEIGHT OF CALORIMETER AND CONTAINED WATER.		WEIGHT OF CONDENSED STEAM.		TEMPERATURE OF WATER.		DIFFERENCE OF TEMPERATURE.	INTERVAL OF TIME.	STEAM-GAUGE PRESSURE.
	Before admitting steam.	After admitting steam.	By difference.	By wt., after drawing off.	Before admitting steam.	After admitting steam.			
1	2	3	4	5	6	7	8	9	10
P.M.	Lbs. av.	Lbs. av.	Lbs. av.	Lbs. av.	Deg. F.	Deg. F.	Deg. F.	Seconds.	Lbs. per sq. in.
5:55	518.6875	531.3125	12.625	12.6289					
0					41.5°				
1					41.525°				
2					41.55°				
3					41.575°				
4	Steam	let on.			41.6°				
5					41.625°				
6					41.65°	104°	62.35°	180	24.9
7	Steam	shut off.				103.95°			
8						103.925°			
9						103.9°			
10						103.875°			
11						103.8°			

TABLE XXIII.—CALORIMETRIC OBSERVATIONS, JULY 16, 1881, A.M.—*Continued.* (14)

CALORIMETRY: QUALITY OF STEAM.

TIME OF READINGS.	WEIGHT OF CALORIMETER AND CONTAINED WATER.		WEIGHT OF CONDENSED STEAM.		TEMPERATURE OF WATER.		DIFFERENCE OF TEMPERATURE.	INTERVAL OF TIME.	STEAM-GAUGE PRESSURE.
	Before admitting steam.	After admitting steam.	By difference.	By wt., after drawing off.	Before admitting steam.	After admitting steam.			
1	2	3	4	5	6	7	8	9	10
A.M.	Lbs. av.	Lbs. av.	Lbs. av.	Lbs. av.	Deg. F.	Deg. F.	Deg. F.	Seconds.	Lbs. per sq. in.
11:05	518.25	526.1875	7.9375	7.92					
0					42.025°				
1					42.05°				
2					42.075°				
3	Steam	let on.							
4:15	Steam	shut off.			42.125°	82.6°	40.475°	75	33.4
5									
6						82.6° (?)			
7						82.5°			
8						82.45°			
9						82.475°			
10						82.4°			

Calculation of the quantity of entrained water in steam, from data obtained by calorimetric observations, Monday, July 11, 1881, 11h. 48m. A.M. given in detail in Table XXIII. (1), p. 765.

Barometer, corrected reading	in.	29.79
Corresponding atmospheric pressure	lbs. per sq. in.	14.63
Boiler pressure by steam gauge	" "	39.90
Steam pressure absolute	" "	54.53
Number of British thermal units above 0° F. contained in 1 lb. of saturated steam of 54.53 lbs. per sq. in. absolute pressure	B. t. u.	1201.2755
Number of B. t. u. contained in 1 lb. of water of 86.575° F. (also above 0° F.)	B. t. u.	86.6232
Number of B. t. u. given up by 1 lb. of saturated steam of 54.53 lbs. per sq. in. absolute pressure condensed and cooled to 86.575° F.	B. t. u.	1114.6523
Number of B. t. u. which would be given up by 5.6094 lbs. of saturated steam of 54.53 lbs. per sq. in. absolute pressure, by condensation at 86.575°; 1114.6523 × 5.6094 =	B. t. u.	6252.5306
Gross weight of calorimeter and water therein contained, before admitting steam, col. 2	lbs.	515.625
Weight of calorimeter, empty	lbs.	317.625
Net weight of water in calorimeter	lbs.	198.
Heat capacity of calorimeter, in equivalent weight of water	lbs.	17.2
Calorific value in B. t. u. of calorimeter and contents	lbs.	215.2
Number of B. t. u. contained in water at 86.575°; brought forward	B. t. u.	86.6232
Number of B. t. u. contained in water at 57.95 (col. 4, p. 707.)	B. t. u.	57.9570
Number of B. t. u. actually gained by each 1 lb. of water raised from 57.95° F to 86.575° F	B. t. u.	28.6662
Number of B. t. u. gained by 215.2 lbs. of water, including the equivalent for the calorimeter, in rising from 57.95° to 86.575; 215.2 × 28.6662	B. t. u.	6168.9662
Excess of the number of B. t. u. which would have been given up by saturated steam, over the number actually gained by the water, = 6252.5306 − 6168.9662	B. t. u.	83.5644
Ratio of this excess to the number which would have been given up by 5.6094 lbs. of saturated steam of 54.53 lbs. per sq. in. absolute pressure, condensed at 86.575° F., = 83.5644 ÷ 6252.5306 =	Ratio.	.013365

It therefore appears that the 5.6094 lbs. of actual "steam" admitted to the calorimeter was not saturated steam, but a mixture of saturated steam and water of equal temperature, in such proportions as to require 1.3365 per cent. of the quantity of heat which 5.6094 lbs. of saturated steam of 54.53 lbs. per square inch pressure absolute would have given in condensing at 86.575° F., to complete the evaporation of the entrained water.

The temperature of the steam and water alike is..	Deg. F.	286.3457
The number of B. t. u. above 0° F. contained in water of temperature 286.3457° F. is	B. t. u.	288 5885
From this number subtract the number of B. t. u. above 0° F. contained in water of temperature 86.575° F..................................	B. t. u.	86.6232
And we have the number of B. t. u. imparted per pound of water, between 286.3457° and 86.575° F.	B. t. u.	201.9653

Each pound of saturated steam of 54.53 lbs. per square inch pressure absolute, and therefore of 286.3457° F. temperature, contains, as we have seen, 1201.2755 B. t. u., and in condensing and cooling to 86.575° F., must give out, $1201.2755 - 86.6232 = 1114.6523$ B. t. u., and $1114.6523 \div 201.9653 = 5.5190$, the ratio of the heating power of unit weight of steam to that of unit weight of water of this temperature. These two fluids, then, steam and water, are in this instance, mixed in such proportions that 5.6094 pounds of the mixture give out, in cooling from 286.3457° to 86.575° F., 6168.9662 B. t. u. A few trials enable us to determine that 98.365 per cent. of the 5.6094 pounds of the mixture, amounting to 5.5177 pounds, are steam, giving out:

$5.5177 \times 1114.6523 =$	B. t. u.	6150.3170
And that $100 - 98.365 = 1.635$ per cent., amounting to 0.0917 pounds, are water, giving out $0.0917 \times 201.9653 =$	B. t. u.	18.5202
Making a total of....	....	6168.8372
Which is substantially equal to the heat in B. t. u. gained by the water; $= 215.2 \times 28.6662 =$	B. t. u.	6168.9662

Calculations similar to the foregoing applied to the data obtained by calorimetric observations at other times during the week, July 11–16, as given in Table XXIII. (1) to (14), give results which, with the one above given in detail, are tabulated below.

TABLE XXIV.

REDUCTION OF CALORIMETRIC OBSERVATIONS. (1)

No.	Day in July, 1881, when experiments were made, and hour and minute of beginning of experiment.			Pressures: atmos. and steam.				Temperatures.	
				Barometer.		Steam gauge.			
	Day of month.	Part of day.	H. M.	Inches of mercury, corrected to 32° F.	Pressure of atmos., lbs. per sq. in.	Boiler press. above atmos., lbs. per sq. in.	Boiler press. absolute, lbs. per sq. in.	Of steam admitted to calorimeter and entrained water, Degrees F.	Of water condensed in calorimeter and entrained water, Degrees F.
1	2	3	4	5	6	7	8	9	10
1	11	A.M.	11:48	29.79	14.63	39.9	54.53	286.3456	86.575
2	11	P.M.	2:20	29.80	14.64	75.5	90.14	320.1485	98.55
3	12	A.M.	9:5	29.69	14.58	54.2	68.78	301.5381	94.4
4	12	P.M.	3:15	29.63	14.55	75.5	90.05	320.0781	102.625
5	13	A.M.	9:35	29.52	14.50	17.5	32.00	253.9520	71.
6	13	P.M.	3:5	29.45	14.46	41.3	55.76	287.7748	86.4
7	13	P.M.	5:15	29.42	14.45	50.7	65.15	297.9286	87.575
8	14	A.M.	9:30	29.45	14.46	49.1	63.56	310.5596	77.65
9	14	P.M.	3:45	29.48	14.48	36.6	51.08	282.1972	93.075
10	14	P.M.	5:30	29.51	14.49	65.0	79.49	311.4098	86.2
11	15	A.M.	9:10	29.66	14.57	67.8	82.37	313.8312	88.425
12	15	P.M.	1:40	29.63	14.55	75.0	89.55	319.6835	91.8
13	15	P.M.	5:55	29.59	14.53	24.9	39.43	266.2530	104.
14	16	A.M.	11:5	29.30	14.39	33.4	47.79	278.0217	82.6
	Means						64.98	296.4087	89.3482

TABLE XXIV.—REDUCTION OF CALORIMETRIC OBSERVATIONS.—*Continued.* (2)

	BRITISH THERMAL UNITS.			Weight of water condensed in steam-drum of calorimeter during experiment.	Total B. t. u. which would have been imparted to the water if the steam had been saturated, dry steam.
	Contained in one pound of saturated steam of given absolute pressure.	Contained in one pound of water condensed in steam-drum of calorimeter.	Which would have been imparted to the water if the steam had been saturated.		
No.	B. t. u. per lb.	B. t. u. per lb.	B. t. u. per lb.	Pounds av.	Total B. t. u.
1	11	12	13	14	15
1	1201.2755	86.6232	1114.6523	5.5324 .0770 5.6094	6252.5306
2	1211.5858	98.6257	1112.9601	8.6611 .1182 8.7793	9771.0106
3	1205.9092	94.4638	1111.4454	9.6603 .0565 9.7168	10799.6927
4	1211.5637	102.7129	1108.8508	12.5134 .2190 12.7324	14118.3319
5	1191.3882	71.0210	1120.3672	4.7630 .0807 4.8437	5426.7226
6	1201.7115	86.4478	1115.2637	6.7833 .1151 6.8984	7693.5351
7	1204.8084	87.6252	1117.1832	7.0696 .0749 7.1445	7981.7154
8	1204.3102	77.6813	1126.6289	7.6453 .1359 7.7812	8766.5248
9	1200.0106	93.1362	1106.8694	9.6207 .0511 9.6718	10705.4195
10	1208.9076	86.2474	1122.6602	9.7425 .0270 9.7695	10967.8288
11	1209.6584	88.4769	1121.1815	9.3648 .1312 9.4960	10646.7395
12	1211.4434	91.8586	1119.5848	9.7192 .0425 9.7617	10929.0509
13	1195 1463	104.0920	1091.0543	12.4828 .1461 12.6289	13778.8156
14	1198.7372	82.6402	1116.0970	7.9065 .0135 7.9200	8839.4882
Mean by weighing				8.7681	9762.6719
Mean by difference				8.7366	
Mean apparent error				.0315	
				8.66185 .10625 8.76810	

TABLE XXIV.—*Continued.*

REDUCTION OF CALORIMETRIC OBSERVATIONS. (3)

No.	Weight of water in calorimeter including thermal equivalent of calorimeter.	Temperature of water in calorimeter just before admitting steam.	B. t. u. contained in one pound of water in calorimeter before admitting steam.	B. t. u. imparted to one pound of water raised from initial to final temperature.	Total heat gained by the water in column 16 in being raised from *t* in column 17 to temperature in column 10.	Deficit of heat due to water entrained in the steam. Difference of columns 15 and 20.
	Lbs. av.	Deg. F.	B. t. u.	B. t. u.	Total B. t. u.	B. t. u.
1	16	17	18	19	20	21
1	215.2	57.95	57.9570	28.6662	6168.9662	83.5644
2	216.6375	54.1	54.1051	44.5206	9644.8315	126.1791
3	216.6375	44.8875	44.8895	49.5743	10739.6524	60.0403
4	216.1375	38.45	38.4505	64.2624	13889.5145	228.8174
5	218.3875	46.575	46.5770	24.4440	5338.2641	88.4585
6	217.075	51.575	51.5790	34.8688	7569.1448	124 3903
7	217.825	51.35	51.3540	36.2712	7900.7741	80.9413
8	217.2	38	38.0000	39.6813	8618.7784	147.7464
9	215.1375	43.625	43.6266	49.5096	10651.3716	54.0479
10	215.45	35.475	35.4750	50.7724	10938.8709	28.9579
11	216.95	40.05	40.0510	48.4259	10505.9990	140.7405
12	218.075	41.95	41 9510	49.9076	10883.5999	45.4510
13	218.2625	41.65	41.6510	62.4410	13628.5288	150.2868
14	217.825	42.125	42.1260	40.5142	8825.0056	14.4826
	216.9143	44.84	Means	Means	9664.5216	98.1503

TABLE XXIV.—*Continued.*

REDUCTION OF CALORIMETRIC OBSERVATIONS. (4)

	Day in July, 1881, when experiments were made, and hour and minute of beginning of experiments.				BRITISH THERMAL UNITS.		RATIO: PER CENT.	
	Day of week.	Part of day.	Day of month.	H. M.	Gained by the water in column 16, in being raised from initial to final temperature.	Which would have been imparted to the water if the steam had been saturated : dry.	Heat required to evaporate the entrained water.	Ratio of entrained water to total water and steam.
					B. t. u.	B. t. u.	Per cent.	Per cent.
1	22	23	24	25	26	27	28	29
1	Monday	A.M.	11	11:48	6168.97	6252.53	1.34	1.37
2	"	P.M.	11	2:20	9644.83	9771.01	1.29	1.35
3	Tuesday	A.M.	12	9:5	10739.65	10799.69	.55	.58
4	"	P.M.	12	3:15	13889.51	14118.33	1.62	1.72
5	Wed'day	A.M.	13	9:35	5338.26	5426.72	1.63	1.67
6	"	P.M.	13	3:5	7569.14	7693.54	1.62	1.67
7	"	P.M.	13	5:15	7900.77	7981.72	1.01	1.05
8	Thursd'y	A.M.	14	9:30	8618.78	8766.52	1.69	1.75
9	"	P.M.	14	3:45	10651.37	10705.42	.50	.53
10	"	P.M.	14	5:30	10938.87	10967.83	.26	.28
11	Friday	A.M.	15	9:10	10506.00	10646.74	1.32	1.38
12	"	P.M.	15	1:40	10883.60	10929.05	.42	.44
13	"	P.M.	15	5:55	13628.53	13778.82	1.09	1.16
14	Saturday	A.M.	16	11:5	8825.01	8839.49	.16	.17
	Means				9664.52	9762.67		
	Mean ratios, per cent						1.04	1.08

A few words as to the possible limits of error in these observations and results may be of interest.

Mean weight of water, including 17.2 lbs. for calorimeter, col. 16	lbs.	216.9143
Gross weight, after admitting steam	lbs.	526.0759
Gross weight, before admitting steam	lbs.	517.3393
Mean weight of steam, by difference	lbs.	8.7366
Mean by separate weighing, col. 14	lbs.	8.7681
Mean sum of errors	lbs.	.0315
Greatest possible error in separate weighing, say 7 grains	lbs.	.0010
Greatest probable error in weight of water in calorimeter	lbs.	.0300
Greatest probable error in pressure by steam gauge and barometer	lb. per sq. in.	0.1000
Greatest probable error in temperatures; thermometers graduated to tenths of a degree F	Deg. F.	0.1000

In the following table, Table XXV., all the assumed errors are added to the mean in the left-hand column, headed "maximum" and subtracted in the right-hand column, headed "minimum," except in the third line, t_2, temperature of water, final. The difference, or assumed error is here subtracted in the left-hand column, and added in the right-hand column, since this tends to magnify the error in the final result. It will be noticed that the mean deficit of heat, per cent., in this table—last line but one of middle column, is 1.16%, while in Table XXIV., it is 1.04%—of course because the mean of the separate calculations ought not to agree with the result of a calculation based on means of the observations. The wide variation from the mean—almost 40 per cent. each way—*may* occur in single observations, but are not probable, since errors are not unlikely to balance each other in some degree. In our case, with so many as fourteen observations, the mean result seems entitled to some degree of confidence.

If the assumed errors in the third line are transposed, and the maximum be put into the left-hand column, as in all the other cases, the variation in the final result almost disappears—the three numbers in the line next to the bottom—deficit of heat, per cent., become respectively 1.15, 1.16, 1.17. There is no constant ratio between the figures in column 28, Table XXIV., and those in column 29, the latter being affected by variations of final water temperature (column 10) and steam pressure and temperature (columns 8 and 9).

TABLE XXV.

LIMITS OF ERROR IN CALORIMETRIC WORK.

The numbers in this column, right-hand, refer to the headings of columns in Table XXIV.	NO.	KIND OF QUANTITY.	MAXIMUM.	MEAN.	MINIMUM.
P, pressure absolute......	8	Lbs. per sq. in.	65.08	64.98	64.88
t, temp of steam	9	Degrees F.	297.8587	297.7565	297.6540
t_2, of water, final	10	Degrees F.	89.2482	89.3482	89.4482
In 1 lb. of steam	11	B. t. u.	1204.7868	1204.7558	1204.7246
In 1 lb. of water	12	B. t. u.	89.3017	89.4018	89.5021
Difference	13	B. t. u.	1115.4851	1115.3540	1115.2225
				8.66185	
				.10625	
w, weight of steam	14	Lbs.	8.7691	8.7681	8.7671
Total heat................	15	B. t. u.	9781.8004	9779.5354	9777.2672
W, weight of water	16	Lbs.	216.9443	216.9143	216.8843
t, of water, initial	17	Degrees F.	44.94	44.84	44.74
In water..................	18	B. t. u.	44.9419	44.8417	44.7415
89.3017−44.9419, etc......	19	B. t. u.	44.3598	44.5601	44.7606
Total heat	20	B. t. u.	9623.6058	9665.7229	9707.8714
Deficit of heat............	21	B. t. u.	158.1946	113.8125	69.3958
Deficit of heat............	28	Per cent.	1.617	1.164	0.710
Water in steam	29	Per cent.	1.683	1.212	0.739

Continuous analysis of flue gases.—It would be out of place here to attempt a full description of the process of analysis pursued with the gaseous products of combustion, drawn from the descending smoke-flue near the blower. Such a description would seem to a chemist impertinent, and to others than chemists, pedantic. In brief, it was the gravimetric process, and was conducted as follows:

Samples of considerable volume were obtained through the mixing-box shown in Fig. 177, two of which were set in the flue,

one over the other, one foot apart, with the pipes disposed differently, so as to bring long pipes over short ones, next to the longest over next to the shortest, and only the ends of the central pipe of each group of five over the ends of the corresponding pipes below them; by which arrangement samples from the two boxes proved,

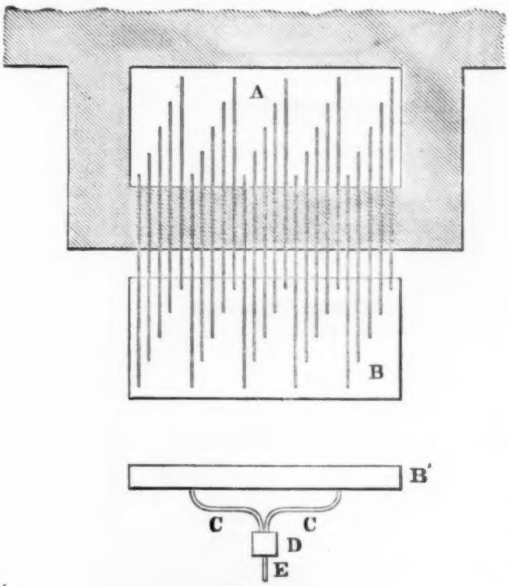


FIG. 177.
MIXING-BOX, FOR OBTAINING SAMPLES OF FLUE GASES FOR ANALYSIS.

A, Section of flue.
B, Section of mixing-box, showing the arrangement of the 25 pipes of $\frac{1}{4}$ inch gas-pipe.
B', Front elevation of mixing-box.
C, C, Pipes, four in number, from mixing-box to mixing chamber.
D, Mixing chamber.
E, Discharge-pipe leading to aspirator.

by their agreement, that they truly represented the heterogeneous assemblage of unmixed gases passing through the flue. The greater part of the samples so drawn off by the aspirator was permitted to go to waste; but a small stream was drawn out into a jar filled with water, out of which water flowed in drops in regulated quantity, to be constantly replaced by the sample of gases. The small stream of gases so drawn off was divided, part going to each one

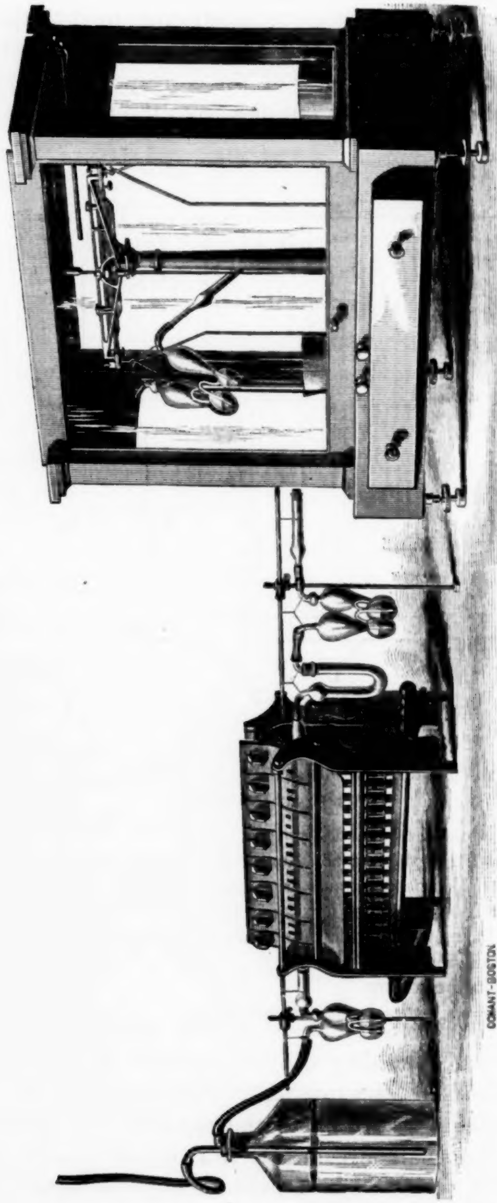


FIG. 172.—APPARATUS FOR CONTINUOUS ANALYSIS OF FLUE GASES BY THE GRAVIMETRIC METHOD.

of two exactly similar sets of Geissler bulbs, first, however, passing through a bulbous tube, Fig. 176, which will arrest any liquid water condensed from vapor in the gases, and then through the U tube, also seen in the figure, which is filled with calcium chloride, and will (if kept at a low temperature, by surrounding it with crushed ice) take up all moisture, and leave the fixed gases completely dry. The dry gases next pass on to and through the group of three Geissler bulbs, each one filled about three-fourths full of hydrate of potash, *i. e.*, a saturated solution of caustic potash. At each drop of water, a small bubble of the mixed gases passes down through a central tube nearly to the bottom of the first bulb, and rises as a bubble through the hydrate of potash, to the space above the surface of the liquid, dismissing, simultaneously, a similar bubble at the bottom of the second bulb, which in turn, and simultaneously, dismisses a third bubble into the last bulb, and liberates a similar bubble to pass to and slowly through the second straight, horizontal, bulbous tube, seen at the left-hand of the Geissler bulbs in Fig. 176. This bulbous tube is filled with dry caustic potash, which absorbs all moisture which may have been taken up by the dry gases in their passage through the hydrate of potash, so that the latter suffers no loss of weight—this bulbous tube and the set of Geissler bulbs being weighed together, as seen in Fig. 172. The carbon dioxide (CO_2) contained in the mixed gases is taken up by the hydrate of potash, rapidly by that in the first bulb, which soon presents a nacreous appearance, more slowly by the second, which gradually becomes opalescent, and still more slowly by the third, which is very slightly affected, as nearly all the CO_2 is absorbed in the first and second bulbs.

Some water is taken up by the dry gases, and possibly a little CO_2 along with it; but the dry caustic potash arrests both. The gases, deprived of their moisture and of their carbon dioxide, pass on to the left, to and through a glass tube about 0.6 inch in diameter and 20 inches long, seen about the middle of Fig. 176, extending through a small gas furnace.

This tube has two porous plugs of fibrous asbestos, about six inches apart, near the middle of its length, and the space between these plugs is filled with copper scale (oxide of copper), which is kept at a low red heat by the gas furnace. The gases, which, it will be remembered, now consist solely of oxygen, nitrogen and carbon monoxide (O, N, and CO), are changed, in passing through the hot copper scale, by the complete oxidation of the carbon in the

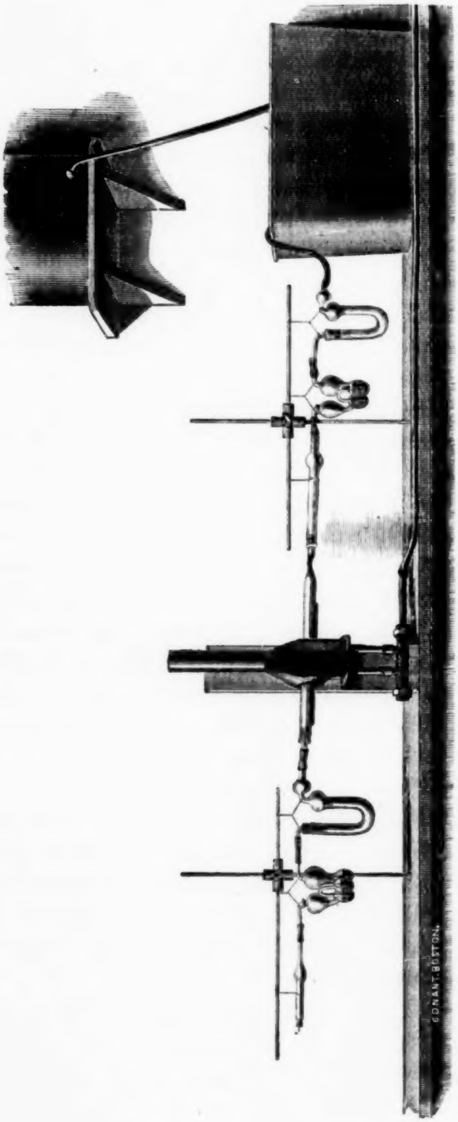


FIG. 176.—APPARATUS FOR ABSORPTION OF CO_2 FROM FLUE GASES, FOR CONVERSION OF CO INTO CO_2, AND FOR THE ABSORPTION OF THE RESULTING CO_2, IN THE CONTINUOUS ANALYSIS OF FLUE GASES. THE COURSE OF THE GASES IS FROM RIGHT TO LEFT.

CO, and the conversion of the CO and additional oxygen into CO_2. It is not easy—for a layman—to see just what office the copper scale performs that would not be as well performed by sand, or bits of fire-brick, since there is always an abundant supply of oxygen present in the surplus air. But the copper oxide would supply oxygen if there were none other present, and may act in some unexplained manner to promote oxidation of the CO. It is also possible that the dissociation of copper and oxygen offers less resistance than the mere mechanical obstruction of the nitrogen and carbon dioxide, after the free oxygen in the flue gases has been reduced as low as 10 per cent. Some experiments cited by Angus Smith in *Air and Rain*, make this seem probable. An analogy is found in the case of iron, which burns eagerly in pure oxygen, but is rendered incombustible in common air, containing 21 per cent. of oxygen, by the mechanical obstruction of the 79 per cent. of nitrogen. However this may be, the carbon which enters the tube as carbon monoxide (CO), leaves it as carbon dioxide (CO_2). Passing on through a second set of potash bulbs, supplemented as before with a dry potash tube, this CO_2 is all absorbed, and the residuary gases, oxygen and nitrogen, are received in a bottle over water (or glycerine), and stored for measurement.

This measurement is readily effected by weighing the liquid drawn off to make room for the gases. The weight and temperature of this liquid (and its specific gravity, also, if other than water) being ascertained, its volume becomes known; the tension of the gases is made equal to that of the atmosphere, which is ascertained by the barometer; and their temperature being also noted, their weight becomes known. From the weight of these residuary gases, and that of the carbon dioxide and the carbon monoxide separated from them, the weight of the original, dry, composite, or mixed gases is readily deducible. The absolute weight of the carbon dioxide obtained, is found by directly weighing the potash bulbs and tube, as seen attached to the scale-beam in Fig. 172, before and after the experiment. The difference is the weight of the CO_2 taken up by the potash, of which $\frac{3}{11}$ is carbon and $\frac{8}{11}$ oxygen.

The weight of the carbon monoxide is ascertained, indirectly, in a similar manner. The difference in weight before and after the experiment is again CO_2, of which all the carbon, $\frac{3}{11}$, and one-half the oxygen, $\frac{4}{11}$, are derived from the gases in the form of CO, and the remaining $\frac{4}{11}$, oxygen, derived from the free oxygen in the surplus air, or from the copper oxide.

Sulphur, in burning, forms chiefly sulphurous acid (SO_2), and a small quantity of sulphuric acid ($H_2O + SO_3 = H_2SO_4$), both of which are taken up by the water. A small quantity of CO_2 is also absorbed by the water, but this soon becomes saturated with CO_2, while it will continue to absorb sulphuric acid and sulphurous acid for some time.

The quantity of H_2SO_4 is so small as to render its accurate determination difficult in the flue gases, diluted as these are with air. The considerable increase in the quantity of ammonia found in the gases of the warm-blast boiler, makes it probable that all the sulphuric acid exists as a sulphate, mainly sulphate of ammonia.

Carbonate of ammonia was also produced in the warm-blast boiler in considerable quantities, coating all the smoke passages as white as the bolt-trough of a flouring mill.

It is chiefly for the determination of the quantity of carbon dioxide, of carbon monoxide and of surplus air, that analysis of the gaseous products of combustion is desirable, and for those purposes it is invaluable.

In addition to the continuous analysis, carried along all day and all night, in duplicate, for greater assurance of accuracy, samples of the gases drawn off at the same time were stored in bottles properly labeled, for subsequent repetition of the analysis in case verification appeared to be desirable. Such samples should be stored over glycerine, on account of the absorption of CO_2 by water; and on the same account the glycerine should be as nearly as possible anhydrous.

A very small steam or electric pump, with a plunger about 0.25 inch diameter, and stroke 0.5, or 0.75 inches, driven at such speed as to give about one bubble of gas per second at each set of Geissler bulbs, may be conveniently substituted for a siphon, to regulate the flow of the gases; and a short bit of broken thermometer tube, of small caliber, inserted in the line of flexible tube, helps to give a more uniform flow.

It is better to use Geissler bulbs of large size, and to deal with as large quantities of gas as can be conveniently managed; and on this account the balance--which cannot be too nice—should be of large size, adapted to weigh, without undue strain, 200 grammes, nearly 3,100 grains, say 7 oz. avoirdupois. With these precautions, proper care, adequate skill and *perfect integrity*, duplicate and repeated analyses will be found to agree very closely. Differences will appear, under the high magnifying power of decimals of one per

cent., but these differences will usually be very small. Such quantities as 0.12, or 0.08 of one per cent. (.0012, or .0008), appear small; but when they are found repeating themselves under like conditions, the results appear to be entitled to much confidence. Subsequent experience, indeed, has led Mr. Prentiss to the opinion that neglect to surround the calcium chloride tube with crushed ice may have permitted a little vapor of water to pass with the imperfectly desiccated gases into the potash bulbs, and so to increase very slightly the small quantity of CO. Reference has already been made to persistent attempts to produce carbon monoxide in quantities unusually large

CARBON-MONOXIDE PRODUCED BY EXCESSIVELY RAPID FIRING.
GRAPHICAL REPRESENTATION OF TABLE XXVI.
EXPERIMENTS MADE SEPT. 1. 1881.

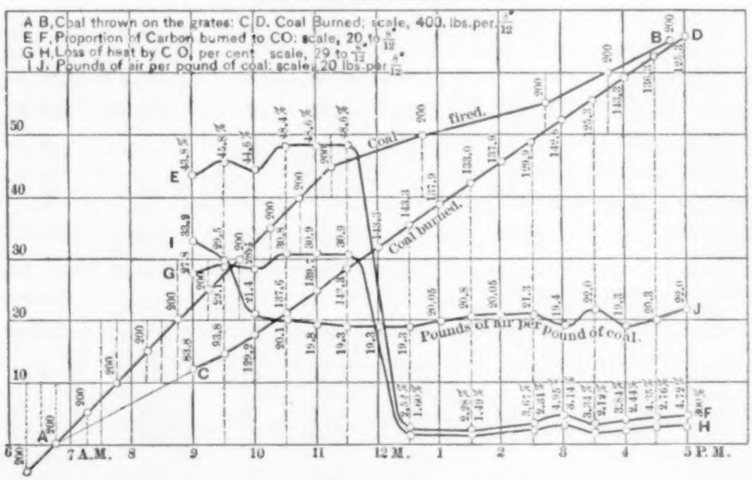


FIG. 178.

and to special analyses of the chimney gases during short periods, at regular intervals, under certain conditions of the fire, to determine the quantity of CO so produced. Such experiments were made during the entire working day, September 1, 1881. A succinct statement of the results of these experiments will be found in Table XXVI., and all the figures of this table, except those in columns 3 and 4, are graphically represented on the diagram, Figure 178. Both table and diagram are so plain as to require little explanation. They will be readily understood by any one who will give them a few minutes careful attention. Beginning at the lower left-hand corner of the diagram, it will be seen that a charge of 200 pounds of

TABLE XXVI.

CARBON MONOXIDE PRODUCED BY EXCESSIVELY RAPID FIRING.

A.M. TIME. H. M.	Pounds of coal thrown on the grate. Lbs.	Carbon dioxide in chimney gases. Per centum, CO_2.	Carbon monoxide in chimney gases. Per centum, CO.	Ratio of carbon in CO to total carbon. Per centum.	Pounds of air per pound of coal. Lbs.	Pounds of coal burned each half hour. Lbs.	Ratio of loss by CO to full power of coal. Per centum.
1	2	3	4	5	6	7	8
6:15	200						
6:45	200						
7:15	200						
7:45	200						
8:15	200						
8:45	200						
9:		5.12	2.54	43.80	33.2	83.81	27.84
9:15	200						
9:30		5.55	2.99	45.85	29.5	93.75	29.14
9:45	200						
10:		7.79	3.99	44.63	21.4	129.24	28.37
10:15	200						
10:30		7.70	4.61	48.47	20.1	157.60	30.81
10:45	200						
11:		7.82	4.70	48.57	19.8	139.68	30.88
11:15	200						
11:30		8.01	4.81	48.55	19.3	143.30	30.86
12 M.					19.3	143.30	
12:30		15.21	.25	2.52	19.3	143.30	1.60
12:45	200						
1:					20.05	137.94	
1:30		14.11	.21	2.28	20.8	132.96	1.49
2:					21.05	137.94	
2:30		13.62	.33	3.67	21.3	129.85	2.31
2:45	200						
3:		14.50	.48	4.95	19.4	142.56	3.14
3:30		13 18	.29	3.34	22.	125.34	2.12
3:45	200						
4:		14.96	.38	3.84	19.3	143.30	2.44
4:30		14.18	.41	4.35	20.3	136.25	2.76
4:45	200						
5:		13.01	.41	4.72	22.	125.34	3.00
Mean quantity of air					21.653		
Mean of all but two first					20.36		
Mean ratio of loss; first, 6 %							29.65
Mean ratio of loss; last, 8 %							2.36

coal—anthracite, egg size—was thrown on the fire-grates, upon a banked fire, started up at 6:15 A.M., and a like charge every 30 minutes thereafter until 11:15 A.M.

After an interval of 1 hour and 30 minutes, at 12:45 P.M., 200

pounds was again thrown on the fire; and again at 2:45, 3:45, and 4:45 at intervals, respectively, of 2 hours, 1 hour and 1 hour. Thus, the firing was, for 5 hours 15 minutes, up to 11:15 A.M., at the uniform rate of 400 pounds per hour, equal to 16 pounds per square foot of fire-grate per hour; and after 11:15 A.M., it was at the mean rate of 145.45 pounds per hour, equal to 5.82 pounds per square foot of fire-grate per hour—only 36 per cent. as much.

Beginning at 9 A.M., samples of gas were obtained and analyzed half-hourly, except at the hours of 12 M., and 1 and 2 P.M., when there was, in each case, an interval of an hour. The half-hourly samples were taken during the whole preceding half hour, and the hourly samples during the whole preceding hour, so that the whole day from half-past eight is covered by the analyses of the gases. The ratio, per cent. of CO_2 and of CO to the total quantity of dry flue gases, is given in columns 3 and 4 of Table XXVI., but these figures are not represented on the diagram, Fig. 178.

In column 5 of the table, represented by line EF of the diagram, the proportion of coal burned to CO is given as a *per centum* of all the carbon in the coal. During 2 hours and 30 minutes, 9:00 to 11:30 A.M., the mean is 46.64 per cent., showing that only 53.36 per cent. was completely burned to CO_2. The number of pounds of atmospheric air found in the flue gases for each pound of coal consumed, given in column 6 of the table, and represented by line IJ of the diagram, was rather small, and nearly uniform; the mean for 8 hours being 21.65 pounds, and for 7 hours, after 10:00 A.M., only 20.36 pounds. The ratio of heat lost by CO to the full heating power of the coal is given in column 8 of the table, and is represented by line GH of the diagram.

This loss is obviously less than the whole quantity of CO produced, because *some* heat is evolved in burning carbon to CO.

While carbon burned to CO_2 produces, per pound, 14,544 British thermal units, the same quantity burned to CO produces but 4,451 of the same heat units. The loss ($= 14544 - 4451 = 10093$ British thermal units) is about 69.39 per cent., and the numbers in column 8 would be 69.39 per cent. of those opposite in column 5, if carbon were the only combustible in the coal, as it is in coke. But there is, in fact, an appreciable quantity of hydrogen in this coal, probably united with carbon as some one or more of the hydrocarbons, useful as fuel, and this hydrogen loses nothing in consequence of the formation of CO; and the effect of this circumstance is to reduce the ratio of the loss by CO to about 63.5 per cent.

The losses to be accounted for, to be guarded against, and to be reduced to a minimum, in the combustion of coal in the furnaces of steam boilers (aside from external radiation from boiler and brickwork), are all embraced as classified under the five heads, B, C, D, E and F, in the following list.

A = Pounds off flue gases per pound of coal.

A—a = Pounds of atmospheric air per pound of coal.

B = Heat carried off by flue gases (exclusive of vapor contained in these gases).

C = Heat lost by water in the coal.

D = Heat lost by vapor in the air.

E = Heat lost by CO in the flue gases.

F = Heat lost by hydrogen in the flue gases.

a = Number of pounds of carbon in 100 pounds of coal.

b = Number of pounds of hydrogen in 100 pounds of coal,

c = Number of pounds of water in 100 pounds of coal.

d = Number of pounds of ash in 100 pounds of coal.

e = Number of pounds of CO_2 in 100 pounds of flue gases.

f = Number of pounds of CO in 100 pounds of flue gases.

g = Number of pounds of hydrogen in 100 pounds of flue-gases.

h = Proportion of vapor in atmospheric air.

k = Number of British thermal units developed by 1 pound of coal perfectly burned; ascertained by analysis.

n = Temperature of external air; degrees F.

p = Temperature of escaping gases; in smoke-box, with natural draft, or in blower, with the warm-blast apparatus.

To compute the number of pounds of dry flue gases, per pound of coal consumed:

$$A = \frac{a}{.27273e + .42857f} \qquad (1)$$

That is:—Divide the number of pounds of carbon found by analysis in 100 pounds of coal (a), by the sum of $\frac{3}{11} = .27273$ of the CO_2, and $\frac{3}{7} = .42857$ of the CO, found by analysis in the flue gases. The quotient will be the number of pounds of dry flue gases per pound of coal consumed.

EXAMPLE.

We find, for instance, that during the week I, ending May 20, 1882, the mean number of pounds of CO_2 in 100 pounds of flue

gases (days), was 12.27; and of CO, 0.18 pounds; and that the number of pounds of carbon in 100 pounds of coal was 82.92.

Then, $$\frac{3 \times 12.27}{11} = 3.342727$$

and $$\frac{3 \times 0.18}{7} = \underline{0.077143}$$

$$.27273e + .42857f = 3.419870$$

and $$\frac{82.92}{3.41987} = 24.2 = A.$$

$A - a = 24.2 - .8292 = 23.37 =$ the number of pounds of atmospheric air in flue gases per pound of coal consumed.

To find the heat carried off by the flue gases (exclusive of vapor contained in these gases), in terms of the full heating power of the coal.

$$B = \frac{A \times .238 \times (p - n)}{k} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

That is, multiply the number of pounds of flue gases per pound of coal consumed, by .238, which is the mean specific heat of the mixed flue gases; and this product by the difference in temperature in degrees Fahrenheit between the external air and the escaping gases—at the smoke-box, with natural draft, or, at the blower, with the warm-blast apparatus; and divide this second product by the number of British thermal units expressing the full heating power of the coal.

EXAMPLE.

$A = 24.2$; $p = 164°$; $n = 49°$; $p - n = 115°$; $k = 13139$.

Then: $$\frac{24.2 \times .238 \times 115}{13139} = .0504 = B = 5.04\%.$$

To find the heat lost by water in the coal, in terms of the full heating power of the coal.

$$C = \frac{(c + 9b) \times (1076 - n + .48p)}{100k} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

That is, to the number of pounds of water in 100 pounds of coal, add 9 times the number of pounds of hydrogen in 100 pounds of coal, and multiply the sum by the number 1076 *diminished* by the

number of degrees F. expressing the temperature of the external air, and increased by 0.48 times the number of degrees F. expressing the temperature of the escaping flue gases—at the smoke-box, with natural draft, or at the blower, with the warm-blast apparatus; and divide the product by 100 times the number of British thermal units expressing the full heating power of the coal.

The quotient will be the loss by water in the coal, the quantity sought.

EXAMPLE.

Let $c = 2.39$; $b = 1.80$; $n = 49°$ F.; $p = 164°$; 0.48 = specific heat of steam; $.48p = 79$, and $100k = 1313900$. Then:

$$C = \frac{[2.39 + (9 \times 1.80)] = 18.59 \times (1076 - 49 + 79 = 1106)}{1313900} =$$

$.0156 = 1.56\%$.

To find the loss of heat by vapor in the air, in terms of the full heating power of the coal expressed in British thermal units:

$$D = \frac{(A - \frac{a}{100}) \times h \times .48\,(p - n)}{k}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

That is, from the number of pounds of flue gases per pound of coal consumed, subtract one one-hundredth part of the number of pounds of carbon in 100 pounds of coal; and multiply this difference by the proportion of vapor in the air as ascertained by the hygrometer, and by 0.48 times the difference between the number of degrees F. expressing the temperature of the escaping flue gases (at smoke-box, or blower, as the case may be), and the number of degrees F. expressing the temperature of the external air; then divide the continued product by the number of British thermal units expressing the full heating power of the coal. The quotient will be the loss of heat by vapor in the air, in terms of the full heating power of the coal.

EXAMPLE.

Let $A = 24.2$; $a = 82.92 \therefore \frac{a}{100} = .8292$; $h = 1.80\%$; $p = 164°$, $n = 49°$, $p - n = 115°$ F. and $k = 13139$. Then:

$$D = \frac{(24.2 - .8292) \times .018 \times .48 \times 115}{13139} = .0018 = 0.18\%.$$

To find the heat lost by carbon monoxide in the flue gases, in terms of the full heating power of the coal expressed in British thermal units.

$$E = \frac{\frac{3}{7}f \times 101a}{(\frac{3}{11}e + \frac{3}{7}f) \times k} = \frac{.42857f \times 101a}{(.27273e + .42857f) \times k} \quad . \quad . \quad (5)$$

That is, multiply three-sevenths ($\frac{3}{7} = .42857$) of the number of pounds of CO found by analysis in 100 pounds of flue gases, by 101* times the number of pounds of carbon found by analysis in 100 pounds of coal; and divide this product by the continued product of the number of British thermal units expressing the full heating power of the coal, multiplied by three-elevenths of the CO_2 and by three-sevenths of the CO, in pounds found by analysis in 100 pounds of flue gases. The quotient will be the loss of heat caused by the CO in the flue gases, in terms of the full heating power of the coal expressed in British thermal units.

EXAMPLE.

Let $f = 0.18\%$; $a = 82.92\%$; $e = 12.27\%$, and $k = 13139$. Then:

$$E = \frac{(\frac{3}{7} \times .18) \times (82.92 \times 101)}{[(\frac{3}{11} \times 12.27) + (\frac{3}{7} \times .18)] \times 13139} = .0144 = 1.44$$

To find the heat lost by hydrogen in the flue gases, in terms of the full heating power of the coal, expressed in British thermal units.

$$F = \frac{A \times g \times 620.32}{k} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

That is, multiply the number of pounds of flue gases per pound of coal consumed by the number of pounds of hydrogen found by analysis in 100 pounds of flue gases, and by 620.32 (= one one-hundredth part of the number of B. t. u. expressing the full heating power of one pound of hydrogen); and divide the product by the number of British thermal units expressing the full heating power of one pound of the coal as determined by analysis. The quotient will be the loss by unburned hydrogen in the flue gases, in terms of the full heating power of the coal, expressed in British thermal units.

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* 101, put for $\frac{10093}{100} = 100.93$; the sum 10093 being 14544 − 4451, see p. 795.

EXAMPLE.

Let $A = 24.2$; $g = 0$; $k = 13139$. Then,

$$F = \frac{24.2 \times 0 \times 620.32}{13139} = 0.$$

No hydrogen has ever been detected in the flue gases, and it seems little likely that any ever escapes from the furnace unburned. Some hydrocarbons, in natural gas, deposit a portion of their carbon in a solid mass behind the bridge wall, especially if introduced into the furnace at too high a temperature; but it is probable that all the free hydrogen present, and all which is combined with carbon, that is, all that is not already burned to water, is so burned in any furnace fire. If, however, any hydrogen should ever be found in the flue gases, its quantity inserted in place of 0 in the above example, will bring out the resulting loss of heat. The sum of the losses B, C, D and E, is as follows:

B, loss of heat carried off by the dry flue gases	5.04
C, loss of heat by water in coal	1.55
D, loss of heat by vapor in the air	0.18
E, loss of heat by carbon monoxide	1.44
Total losses at the chimney, per cent	8.21
Add to this the loss by radiation from boiler and brick-work; a quantity varying with the temperature of the external air and with the conditions of each case, but in this case	4.00
Total sum of losses, per cent	12.21
Efficiency of boiler, per cent	87.79
	100.00

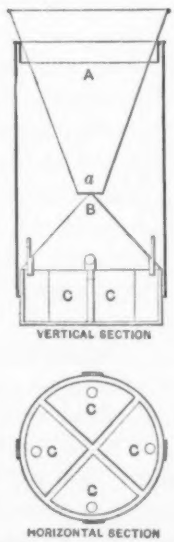
Various small savings can be made in ways already pointed out, which, in the aggregate, may be brought up to the 2.21 per cent. required in order to make the net efficiency 90 per cent. There is still five per cent. of the heat carried off by the flue gases at the moderate temperature of 164° F., only 115° F. above the temperature of the external air.

Part of this may sometimes be saved by warming water after the gases leave the abstractor, and possibly a little may be saved by improvements in the abstractor itself.

But according to present appearances 90 per cent. is about the maximum efficiency attainable by the best possible boiler with warm-blast apparatus, and *that* should be steadily aimed at and pretty nearly attained.

ASHES AND RESIDUE.—All ashes and residue withdrawn from

the furnace and ash-pit during each weekly experiment were kept together under cover, until the fire was drawn at the end of the week's work, at midday on Saturday. The fire was allowed to burn pretty low on Saturday; still, as steam was kept up, there was some partially burned coal on the grates when the fire was drawn, and some water was used to quench this coal; but only enough to cool it by evaporation below the point of ignition, so that the ashes and residue, when cold, might be considered to be as dry as the hygrometric state of the air would permit. After division, as has been already said, into five grades, namely (*a*), unburned coal (a small quantity); (*b*), clinker, partly vitreous; (*c*), coarse residue, which would not pass through a screen with hexagonal meshes five-eighths of an inch in short diameter; (*d*), finer residue, passing through said hexagonal meshes, but not passing through a screen with three meshes to an inch each way; and (*e*), ashes which passed through said screen; each grade was weighed by itself, kept separate, and sampled for analysis. The first grade (*a*) was pulverized and sampled in the same manner as the week's coal. The second grade (*b*), clinker, was sampled, by taking a part of almost every lump, making as fair a selection as possible. This grade, which sometimes reached 500 pounds in a week—more than one-fifth of the whole quantity of ashes and residue—was nearly barren of carbon, while the first grade, although small in quantity, was little inferior in carbon to fresh coal.



ORE SAMPLER, FOR OBTAINING SAMPLES OF ASHES AND RESIDUE.

FIG. 179.

The third grade (*c*), the fourth grade (*d*), and the fifth grade (*e*), were sampled by passing them twice in succession through an ore-sampler, shown in Fig. 179. Placed in the conical hopper A, they passed though its open end (*a*), concentrically upon the apex of the right cone B, which distributed them evenly on all sides in a sheet, growing gradually thinner toward its base, near which were placed four tubes, one inch in inside diameter, equidistant in a circle forty inches in circumference, so that each tube was equal in diameter to one-tenth of the quadrant in which it was set.

Of each tube, the side facing the center of the cone and above its surface, was cut away so as to present an open mouth, one inch wide, towards the descending sheet of ashes or cinders, one-tenth of which they received and conducted into the quadrant-shaped cups beneath them in the base of the sampler.

When these cups were full, or when all the ashes or cinders of any grade had been passed through the sampler, the cups were taken out, emptied, and replaced in position; and their contents were again passed through the sampler. By this process, supplemented by a small correction (found by weighing the whole quantity, and the quantity delivered each time into the cups), for any variation in the actual dimensions of the sampler from the exact one-tenth contemplated, a known proportion, about one one-hundreth part of each grade of ash and cinders was obtained, of presumably average quality. Each sample so obtained was then pulverized, and a smaller sample obtained by subdivision in the manner usual in treating ores, was finally bottled, labeled, and put aside for analysis in its turn. The fifth grade (*e*) was, after sampling, again subdivided by passing its finer portion through a sieve of brass wire-cloth of forty meshes to an inch each way. The portion which passed through this sieve, which was much the larger portion, was almost wholly incombustible ash—only about 5 per cent. of it being carbon, while the portion remaining on the sieve, although small in quantity, was almost wholly pure coal, apparently resulting from decrepitation. The weight of each grade being known, and the proportion of carbon in each being ascertained by analysis, it of course follows that the total quantity of carbon in ashes and residue becomes known.

It is probably a safe assumption that no combustible save carbon remains, since volatile hydrocarbons must be either burned to CO_2 and water, or driven off by the heat of the fire.

Results finally obtained in the manner above described may be checked by a method much easier, and little less accurate, even in theory, while its simplicity eliminates an accumulation of errors of observation, and makes it, in practice, quite as accurate.

This second method is as follows:

The analysis of the coal thrown on the fire-grates during the week gives the proportion of ash it contains, and this proportion applied to the weight of the coal consumed during the week, after deducting the weight of the unburned coal picked out of the ashes and residue [grade (*a*)], gives the quantity of "ash" proper, in the

week's ashes, cinders, and clinkers of all grades (*b*), (*c*), (*d*), and (*e*). It follows that the excess of the combined weight of ashes and residue of these four grades, over the weight of ash as determined by analysis of the coal, is equal, or nearly equal to the quantity of carbon contained in the ashes and residue of these four grades. It would be exactly equal if all the unburned coal could be picked out; but this can hardly ever be the case, since no inconsiderable quantity goes through the grates in particles too fine to be picked out, and can be segregated only by subdividing grade (*e*), after pulverization, by means of a fine sieve, as above described. In treating a coal which decrepitates very badly, it may be necessary to sample and analyze the ashes and residue, as herein described. Such is the Rhode Island coal, large lumps of which sometimes crumble to fine black sand and sift through a thick fire to the ash-pit, with startling suddenness, without becoming too hot to be held in the hand. But in the use of most, perhaps all, of the Pennsylvania anthracites, the second and simpler mode of procedure I have described will be found sufficiently accurate; and this was the method pursued in the later portion of our work.

Boiler Pressure by Steam Gauge.—One of Edson's Pressure Recording Gauges was connected with the boiler, and kept in operation throughout the whole duration of the trials. A set of the diagrams from this gauge running through the days and nights of the week ending July 23, 1881 (week B), is given in Fig. 180*a*,

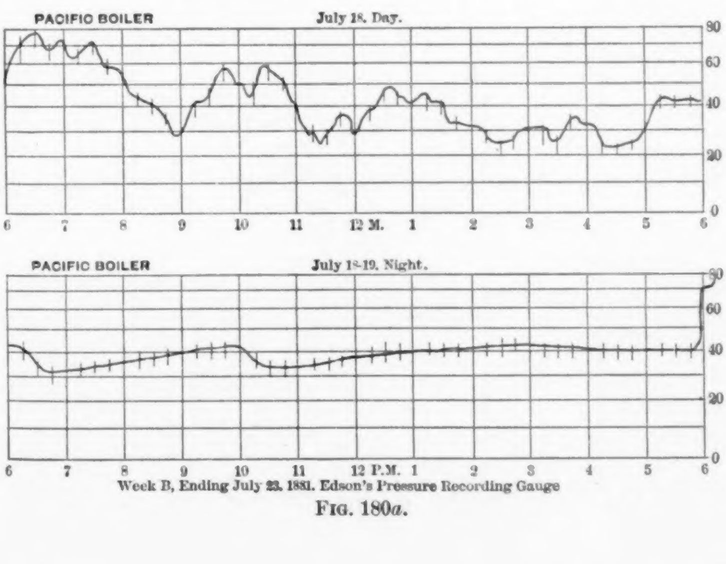
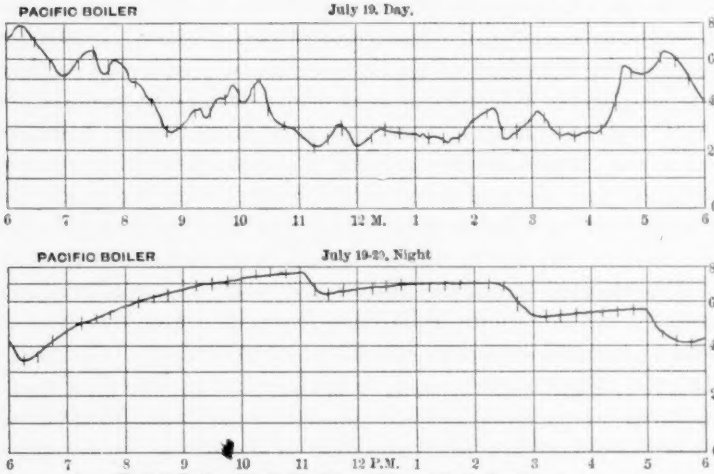


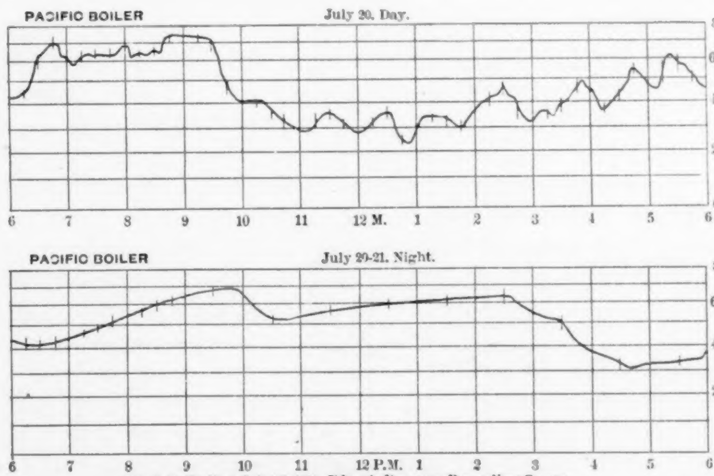
Fig. 180*a*.

b, *c*, *d*, *e*, and *f*, reduced by photography to two-thirds of the size of the diagrams. The upper diagram on each set exhibits the pressures during the day—6 A.M. to 6 P.M., except on Saturday, when the day closed at 12 M. They show very clearly the extremely unequal demand for steam, but very inadequately, for two reasons:—*first*, because the fire was urged and evaporation was accelerated whenever steam pressure was rapidly drawn down, and in



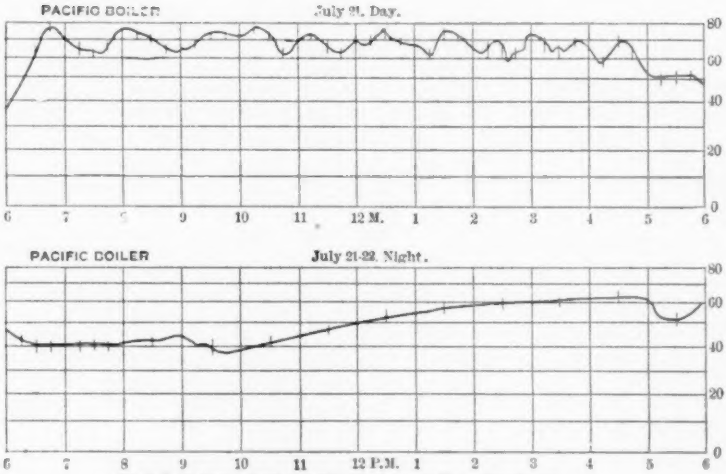
Week B, Ending July 23, 1881. Edson's Pressure Recording Gauge

FIG. 180*b*.



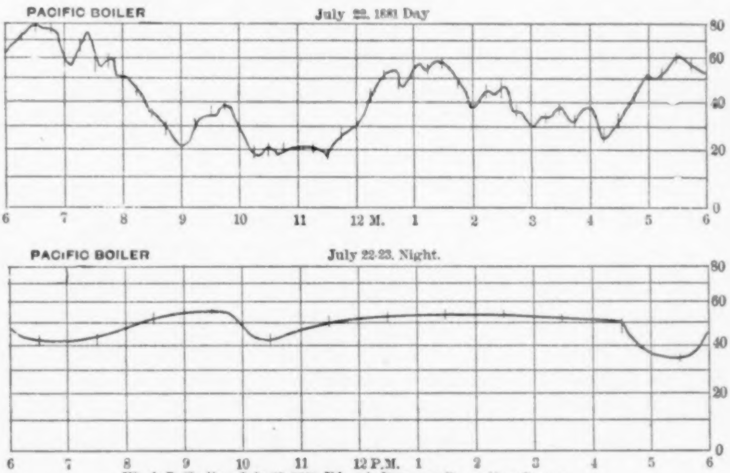
Week B, Ending July 23, 1881 Edson's Pressure Recording Gauge

FIG. 180*c*.



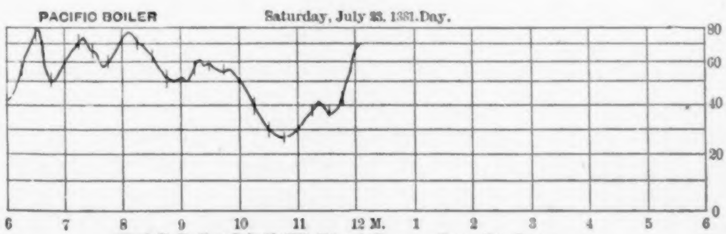
Week B, Ending July 23, 1881. Edson's Pressure Recording Gauge

FIG. 180*d*.



Week B, Ending July 23, 1881. Edson's Pressure Recording Gauge

FIG. 180*e*.



Week B, Ending July 23, 1881. Edson's Pressure Recording Gauge

FIG. 180*f*.

some degree checked when it rose; and *second*, on account of the smaller scale on which this gauge records pressures in the upper portion of its register, the 10 lbs., 70 to 80, occupying only half as much space as the lower 10 lbs., above 0. The effect of this is to mask the irregularities, in some degree, making them appear much less than if the scale were uniform throughout.

The Edson gauge is excellent for the purpose of recording the general state of the pressure; but its indications are not sufficiently accurate for numerical calculation, if for no other reason, on account of the small scale on which it works.

It was therefore necessary to take readings of an accurate pressure gauge at stated intervals, as accurately spaced in time as possible.

Such readings were taken every quarter of an hour during the day, and part of the time by night also; but the greater uniformity at night led us, soon, to take readings at the hours and half-hours only. The gauge was a ten-inch Bourdon test gauge, made by the American Steam Gauge Company, which had never before been used except for comparison with other gauges. It had been compared many times with a mercury column, with which it agreed quite closely, and had not been used after having been so tested, until it was used in these experiments.

It was connected with the boiler by a branch pipe from the pipe leading to the Edson gauge, and as the pressure was shut off from the test gauge except when a reading was to be taken for record, a slight reduction of pressure took place at the Edson gauge whenever the stop-cock of the test gauge was opened, producing by the downward motion of the marking pencil, and a little recoil on its rising at the close, a short mark crossing the trace of the Edson recording gauge, which indicates the moment of the reading, and the point in the trace with which the reading is to be compared. A mean was taken of the readings of the test gauge for each day, and for each night, and a general mean for each week, of the days and also of the nights. It will be observed that the diagrams, Fig. 180 *a*, *b*, etc., are reversed in direction from the original diagrams, which read from right to left. This is merely for convenience of reading in the ordinary manner, from left to right. I have not thought it worth while to reproduce here these diagrams for more than a single week, since these fairly represent them all in general character.

CAPACITY OF BOILER AT VARIOUS HEIGHTS OF WATER LINE AND AT VARIOUS PRESSURES.—A scale, graduated to inches and tenths of an inch, was attached to each glass water gauge in such a manner that the surface of the water in the glass tube could be readily referred to it, and readings of this gauge were recorded every quarter of an hour. It was practically impossible to maintain a uniform water level, and it was found to be inconvenient to bring the water at the close of an experiment, at noon on Saturday, to agree exactly with that at starting, on Monday morning. It was therefore necessary to ascertain the true difference in quantity due to any observed difference in height of surface; and convenience required that this should be ascertainable by inspection of a table. The subjoined table, Table XXVII., was therefore constructed, showing the capacity of the boiler expressed in pounds avoirdupois of water at the zero of the scale, and at each inch of height above that zero, with differences for ascertaining by interpolation the quantity for parts of inches. The height, in inches of the water surface above the zero of the scale, is given in the left-hand column of the table, which is in two parts. But the weight of a given volume of water varies with its temperature, which corresponds with the absolute steam pressure. The table is, therefore, computed for 17 different pressures, from 0 = one atmosphere = 14.7 pounds per square inch absolute, up to 80 pounds steam-gauge pressure = 94.7 pounds absolute, at intervals of 5 pounds, as indicated by the figures at the head of the columns, which are steam-gauge pressures; with columns of differences for ascertaining by interpolation the quantity of water at intermediate pressures. The zero of the scale is near the lower end of the tube, and about 3.08 inches above the top of the upper row of flues, and 9.33 inches above the center of the shell.

EXAMPLE OF THE USE OF THE TABLE.

In the experiment for the week ending May 20, 1882, at 6 h. 32 m. A.M. on Monday, May 15, the reading at the scale of the glass water gauge was 5.3 inches; pressure of steam by steam gauge, 15 pounds. At 12 M. on Saturday, May 20, water stood at 3.0 inches, steam at 50 pounds.

Then, by consulting the table, we find in the column headed 15, opposite the height of 5 inches, water in boiler	13,546 lbs.
Difference for 1 inch = 424 lbs.	
And 424 × .3 =	127 lbs.
Pounds of water at starting	13,673 lbs.
The column headed 50, opposite the height 3 inches, water in boiler	12,404 lbs.
Number of pounds less at the end of the experiment than at its beginning	1,269 lbs.
Number of pounds fed into the boiler during the experiment	156,214 lbs.
Number of pounds of water evaporated during the experiment	157,483 lbs.

Since the two boilers are alike, this table applies equally well to both.

Radiation from Brick-work.—An attempt was made to measure the quantity of heat lost by radiation from the brick-work, which, although unsatisfactory, yet seems to possess some interest, and will be briefly noticed.

The Apparatus.—Two tin-plate vessels were provided, each twelve inches square and one inch thick, closed on all sides. On one side, near the corners, there were two rings by which the vessels could be hung up upon nails driven into the brick-work. In the upper edge, when so suspended, there was a tubular orifice, about 0.75 inch in diameter, slightly tapering, for convenient insertion of a cork. Through the cork two small glass tubes were inserted; one, for inflowing water, extending down inside nearly to the bottom of the vessel; the other, for outflowing water, extending but slightly through the cork.

Each of these tubes, near the entrance through the cork into the vessel, was provided with a suitable enlargement, bend and orifice for convenient insertion of a thermometer, to show the temperature of inflowing and outflowing water. Water was supplied from a bucket suspended in an elevated position, and received in a bucket on the floor, surrounded by ice, to reduce loss of weight by evaporation. The edges and the back of the vessels were protected from loss of heat by radiation, at least in some degree, by a hood of cotton flannel filled with eider down; and the edges of this hood were drawn slightly over the naked side next the brick-work, by a gathering-string, to cut off circulating air currents which would carry off heat by convection. Finally, the naked side was coated thickly with dry lampblack, for the better absorption of radiant heat.

TABLE XXVII.

CAPACITY OF BOILER IN POUNDS OF WATER, FOR EACH INCH IN HEIGHT, FROM 0 TO 10 INCHES, AND FOR EACH 5 POUNDS OF STEAM-GAUGE PRESSURE, FROM 0 TO 80 POUNDS.

IN.	0	D.	5	D.	10	D.	15	D.	20	D.	25	D.	30	D.	35	D.
10	15809	101	15708	80	15628	67	15561	61	15500	50	15450	48	15402	44	15358	42
	386		382		381		379		378		376		376		375	
9	15423	97	15326	79	15247	65	15182	60	15122	48	15074	48	15026	43	14983	41
	398		396		394		392		390		390		388		387	
8	15025	95	14930	77	14853	63	14790	58	14732	48	14684	46	14638	42	14596	40
	412		410		408		406		405		403		402		400	
7	14613	93	14520	75	14445	61	14384	57	14327	46	14281	45	14236	40	14196	39
	420		417		414		414		411		410		408		408	
6	14193	90	14103	72	14031	61	13970	54	13916	45	13871	43	13828	40	13788	38
	431		429		427		424		423		422		421		419	
5	13762	88	13674	70	13604	58	13546	53	13493	44	13449	42	13407	38	13369	37
	446		442		440		438		436		435		433		433	
4	13316	84	13232	68	13164	56	13108	51	13057	43	13014	40	12974	38	12936	35
	449		447		444		443		441		439		438		436	
3	12867	82	12785	65	12720	55	12665	49	12616	41	12575	39	12536	36	12500	34
	457		453		453		449		448		446		445		444	
2	12410	78	12332	65	12267	51	12216	48	12168	39	12129	38	12091	35	12056	33
	464		462		458		457		455		454		452		451	
1	11946	76	11870	61	11809	50	11759	46	11713	38	11675	36	11639	34	11605	31
	470		466		464		462		460		459		458		456	
0	11476	72	11404	59	11345	48	11297	44	11253	37	11216	35	11181	32	11149	31

TABLE XXVII.—*Continued.*

CAPACITY OF BOILER IN POUNDS OF WATER, FOR EACH INCH IN HEIGHT, FROM 0 TO 10 INCHES, AND FOR EACH 5 POUNDS OF STEAM-GAUGE PRESSURE FROM 0 TO 80 POUNDS.

IN.	40	D.	45	D.	50	D.	55	D.	60	D.	65	D.	70	D.	75	D.	80	D.
10	15316	41	15275	35	15240	35	15205	32	15173	29	15144	30	15114	28	15086	28	15058	27
	374		373		371		370		370		369		368		368		367	
9	14942	40	14902	33	14869	34	14835	32	14803	28	14775	29	14746	28	14718	27	14691	26
	386		385		384		384		382		382		381		380		380	
8	14556	39	14517	32	14485	34	14451	30	14421	28	14393	28	14365	27	14338	27	14311	26
	399		398		398		396		396		395		394		394		392	
7	14157	38	14119	32	14087	32	14055	30	14025	27	13998	27	13971	27	13944	25	13919	25
	407		405		404		404		403		402		402		400		400	
6	13750	36	13714	31	13683	32	13651	29	13622	26	13596	27	13569	25	13544	25	13519	25
	418		417		416		415		414		413		412		412		411	
5	13332	35	13297	30	13267	31	13236	28	13208	25	13183	26	13157	25	13132	24	13108	24
	431		430		429		428		427		427		425		425		424	
4	12901	34	12867	29	12838	30	12808	27	12781	25	12756	24	12732	25	12707	23	12684	23
	435		435		434		432		431		430		430		429		428	
3	12466	34	12432	28	12404	28	12376	26	12350	24	12326	24	12302	24	12278	22	12256	22
	443		441		439		439		439		437		437		435		435	
2	12023	32	11991	26	11965	28	11937	26	11911	22	11889	24	11865	22	11843	22	11821	21
	449		448		448		447		445		445		444		443		442	
1	11574	31	11543	26	11517	27	11490	24	11466	22	11444	23	11421	21	11400	21	11379	20
	456		454		453		452		451		450		449		449		448	
0	11118	29	11089	25	11064	26	11038	23	11015	21	10994	22	10972	21	10951	20	10931	19

The method of using this simple apparatus consisted of noting at frequent and regular intervals the temperature of the inflowing and outflowing water, and in ascertaining the quantity of water flowing through each vessel in a known interval of time.

TRIAL OF THE APPARATUS.—On the 10th of August, 1881, both these radiometers were placed, side by side, on the smoke-box cover of the Pacific Boiler—marked, for distinction No. 1 and No. 2—and streams of water, supposed to be nearly alike, were set to flow through them. A first experiment of one hour, 8 h. 45 m. to 9 h. 45 m. A.M., was immediately followed by a second, of 3 h. 30 m.—10 h. 0 m. A.M. to 1 h. 30 m. P.M. Observations of temperatures were noted every 15 minutes. The water was weighed at the close of each experiment. The results are given in Tables XXVIII. and XXIX.

The line marked "B. t. u., total," is obtained by multiplying the number of British thermal units corresponding to the increase of temperature, by the number of pounds of water to which such quantity of heat was imparted, in each case.

TABLE XXVIII.

RADIATION: EXPERIMENT NO. 1.

RADIOMETER NO. 1.			RADIOMETER NO. 2.		
Water heated, lbs.		8.797	Water heated, lbs.		15.563
Mean t, initial		78.16°	Mean t, initial		77.32°
Mean t, final		95.40°	Mean t, final		93.10°
Mean increase, deg.		17.24°	Mean increase, deg.		15.78°
TIME.	TEMPERATURE OF WATER. Initial. Degrees F.	TEMPERATURE OF WATER. Final. Degrees F.	TIME.	TEMPERATURE OF WATER. Initial. Degrees F.	TEMPERATURE OF WATER. Final. Degrees F.
8:45	76.8°	98.8°	8:45	76.5°	91.0°
9	77.4°	92.3°	9	76.3°	93.8°
9:15	78.0°	97.9°	9:15	76.9°	93.0°
9:30	78.7°	93.0°	9:30	77.0°	92.0°
9:45	79.9°	95.0°	9:45	79.9°	95.7°
Mean	78.16°	95.4°	Mean	77.32°	93.10°
B. t. u.	78.1923	95.4642	B. t. u.	77.3506	93.1612
B. t. u., increase		17.2719	B. t. u., increase		15.8106
B. t. u., total, 1 hr.		151.9409	B. t. u., total, 1 hr.		246.0604

TABLE XXIX.

RADIATION: EXPERIMENT NO. 2.

RADIOMETER NO. 1.			RADIOMETER NO. 2.		
Water heated, lbs.		13.105	Water heated, lbs.		23.699
Mean t, initial		85.37°	Mean t, initial		79.91°
Mean t, final		109.47°	Mean t, final		169.93°
Mean increase, deg.		24.10°	Mean increase, deg.		30.01°
TIME.	TEMPERATURE OF WATER. Initial. Degrees F.	TEMPERATURE OF WATER. Final. Degrees F.	TIME.	TEMPERATURE OF WATER. Initial. Degrees F.	TEMPERATURE OF WATER. Final. Degrees F.
10	87.8°	105.2°	10	79.0°	100.0°
10:15	75.2°	105.3°	10:15	78.0°	105.0°
10:30	78.7°	101.8°	10:30	78.5°	108.5°
10:45	79.6°	103.0°	10:45	79.0°	111.6°
11	79.5°	104.0°	11	79.0°	114.5°
11:15	80.3°	105.0°	11:15	79.5°	117.0°
11:30	80.7°	105.5°	11:30	77.0°	109.0°
11:45	81.7°	106.0°	11:45	79.5°	109.0°
12	87.8°	109.0°	12	88.3°	112.0°
12:15	90.5°	111.2°	12:15	89.1°	112.5°
12.30	93.1°	114.3°	12:30	77.5°	112.2°
12:45	94.7°	120.2°	12:45	79.0°	107.0°
1	96.0°	119.5°	1	78.2°	108.4°
1:15	96.6°	121.0°	1:15	78.6°	110.2°
1:30	78.4°	111.0°	1:30	79.0°	113.0°
Mean	85.37°	109.47°	Mean	79.91°	110.00°
B. t. u	85.4157	109.5784	B. t. u.	79.9458	110.1100
B. t. u., increase		24.1627	B. t. u., increase		30.1642
B. t. u., total, 3.5 hrs		316.6522	B. t. u., total, 3.5 hrs		714.8614
B. t. u., per hour		90.4721	B. t. u., per hour		204.2461
B. t. u., Mean, 4.5 hrs.		104.1318	B. t. u., mean, 4.5 hrs.		203.4015

The tables present some striking anomalies, but some coincidences no less striking.

The result obtained from radiometer No. 1, is very much less than that obtained from No. 2—only 62 per cent. as much in the first hour, experiment No. 1; only 44 per cent. in the following 3.5 hours, experiment No. 2; and for 4.5 hours, taking both experiments together, 51 per cent.

There was certainly no corresponding difference in the *radiation* from the two parts of the smoke-box cover, which were only a few inches apart, and almost certainly of equal temperature. The difference here noted in the *apparent* radiation is at once too large and too uniformly persistent to be explained by any errors of ob-

servation. Two explanations suggest themselves, which, singly or in conjunction, may account for it.

First, radiometer No. 1 may not have been so adjusted to the brick-work as entirely to cut off circulation of air, and consequent loss of heat by convection; and *second*, radiometer No. 2 may have been, to some extent, in contact with the smoke-box cover, so as to receive some heat by conduction. The object of these preliminary experiments was to test the accuracy of the radiometers, and the intention was to divide the area of the brick-work into portions of about one square yard, and to apply the radiometers in turn to each and all of these partial areas. The first results were not satisfactory, and circumstances did not permit the prosecution of this inquiry. The date of these experiments, August 10, 1881, falls in the week ending August 13, week E. The coal burned that week was 14,670, pounds of evaporative power equal to the evaporation of 13.64 pounds of water from and at 212° F., and therefore capable of producing:

$$14670 \times 13.64 \times 965.7 = 193235411 \text{ B. t. u.}$$

Loss from imperfect combustion was 1.81 per cent. The loss by radiation from brick-work, for week E., appears to be 1.39 per cent.; but this is a residuum, and is affected by many small errors. It is therefore proper to take the mean for the six weeks, July 16 to August 20, which was 2.81 per cent.

Now, 1.81 per cent. of 193235411 = 3497561, and
subtracting 3497561

we have 189737850 as the number of British thermal units actually produced in week E. Taking 2.81 per cent. of the heat produced = 5331634 British thermal units, going on day and night, say 132 hours per week, we have $\frac{5331634}{132} = 40391$ B. t. u. per hour. The total radiating surface of the Pacific boiler setting was about 1000 square feet, and dividing by this number the quantity of heat radiated per hour, we have, $\frac{40391}{1000} = 40.39$ B. t. u. per square foot per hour. If this be the mean radiation from the whole outside surface of the brick-work, the rate must be much greater directly opposite the fire. If 2.5 times as much, it would be $40.39 \times 2.5 =$ say, 101, about equal to the quantity shown by radiometer No. 1, and about half as much as appears by No. 2. But these experiments were upon the iron cover of the smoke-box, where the

radiation was probably considerably more rapid than from the brick-work, although the internal temperature was low. Inconclusive as were these experiments, the apparatus appears to have elements of usefulness, and may, with patience and care, yield valuable information.

TRANSMISSION OF HEAT THROUGH BRICK-WORK.—All the heat radiated from the surface of the brick-work must of course reach the surface from within by conduction.

For studying the conduction the following provision was made Round wooden rods, about 1.5 inches in diameter, a little tapering, were laid, horizontally and transversely, in the side wall of warm-blast boiler No. 2, in two rows, respectively 4′ 5″ and 5′ 0″ above the floor, 14 inches apart in each row, those in the upper row being placed centrally over the spaces between those in the lower row. There were 7 in each row, penetrating respectively 4, 8, 12, 16, 20, 24, and 28 inches; the latter, therefore, having only the width of one fire-brick (4.5″) between its extremity and the combustion chamber, about midway between the bridge wall and the pier. On the withdrawal of the rods, holes were left for the insertion of thermometers to the several depths above mentioned.

The position of the deep and shallow holes was reversed in the two rows, so that the 4″ holes in each row were near the 28″ holes in the other. This arrangement will be clearly seen in Fig. 181.

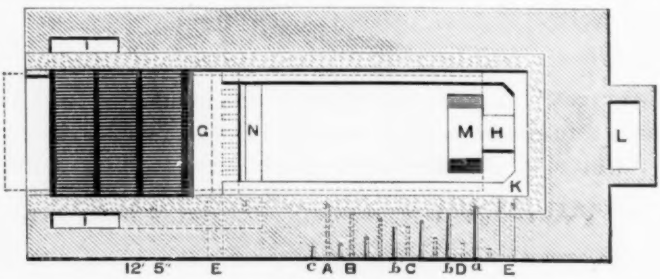


FIG. 181.

HORIZONTAL SECTION OF BRICK-WORK OF WARM-BLAST BOILER; SHOWING THE LOCATION OF HOLES FOR TAKING TEMPERATURES.

Three sets of observations were taken in these holes; the first, during all the working hours of one week, quarter-hourly—Monday morning, September 19, to Saturday noon, September 24, 1881, in the 28-inch hole, A, Fig. 181, in the lower row, located 12′ 5″ from the front end, and 8″ above the level of the lower side of boiler. The observed temperatures are all given in Table XXX., and repre-

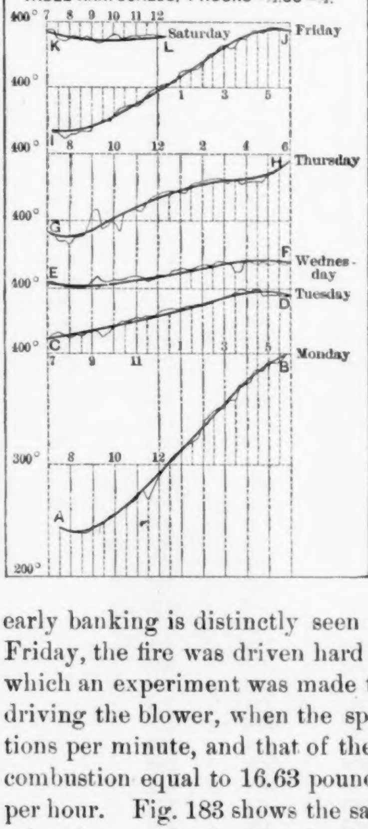
TABLE XXX.

TEMPERATURES OF BRICK-WORK, WARM-BLAST BOILER NO. 1, 12′ 5″ FROM FRONT END,—2′ 5″ ABOVE GRATES,—28″ FROM OUTSIDE, 4.5″ FROM INSIDE OF SIDE WALL,—MONDAY, SEPT. 19, 7 h. 30 m. A.M., TO SATURDAY, SEPT. 24, 12 h. 15 m. P.M., 1881. BY MERCURIAL THERMOMETER: QUARTER-HOURLY READINGS. SEE PROFILE, FIG. 183.

TIME. H.	M.		Monday. 19	Tuesday. 20	Wednesday. 21	Thursday. 22	Friday. 23	Saturday. 24
			Degrees F.	Degrees F.	Degrees F.	Degrees F.	Degrees F.	Degrees F.
7		A.M.		354	344			390
	15			358	344	330	360	392
	30		244	360	344	323	360	384
	45		240	356	341	322	356	383
8			239	355	340	320	360	385
	15		240	358	340	325	360	385
	30		240	357	341	327	363	384
	45		242	360	341	329	363	386
9			242	362	346	349	363	383
	15		246	362	350	350	370	382
	30		248	354	344	334	372	383
	45		254	358	344	340	374	384
10			256	364	346	340	374	388
	15		260	368	346	332	378	384
	30		264	368	348	348	382	334
	45		268	370	348	350	384	384
11			272	372	348	352	386	386
	15		278	372	348	350	386	386
	30		270	374	346	354	394	386
	45		280	374	348	358	397	386
12		M.	294	376	350	362	399	386
	15	P.M.	300	378	352	364	401	386
	30		302	378	354	366	403	
	45		308	380	354	366	404	
1			312	380	354	366	414	
	15		316	382	354	366	415	
	30		320	383	357	372	416	
	45		323	384	358	373	420	
2			337	386	358	373	421	
	15		342	386	358	374	428	
	30		347	388	359	375	430	
	45		350	390	362	374	434	
3			350	390	362	376	438	
	15		358	392	362	374	435	
	30		358	394	356	378	436	
	45		370	398	354	374	444	
4			372	394	364	374	444	
	15		378	398	364	376	450	
	30		384	398	364	376	450	
	45		384	392	366	376	450	
5			392	394	362	384	452	
	15		392	394	364	386	454	
	30		394	394	364	388	452	
	45		400	394	362	392	452	
6				394	364		452	

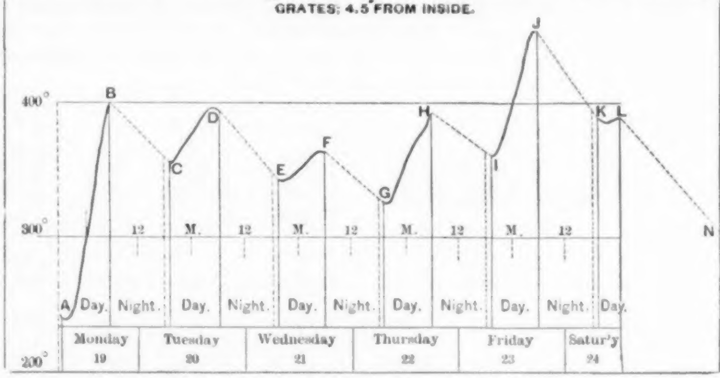
sented graphically in Fig. 182 and Fig. 183. In Fig. 182, the several daily profiles would, if all drawn from the same base-line, confuse each other. The 400° line is therefore raised for each succeeding profile, enough to permit all to be clearly seen. The waving lines connect the points observed, of which there were on Monday, 42, on Tuesday and Wednesday, 45 each, on Thursday 43, on Friday 44, and on Saturday 22, making 241 in all.

Fig. 182. GRAPHICAL REPRESENTATION OF TABLE XXX. SCALES, 4 HOURS = ½″. 80° = ½″.



These waving lines represent, for the most part, if not always, real fluctuations of temperature. Every opening of a fire-door sent a pulse of low temperature through the brickwork, and this was sharply felt so near as 4.5 inches to the source of heat. The smooth curves are intended to represent approximate mean ranges of temperature. On Monday the temperature remained stationary, indeed fell 4° or 5° while the banked fire was opened, and fresh coal put on, but from 8:30 rose sharply, although not quite uniformly, to the close, and reached 400°, a point not again attained until noon of Friday. The effect of light firing and early banking is distinctly seen on Tuesday and Wednesday. On Friday, the fire was driven hard at midday. This was the day on which an experiment was made to ascertain the power consumed in driving the blower, when the speed of the engine was 191 revolutions per minute, and that of the blower, 232, resulting in a rate of combustion equal to 16.63 pounds of coal per square foot of grate per hour. Fig. 183 shows the same mean curves on the same vertical scale, combined with a horizontal scale one-sixth as large, and the intervening nights, in which the temperature is represented by dotted lines, connecting the last observation of each day with the first of the day following. From Saturday noon, when the fire was drawn, the dotted line is seen sloping away to reach some low point on the following Monday morning; but the form of this

Fig. 183.
GRAPHICAL REPRESENTATION OF TABLE XXX. VERTICAL SCALE AS IN FIG. 182, 80° TO $\frac{8}{12}$"; HORIZONTAL SCALE, 24 HOURS TO $\frac{8}{12}$" INCH. IN BRICK-WORK 12'11" FROM FRONT END, 2'5" ABOVE GRATES; 4.5" FROM INSIDE.



curve and of the night curves, is conjectural, no night observations having been taken.

Table XXXI. presents a record of 24 observations at each of 3 holes, severally 8", 16", and 24" in depth, and therefore 20.5", 16.5", and 8.5" from the fire, taken on Monday, February 13, 1882. In Fig. 184, these observations are arranged in the form of three profiles upon the interval of time, 5 h. 45 m. as a base. The assumed means, represented by the full, smooth curves, are here a little more arbitrary; but this is, as will be seen, of no importance.

Fig. 184.
GRAPHICAL REPRESENTATION OF TABLE XXXI. FEBRUARY 13, 1882 TEMPERATURES IN BRICK-WORK.

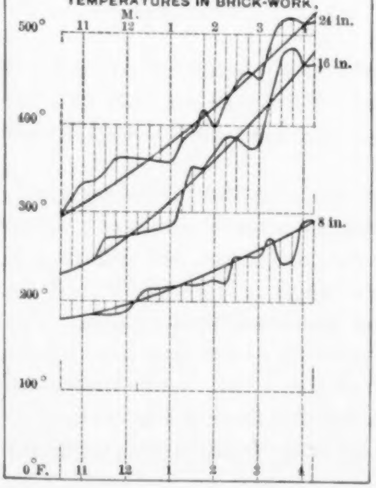


Fig. 185.
QUARTER-HOURLY MEAN TRANSVERSE TEMPERATURES IN BRICK-WORK: FROM TABLE XXXI. FEB. 13. 1882.

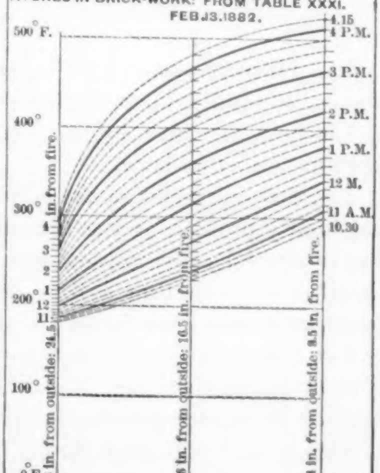


TABLE XXXI.

TEMPERATURE OF BRICK-WORK OF WARM-BLAST BOILER SETTING NO. 1, AT VARIOUS DEPTHS, NAMELY, 8 INCHES, 16 INCHES AND 24 INCHES FROM THE OUTER SURFACE: 10 h. 30 m. A.M. TO 4 h. 15 m. P.M., MONDAY, FEBRUARY 13, 1882. SEE FIG. 184. 5 FEET ABOVE FLOOR.

TIME. H. M.	8 inches from outside. Deg. F.	16 inches from outside. Deg. F.	24 inches from outside. Deg. F.	TIME. H. M.	8 inches from outside. Deg. F.	16 inches from outside. Deg. F.	24 inches from outside. Deg. F.
10 30 A.M.	180	230	294	1 30 P.M.	220	352	392
45	182	232	316	45	220	348	415
11	184	242	332	2	224	368	397
15	186	246	334	15	222	385	425
30	186	272	344	30	252	384	446
45	186	272	361	45	252	374	459
12 M.	190	274	361	3	252	376	453
15 P.M.	203	276	360	15	274	431	490
30	215	279	359	30	244	480	513
45	215	282	357	45	248	487	519
1	217	286	356	4	290	468	513
15	220	311	381	15	293	469	515

Fig. 185 represents these assumed mean temperatures as a succession of profiles upon the thickness of brick-work they embrace (16 inches), as a base, by full lines at the hours, and dotted lines at the quarters of an hour.

Fig. 186 represents in the same manner as the preceding figure, the hourly profiles, by the temperatures actually observed; agreeing in general configuration with the last figure, but differing in detail, and presenting a little less range in consequence of the omission of two lines before **11**, and one line after **4** o'clock.

Fig. 187 is similar to the two preceding, except that here all the quarter-hourly lines are drawn from the actual observations, and embody all the irregularities of the waving lines of Fig. 184.

This table is noticeable for the high temperature found, especially

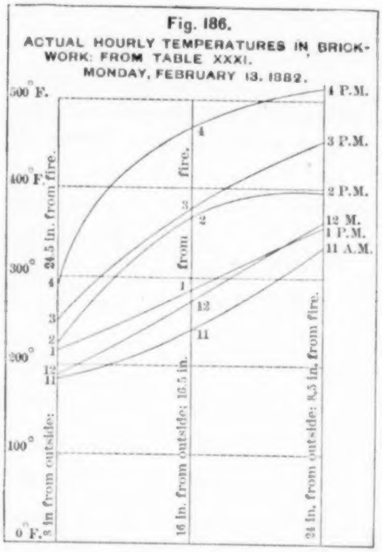


Fig. 186.
ACTUAL HOURLY TEMPERATURES IN BRICK-WORK: FROM TABLE XXXI.
MONDAY, FEBRUARY 13. 1882.

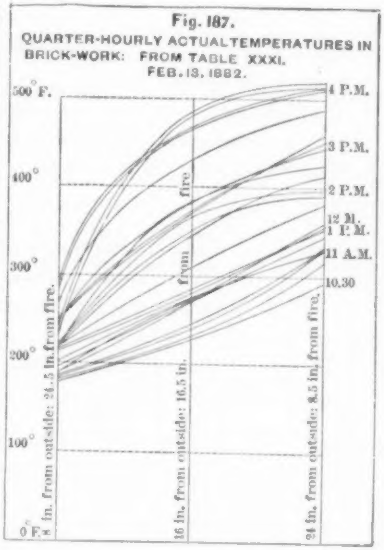


Fig. 187.
QUARTER-HOURLY ACTUAL TEMPERATURES IN BRICK-WORK: FROM TABLE XXXI.
FEB. 13. 1882.

in the 16-inch hole, and notably in the last five observations—431 to 487 degrees.

Table XXXII. embraces 34 sets of observations in 3 holes severally, 4″, 16″, and 28″ deep. The 16″ hole is not identical with the 16″ hole of Table XXXI., but is only 7″ from it horizontally, and

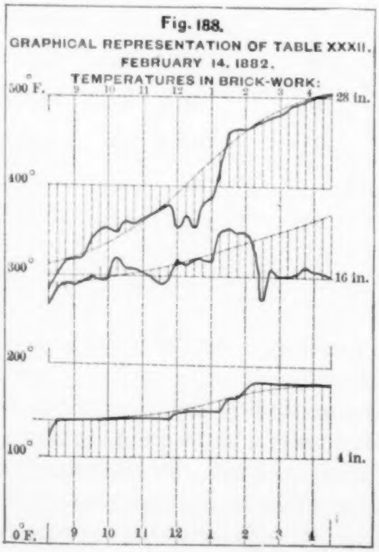


Fig. 188.
GRAPHICAL REPRESENTATION OF TABLE XXXII.
FEBRUARY 14. 1882.

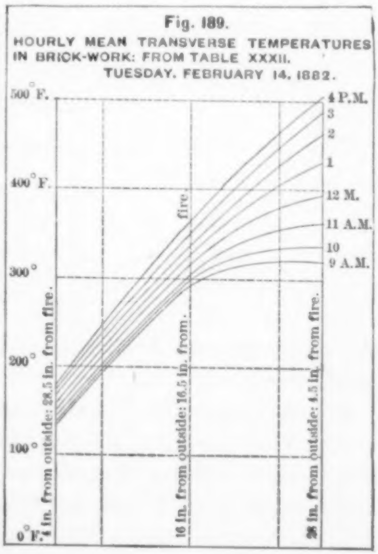


Fig. 189.
HOURLY MEAN TRANSVERSE TEMPERATURES IN BRICK-WORK: FROM TABLE XXXII.
TUESDAY, FEBRUARY 14. 1882.

TABLE XXXII.

TEMPERATURE OF BRICK-WORK OF WARM-BLAST BOILER SETTING NO. 1, AT VARIOUS DEPTHS, NAMELY, 4 INCHES, 16 INCHES, AND 28 INCHES FROM THE OUTER SURFACE: 8 h. 15 m. A.M. TO 4 h. 30 m. P.M., TUESDAY, FEBRUARY 14, 1882. 5 FEET ABOVE FLOOR.

TIME. H. M.	4 inches from outside. Deg. F.	16 inches from outside. Deg. F.	28 inches from outside. Deg. F.	TIME. H. M.	4 inches from outside. Deg. F.	16 inches from outside. Deg. F.	28 inches from outside. Deg. F.
8 15 A.M.	118	267	283	12 30 P.M.	150	319	355
30	140	285	297	45	150	319	382
45	140	292	316	1	150	317	389
9	141	290	321	15	150	350	418
15	142	294	323	30	164	353	460
30	141	299	341	45	166	349	466
45	141	296	352	2	175	349	466
10	141	296	352	15	182	330	471
15	142	320	348	30	182	274	476
30	142	308	362	45	180	308	479
45	142	306	359	3	180	300	481
11	142	302	365	15	181	300	488
15	142	298	370	30	180	362	495
30	142	292	376	45	180	310	498
45	142	296	382	4	180	306	503
12 M.	149	318	354	15	180	304	506
15 P.M.	150	313	369	30	180	298	509

the same distance vertically, and therefore only 10″ from it in a direct line.

The three profiles, Fig. 188, are separated by 12 inches of brick-work. They are noticeable for the sudden rise in the first 15 minutes, at 4″, and in the first 30 minutes, at 16″ and 28″; for the great depression, 12 M. to 1 P.M., at 28″, the still greater depression, 2:30 to 4:30, at 16″, caused by keeping fire-doors open to prevent blow-

ing off steam at the safety-valve; and in the 4″ hole by the two well marked level lines—8:30 to 11:45, and 2:15 to 4:30, with the intermediate steps. The dotted curves which indicate assumed means in this figure are rather violent assumptions, especially at 16″; but this, again, will be seen to be of no importance.

Fig. 189, showing hourly mean transverse temperatures, Fig. 190, showing hourly actual transverse temperatures, and Fig. 191, showing quarter-hourly actual transverse temperatures, being similar in construction to figures already described in connection with Table XXIX., require no comment.

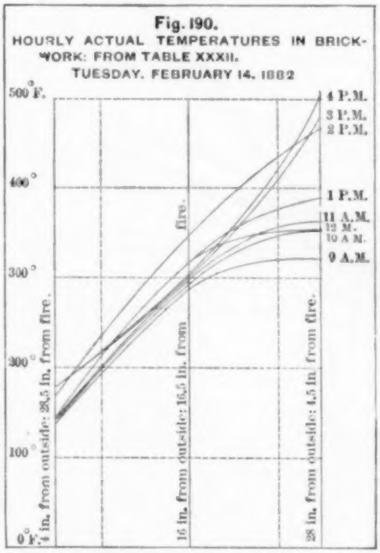


Fig. 190. HOURLY ACTUAL TEMPERATURES IN BRICK-WORK: FROM TABLE XXXII. TUESDAY, FEBRUARY 14, 1882

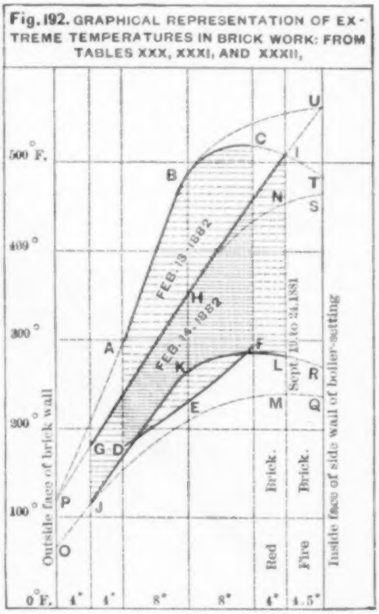


Fig. 192. GRAPHICAL REPRESENTATION OF EXTREME TEMPERATURES IN BRICK WORK: FROM TABLES XXX, XXXI, AND XXXII.

In Fig. 192, the extreme temperatures—highest and lowest—of all three tables, XXVIII., XXIX., and XXX., are grouped together in their appropriate positions in the brick-work. The full vertical line M N represents the entire range of the 241 quarter-hourly observations in the 28-inch hole, during the week Sept. 19–24, 1881.

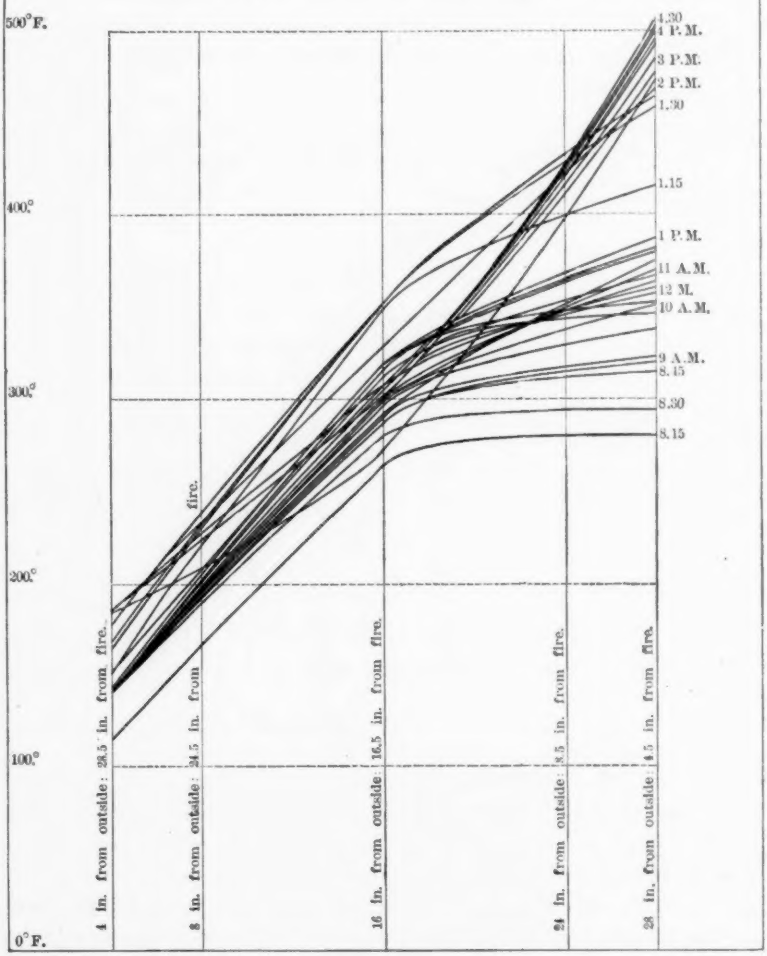
The curved line A B C represents the upper, and the line D E F the lower temperatures, observed on Monday, February 13, 18 and the shaded space between these lines and the vertical dotted lines at 8″ and 24″ from the outside, represents the whole range of the 72 temperatures observed on that day.

The oblique, nearly straight line, G H I, represents the upper, and the sharply bent line J K L, the lower temperatures observed on

Fig. 191.

ACTUAL QUATER-HOURLY TEMPERATURE IN BRICK-WORK:

FROM TABLE XXXII TUESDAY, FEBRUARY 14, 1882.



Tuesday, February 14, 1882; and the shaded space bounded by these two lines and by the 4-inch and 28-inch verticals, represents the entire range of the 102 observed temperatures on that day.

The form of the curves of transmission, whether convex or concave upward, or straight, depends on the relation of the increment of heat at the inner surface, to the conductivity of the brick-work.

In Fig. 190, the hourly lines at 3 and 4 P.M., and Fig. 191, all the lines from 2 to 4:30, are concave upward, showing that heat was received at the inner face of the wall faster than it was conducted away. In Fig. 187, the 12 M. line is almost exactly straight, showing a balance of heat received and conducted; the lines below, especially those at 10 to 11:15, are concave upward, showing that heat is received at the inner face faster than it is conducted outward; and the upper lines, 2 to 4:15, are convex upward, showing that heat is conducted outward faster than it is received at the inner face.

Careful study of these diagrams will clearly teach the importance of thick walls around boiler furnaces. If the wall shown in section in Fig. 192 were, as is too commonly the case, only 16 or 16.5 inches in thickness, the temperature of the outer surface would never rise to 400° or 500° F., because the more active radiation would disperse the heat more rapidly; but this more active radiation would imply a higher temperature than here prevails at the outer surface, perhaps as high as is here found at a depth of 4 to 6 inches, say 120° to 200° F. The mass of brick masonry constituting the inner foot in thickness of a wall two feet or more in thickness, is no mean equalizer of temperatures in the furnace and combustion-chamber of an externally fired boiler. Taking into consideration only five feet in height and one foot in thickness on each side of the furnace, and 26 feet in length (including the cross-wall in rear), we have $2 \times 5 \times 26 = 260$ cubic feet, weighing 100 lbs. per cubic foot, of one-fifth the specific heat of water, equal therefore to 20 lbs. of water per cubic foot; and $260 \times 20 = 5200$. A range of temperature of 200°, from 250° to 450° F., will therefore imply an increase or diminution in the quantity of heat of say 1,000,000 British thermal units, equal to the evaporation from and at 212° F. of more than 1,000 pounds of water. Radiation from brick-work to boiler is rapid and constant, and tends sensibly to maintain uniformity in the transmission of heat to the boiler and its contained water, when the fire-doors are opened for firing, or for cleaning the grates, and when the grates are covered with freshly fired coal not yet fully ignited. The ideal boiler setting will contain, among other things, a series of three-eighths inch iron pipes, welded up at the lower end, inserted vertically, at various

distances from the outer surface of the walls, and to various depths, to contain mercury, for the more complete study of the subject under consideration by the patient and long continued use of the thermometer.

POWER CONSUMED IN DRIVING BLOWER.—The Root blower was driven by a Hoadley portable steam engine detached from its boiler, made by Geo. T. McLauthlin & Co., Boston, 5.5 inches diam-

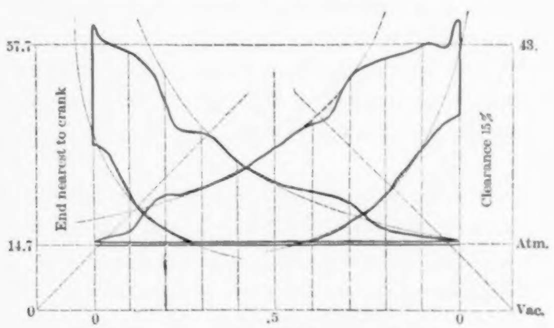


FIG. 193.

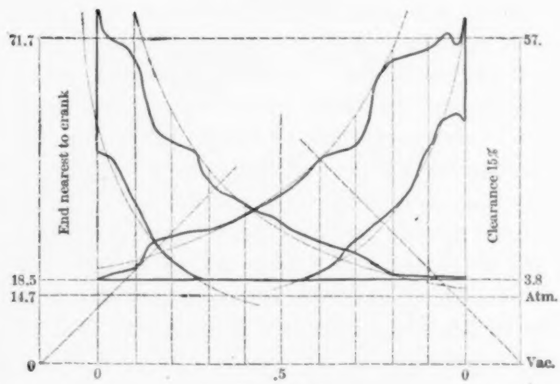


FIG. 194.

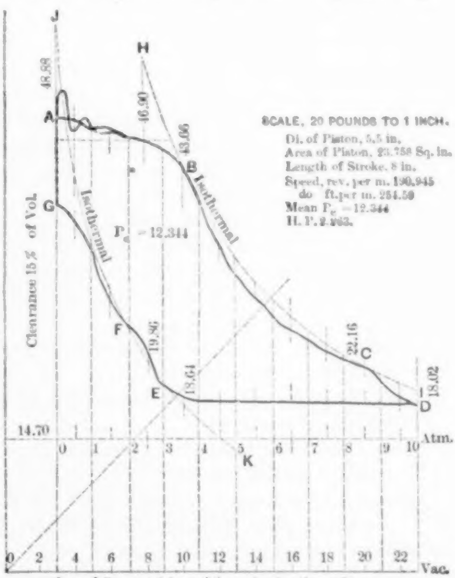
eter of cylinder, and 8 inches stroke, easily capable of producing 9 horse-power, indicated. Its automatic cut-off was adjustable between the limits of 125 and 325 revolutions per minute, by means of a change of links confining the ends of the governor springs.

Steam pressure varied rapidly and widely on account of the variable requirements of the chemical works for steam, so that without automatic cut-off no tolerable regularity of speed could have been maintained.

Indicator diagrams were taken from both ends of the cylinder under various conditions. Two pair of these are given exactly as they were taken, save that they are here reduced to half their original length, the scale of pressures unaltered. In both cases the speed was 200 revolutions per minute. In Fig. 193 the engine was exhausting into the air, and the back pressure was only about 15 pounds absolute. In Fig. 194 the engine was exhausting into an extemporized surface condenser with about 3.8 pounds per square inch back pressure, above the atmosphere, equal to 18.5 pounds absolute. Clearance, ascertained by filling the space with water, was equal to 15 per cent. of the volume swept through by the piston, allowance being made for the volume of the piston-rod at the end nearest to the crank.

There is evidence of leakage in the compression lines. The power shown in Fig. 193 is 2.96 horse-power indicated. The quantity of visible steam exhausted is equal to 40.86 pounds per horse-power per hour.

In Fig. 194 the power is 2.77 horse-power indicated, and the quantity of visible steam is 43.1 pounds per horse-power per hour. On the 23d day of September, 1881, a very careful experiment was made to ascertain the quantity of heat rejected by the engine while driving the blower, by condensing all the steam from the exhaust pipe, and noting the quantity of water used for condensing and its initial and final temperature. For this purpose the steam calorimeter previously described was used.



Mean of diagrams taken while condensing the ex. Steam.

FIG. 195.

Water from a cask placed on a roof, maintained at a constant level by a "ball and cock," was led by pipes to the interior of the calorimeter and distributed by seven pipes of three-quarter inch gas pipe, one passing down

the center in the space usually occupied by the shaft of the agitator, and the other six spaced equally around the sides between the calorimeter lining and the steam drum. Water was supplied to these pipes equally by branches from a vertical, centrally located two-inch pipe extending from the cask.

The water so supplied proved to be adequate to condense the steam at only three to four pounds pressure above the atmosphere (Fig. 195). A weir was fitted to the top of the calorimeter to discharge this water through a spout into a cask placed conveniently near on scales. Delicate and accurate thermometers having their bulbs immersed in small vials filled with oil were placed in the inflowing and outflowing streams, and the temperature they indicated was noted once a minute. The oil in which the bulbs were immersed served to integrate the momentary variations of temperature due to imperfect mixing, so that the changes were moderate both in rapidity and extent.

Time of beginning the experiment, P. M.	4 h. 13 m. 58.5 s.
Time of ending the experiment, P. M	5 h. 35 m. 9.0 s.
Duration of experiment	1 h. 21 m. 10.5 s.
= 1.3529166 h. = 81.175 m. = 4870.5 seconds.	
Reading of counter on engine, final	3122000
Reading of counter on engine, initial	3106500
Number of revolutions of engine in 81.175 minutes	15500
Mean number of revolutions of engine per minute	190.9455
Mean velocity of piston in feet per minute	254.59
Flow of water over weir, 413 lbs. in	5 m. 56.00 s.
Again, 413 lbs. in	5 m. 55.50 s.
Again, 413 lbs. in	5 m. 55.25 s.
Mean flow of water over weir, 413 lbs. in	5 m. 55.58 s.
Total quantity of water in 81.175 minutes, lbs	5657.
Quantity of water per minute, lbs	69.689
Quantity of water per hour, lbs	4181.34
Quantity of water per horse-power per hour, 2.08 indicated horse-power, lbs	2010.26
Total quantity of exhaust steam and entrained and condensed water, lbs	237.
Steam and entrained and condensed water per hour, during 1.3529 hours, lbs	175.18
Steam and entrained and condensed water per horse-power per hour; 2.08 indicated horse-power, lbs	84.22
Ratio of condensing water to steam	23.87
Mean final temperature of condensing water	109.9° F.
Mean initial temperature of condensing water	68.2° F.
Mean rise of temperature	41.7° F.
Number of B. t. u. added to each one pound of water = 110.0047 − 68.2683, B. t. u	41.7364

Total number of B. t. u. added to 5657 lbs. of water in 81.175 m....	236102.8
Mean temperature of condensed steam..................	94.9° F.
Difference in quantity of heat between water at 94.9° F. and at 68.2° F. = 94.9648 − 68.2683 = B. t. u.......	26.6965
Quantity of heat carried off by water condensed from steam (237 lbs.) above initial temperature of condensing water = (94.9648 − 68.2683) × 237 = B. t. u.......	6327.1
Total quantity of heat rejected by the engine and found in the condenser, 236102.8 + 6327.1 = B. t. u....	242429.9
Power represented by the mean indicator, diagram, Fig. 195; mean of five diagrams taken from the end of the cylinder farthest from the crank; three others, substantially like these, being slightly imperfect, are rejected. I. h. p..................................	2.263
Ratio of the mean power at the two ends of the cylinder to the power developed at the end farthest from crank, per cent..	91.78
Power developed at both ends of the cyl., 2.636 × .9178 = i. h. p..	2.08
British thermal units equal to 2.08 indicated h. p. during 81.175 minutes, $= \frac{2.08 \times 33000 \times 81.175}{772} =$ B. t. u,...............	7217.42
Steam pressure, absolute, in condenser, lbs. per sq. in...	18
Heat, above 0° F. contained in one pound of steam of 18 lbs. pressure per sq. in absolute, B. t. u.............	1181.7640
Heat above 0° F. contained in one pound of water of temperature of condensed steam, 94.9° F., B. t. u.........	94.9648
Quantity of heat to be subtracted from one pound steam of 18 lbs. per sq. in. pressure absolute, to condense it to water of 94.9° temperature, B. t. u................	1086.7992
Number of pounds of steam of 18 lbs. per sq. in. p. abs. condensed by convertion of heat into work in the engine, 2.08 i. h. p. during 81.175 minutes, $= \frac{7217.42}{1086.7992} =$ lbs................................	6.641
Per horse-power per hour, $\frac{6.64 \times 60}{2.08 \times 81.175} =$ lbs................................	2.36
Number of pounds of visible steam, according to indicator cards, per horse-power per hour, lbs..............	41.77
Quantity of steam, visible, and condensed in doing work, per h. p. per hour, 2.36 × 41.77 = lbs...............	44.13
Quantity of entrained water, condensation in pipes and cylinder (not in doing work), and leakage; being the excess of total steam and water admitted to steam drum (237), over visible steam and steam condensed in doing work (124 lbs.), per i. h. p. per hour,	

$$= \frac{237 \times 60}{81.175 \times 2.08} - 44.13, = \text{lbs} \ldots\ldots\ldots\ldots\ldots\ldots \quad 40.09$$

Ratio of excess to steam visible :

$$\frac{40.09}{44.13} = .9084, = \text{per cent} \ldots\ldots\ldots\ldots\ldots\ldots \quad 90.84$$

Ratio of excess to total :

$$\frac{40.09}{84.22} = .476, = \text{per cent} \ldots\ldots\ldots\ldots\ldots\ldots \quad 47.6$$

Ratio of visible steam, etc., to total :

$$\frac{44.13}{84.22} = .524, = \text{per cent} \ldots\ldots\ldots\ldots\ldots\ldots \quad 52.4$$

Speed of blower corresponding to 191 rev. of engine per minute 2.32

Rate of combustion of anthracite corresponding to 232 rev. of blower per m. (232 × .07169), in pounds of coal per sq. ft. of grate area per hour (by experiment) 16.63

Pounds of coal burned per hour on fire-grate, 16.63 × 25.83 = lbs 430

Pounds of water evaporated from and at 212° F. per hour, 430 × 11.71 lbs.. 5035.

Ratio of all water passing through the engine to water evaporated,

$$\frac{175.18}{5035} = .035 = \text{per cent} \ldots\ldots\ldots\ldots\ldots\ldots \quad 3.5$$

This engine, then, was using 3.5 per cent. of all the water evaporated; but at the rate of 84.22 pounds of water per horse-power per hour. If power were supplied from a large engine, of good construction, 24 pounds of water per horse-power per hour would be sufficient; and $\frac{24}{84.22} = .287$, and $.287 \times 3.5 = 1$.

Therefore, the steam required to drive the blower, with a reasonably good engine, running with 24 pounds of water per indicated horse-power per hour, is 1 per cent. of the steam generated by its use.

It may be worth noting that the circumference of the engine-pulley was 113.30 inches, and that of the blower pulley, 92.87 inches. The number of revolutions made by the engine and blower respectively, during weeks G, ending February 4, and H, ending February 11, 1882, and the running time each week, were:

	Running time. Minutes.	Whole number of revolutions of Engine.	of Blower.
Week G	3272	542 624	659 808
Week H	3156	508 176	616 088
Total, 107 h. 8 m. =......	6428	1 050 800	1 275 896

The ratio of these numbers is:

$$\frac{1275896}{1050800} = 1.2142$$

and $$\frac{113.3}{92.87} = 1.2200$$

Difference = "slip" = .0058—say about 0.6 per cent.

SOLID CARBON AND ASH IN FLUE GASES.—During the week ending August 20, 1881 (week F), the fuel being bituminous coal, an experiment was made by Mr. Prentiss to determine the quantity of solid matter—finely comminuted carbon and ash—borne off in the cloud of black smoke which to vulgar apprehension appears to present a formidable loss of combustible material, and is in fact a palpable and serious nuisance.

A stream of gas directly from the flue was drawn by an aspirator through a gas meter, to measure its volume; and as its pressure and temperature were observed, and as the error of the gas meter was ascertained, the weight of the gases became known. The stream of gas—smoke—was made to pass through a strainer of muslin, in the form of a bag, secured at the bottom of a vessel of water, which retained, mechanically, some of the soot, and caused the rest to be diffused and retained in the water, while the gas bubbled up and escaped from the water perfectly clear. When a sufficient and known quantity of the gases had been so passed, the water was evaporated, and the residuum was dried and weighed.

One hundred cubic feet by the meter, equal to 108.53 cubic feet corrected, at 72° F., weighing 534 grains = 0.0762857 lb. per cubic foot, yielded 0.49 gramme = 7.57 grains = 0.001081 lb. of solid matter; and 1 cubic foot, therefore, yielded 0.00001 lb.

The quantity of coal burned during the week, was 12890 lbs.: the mean quantity of flue gas per lb. of coal was 25.23 lbs.; and the total quantity of flue gas was 25.23 × 12890 = 325215 lbs.

The volume of this gas, at 72° F., was

$$325215 \div 0.0762857 = 4263119 \text{ cubic feet,}$$

yielding 4263119 × .00001 = 42.63 lbs. of solid matter—soot. The ratio of this soot to the total quantity of coal burned is,

$$\frac{42.63}{12890} = .0033 = 0.33 \text{ per cent.}$$

No analysis was made of this solid matter, as there seemed to be

no way of completely separating it from the muslin bag, and the quantity was extremely small.

Its gray color indicated that not more than one-half was carbon. The proportion of carbon carried off in the black smoke of this bituminous coal, would, therefore, appear to be not far from one-sixth of one per cent.

APPENDIX III.

MEMORANDUM of agreement by and between Obadiah Marland of Boston, in the County of Suffolk and Commonwealth of Massachusetts, and
the Pacific Mills, of Lawrence, Massachusetts,
the Massachusetts Cotton Mills, of Lowell, Massachusetts,
the Boott Cotton Mills, of Lowell, Massachusetts,
the Naumkeag Steam Cotton Company, of Salem, Massachusetts,
the Atlantic Cotton Mills, of Lawrence, Massachusetts,
the Great Falls Manufacturing Company, of Great Falls, N. H.,
the Boston Manufacturing Company, of Waltham, Massachusetts,
the Merrimack Manufacturing Company, of Lowell, Massachusetts,
the Salmon Falls Manufacturing Company, of Salmon Falls, N. H.,
the Nashua Manufacturing Company, of Nashua, New Hampshire
the Lancaster Mills, of Clinton, Massachusetts,
the Manchester Mills, of Manchester, New Hampshire,
S. D. Warren & Co., of Cumberland Mills, Cumberland, Maine.

The said corporations and manufacturers agree to make and cause to be made a test of the apparatus set forth and described in United States Letters Patent, No. 205,282, to O. Marland, dated June 25, 1878, and Great Britain Letters Patent. No. 2553, to said Marland, dated June 26, 1878, in accordance with the description of said apparatus contained in said Letters Patent, at the Pacific Mills in the City of Lawrence, Massachusetts, under the supervision, control and direction of John C. Hoadley, at their joint expense and cost, in the manner and upon the conditions named herein.

Said test to be made forthwith and without delay as soon as the said apparatus can be properly constructed and placed in operation at said Pacific Mills.

Said test to be made with reference to the combustion of both

anthracite and bituminous coals, and the device for superheating air as set forth in said Letters Patent or either thereof shall be applied to the furnace or furnaces used to make said test.

The expense and cost of said test and the apparatus constructed therefor shall be borne and paid by said corporations and manufacturers respectively according to the number of boilers now in use at their mills named herein set against their names hereto respectively.

Said test to be made and a full and complete report thereof to be made by the engineers and experts employed by said corporations and manufacturers to make said test and to be furnished in writing signed by said engineers and experts to said corporations and manufacturers as soon after said test is completed as said report can be prepared, and a copy of said report to be furnished to said Marland.

When said John C. Hoadley shall give notice in writing to said corporations and manufacturers and said Marland that said test has been made, then it shall by the parties to this memorandum be deemed to be made.

All upon the condition that not less than two hundred (200) boilers shall be represented by the corporations and manufacturers named herein.

And the said Obadiah Marland, for himself, his executors, administrators and assigns, agrees to issue and grant unto each and every of the corporations and manufacturers herein named whose signatures are placed hereto, absolute license for the full term of said Letters Patent and all reissues and extensions thereof, without charge for royalty, rental or otherwise, to apply and use his invention set forth in said Letters Patent upon and in connection with any and all boilers for stationary purposes which now are in use or which may be constructed to be used in the mills now owned by said corporations and manufacturers at the places named herein and set against their names hereto respectively.

Said right and license by said Marland or his executors, administrators or assigns, to be made by him or them to said corporations and manufacturers in due form in writing as soon as said test shall be made and the report thereon in writing made by said engineers and experts and furnished to said Marland or his executors, administrators or assigns.

In consideration of the mutual promises of the parties hereto, the said Marland and the said corporations and manufacturers have sev-

erally placed their hands and affixed their seals hereto, this twelfth day of February, A.D. 1881.

Signed,

NO. OF BOILERS.		
	Obadiah Marland,	[L. S.]
50	Pacific Mills, Lawrence, by Henry Saltonstall, Treas.,	[L. S.]
12	Boston Mfg. Co., Waltham, by Edmund Dwight, Treas.,	[L. S.]
9	Naumkeag Steam Cotton Co., Salem, by H. D. Sullivan, Treas.,	[L. S.]
10	Atlantic Cotton Mills, Lawrence, by Wm. Gray, jr., Treas.,	[L. S.]
18	Massachusetts Cotton Mills, Lowell, by Geo. Atkinson, Treas.,	[L. S.]
13	Great Falls Mfg. Co., Great Falls, by A. P. Rockwell, Treas.,	[L. S.]
37	Manchester Mills, Manchester, by John C. Palfrey, Treas.,	[L. S.]
10	S. D. Warren & Co., Cumberland Mills,	[L. S.]
9	Merrimack Mfg. Co., Lowell, by C. H. Dalton, Treas.,	[L. S.]
13	Boott Cotton Mills, Lowell, by Augustus Lowell, Treas.,	[L. S.]
3	Salmon Falls Mfg. Co., by H. Stockton, Treas.,	[L. S.]
8	Nashua Mfg. Co., by Frederic Amory, Treas.,	[L. S.]
10	Lancaster Mills, Clinton, by James S. Amory, Treas.,	[L. S.]

APPENDIX IV.

COMBUSTION OF FUEL.

BY J. C. HOADLEY.

The perfect combustion of one pound of pure carbon produces, it is said, heat equal to 14,500 thermal units; *i. e.*, heat enough to raise the temperature of 14,500 pounds of ice-cold water 1° Fahrenheit. No coal, no coke, consists of pure carbon. Commercial anthracites yield, on analysis, about five per cent. of oxygen and hydrogen united in the form of water, so that the hydrogen is of no calorific value. There is also a varying proportion of earthy matter left in the furnace after combustion—in part also drawn into the flues and chimney—ranging from 5 to 15 per cent. The purer coals are apt to crumble so badly in heating, for want of the tenacity which a larger proportion of "ash" would give, that they often suffer considerable loss by decrepitation, and sifting through the fire into the ash-pit unburned. These causes reduce the theoretical value of one pound of commercial coal (anthracite) about one-sixth, or from 14,500 to 12,083 thermal units.

Each thermal unit is equal to 772 foot-pounds of work, so that the perfect combustion of one pound of commercial anthracite coal is equal to

$$12{,}083 \times 772 = 9{,}328{,}076 \text{ foot-pounds.}$$

One horse-power exerted during one hour is $33{,}000 \times 60 = 1{,}980,$-

000 foot-pounds; therefore, if all the work represented by the perfect combustion of the carbon contained in one pound of commercial coal in one hour could be converted into useful work in an engine, it should produce

$$\frac{9328076}{1980000} = 4.711 \text{ horse-power one hour;}$$

and each horse-power should require, each hour,

$$\frac{1980000}{9328076} = 0.212 \text{ pounds of coal.}$$

But in fact, instead of about one-fifth of one pound, the very best engines require ten times as much, or two pounds per hour. Very good practice requires fifteen times as much, or 3.0 to 3.25 pounds; and the great majority of good engines consume from fifteen to twenty times the above quantity—that is, 3.25 to 4.25 pounds of coal per horse-power per hour—and show a ratio of actual performance to the full calorific power of the fuel consumed of 5 to 6 per cent. But this loss of from nine-tenths to nineteen-twentieths of the work represented by the combustion of coal—almost startling when contemplated for the first time—is in great measure irremediable in the steam engine, arising as it does from the physical properties of water, employed as a vehicle for the use of heat. The problem in the steam engine is to convert the molecular motion of heat into the sensible motion of ponderable masses—a piston, fly-wheel, etc.; and the degree in which it is possible for it to accomplish this, every imperfection and every source of loss eliminated, is the ratio which the difference of temperature of initial and exhaust steam (or its "range") bears to the absolute temperature of initial steam; that is, $\frac{T_0 - T_1}{T_0}$, where T_0 is the absolute initial temperature, and T_1 the absolute final temperature. For instance, if in a locomotive steam be taken into the cylinder up to the point of cut-off, at 120 pounds per square inch, steam-gauge pressure (above a mean atmospheric pressure of 14.7 pounds) = 134.7 pounds absolute pressure, its sensible temperature Fahrenheit will be 350°, and its absolute temperature 461° greater, or 350 + 461 = 811°. Exhausted under pressure a little greater than that of the atmosphere, say 15 pounds per square inch absolute pressure, its sensible heat Fahrenheit will be 213°, and its absolute temperature will be 461° more, = 213 + 461 = 674°. Now, if $T_0 = 811°$, and $T_1 = 674°$, then $\frac{T_0 - T_1}{T_0} = \frac{811 - 674}{811} = \frac{137}{811} = 0.169$, or say $16\frac{9}{10}$ per cent. That is, the range of temperature between initial and

exhaust steam being 137° Fahrenheit, and the absolute initial temperature being 811° Fahrenheit, such a steam engine, on account of being obliged to let the steam go while it still has a temperature 213° above zero Fahrenheit = 674° above absolute zero (which is 461.2° say, 461° below zero Fahrenheit), has within its reach, if it could save it all, only 16.9 per cent. of the whole work contained in the initial steam in the form of heat. Such an engine will in fact yield about 6 per cent.; and, dividing this 6 per cent. by the 16.9 per cent., we have $\frac{6}{16.9} = .355$, or 35.5 per cent., as the ratio of usual engine performance to *perfect* performance of a perfect heat engine, under the above usual conditions.

About two-thirds, then, of the heat work that may at least be striven for is usually lost.

Where is this loss? In the engine chiefly; but the boiler must come in for a share.

Let us see what the boiler's share of this loss amounts to. Pure carbon perfectly burned, with just sufficient air to supply the requisite oxygen, will produce mixed gases weighing 12.6 pounds for each pound of carbon:

Carbon,	1.0	Carbon,	1.00	
Air,	11.6	Oxygen,	2.66	
	12.6	CO_2	3.66	Products.
		Nitrogen,	8.94	Products.
			12.60	Products.

The specific heat of carbon dioxide is 0.216; that of oxygen, 0.217; nitrogen, 0.244; atmospheric air, 0.238. It follows that the specific heat of all the products of combustion, with whatever excess of air over that chemically necessary to the complete combustion of carbon, is about 0.237, and that, to heat one pound of water 1° Fahrenheit, 4.22 pounds of such gaseous products must be cooled an equal amount. If a pound of coal were pure carbon, its gases would weigh, without excess of air, 12.6 pounds; with 50 per cent. surplus, 18.4 pounds; with 100 per cent. surplus, 24.2 pounds; with 125 per cent. surplus, 27.10 pounds; and with 150 per cent. surplus, 30.00 pounds. But of commercial coal only five-sixths is carbon. We neglect the water (or oxygen and hydrogen) —as the quantity, small at most, is variable, and its effect on the result would not justify the complication its consideration would cause—and simply take five-sixths of the above quantities, and tabulate them, with the corresponding weight of water per degree, and the thermal units expressed in foot-pounds.

TABLE I.

GASEOUS PRODUCTS OF THE COMBUSTION OF ANTHRACITE COAL, AND THE LOSS CAUSED BY THE ESCAPE OF THESE GASES AT SEVERAL ASSUMED TEMPERATURES; WITH JUST SUFFICIENT AIR FOR PERFECT COMBUSTION, AND WITH VARIOUS DEGREES OF SURPLUS, 50, 100, 125, AND 150 PER CENT.

Excess of air above that chemically necessary for combustion of carbon, per cent. of the necessary quantity.	Weight of the gaseous products of combustion of the carbon in one pound of anthracite coal, 5-6 of coal.	Corresponding weight of water which could be heated 1° by cooling these gases 1°.	Thermal units expressed in foot-pounds, one thermal unit being 772 foot-pounds.	Total for 300° above external air. Foot-pounds.	Total for 400° above external air. Foot-pounds.	Total for 500° above external air. Foot-pounds.
1	2	3	4	5	6	7
	Pounds.	Thermal units.	Foot-Lbs.	Foot-Lbs.	Foot-Lbs.	Foot-Lbs.
0	10.50	2.4881	1,921	576,300	768,400	960,500
50%	15.333	3.6335	2,805	841,500	1,122,000	1,402,500
100%	20.166	4.7788	3,689	1,106,700	1,475,600	1,844,500
125%	22.583	5.3515	4,131	1,239,300	1,652,400	2,065,500
150%	25.000	5.9242	4,573	1,371,900	1,829,200	2,286,500

I have made the divisions above mentioned for various temperatures, ranging from 300° to 700° above the external air, and have tabulated the result in the following table:

TABLE II.

RATIO, PER CENT., OF THE HEAT CARRIED OFF BY THE GASEOUS PRODUCTS OF COMBUSTION TO THE TOTAL CALORIFIC POWER OF EACH POUND OF COAL; WITH VARIOUS DEGREES OF EXCESS OF AIR, AND AT VARIOUS TEMPERATURES OF THE ESCAPING GASES ABOVE THE EXTERNAL AIR.

Excess of air above that chemically necessary for combustion of carbon, per cent. of the necessary quantity.	RATIO OF LOSS TO TOTAL CALORIFIC POWER. PER CENTUM. Temperatures above external air.						
	300°	400°	500°	600°	700°	800°	75°
1	2	3	4	5	6	7	8
0	6.18	8.24	10.30	12.36	14.42	16.47	1.55
50%	9.02	12.03	15.04	18.04	21.05	24.06	2.25
100%	11.86	15.81	19.77	23.72	27.67	31.63	2.97
125%	13.29	17.72	22.15	26.58	31.01	35.44	3.32
150%	14.71	19.61	24.52	29.42	34.32	39.23	3.68

Since, as we have seen, the total calorific power of the five-sixths of a pound of carbon in one pound of commercial coal is five-sixths of 14,500, equal to 12,083 thermal units of 772 foot-pounds each, equal to 9,328,076 foot-pounds, the numbers in the columns 5, 6, and 7 of Table I., divided by 9,328,076, will give the respective ratios of loss from this cause in each case.

Doubts may be entertained as to so large an excess of air as 150 per cent. occurring in practice. In fact, it is very common. It is not easy to carry on complete combustion by means of natural draft with less than 100 per cent. excess—*i. e.*, double the necessary quantity—reckoned as it usually is at 12 pounds of gases absolutely necessary per pound of *coal*, as if coal were entirely composed of carbon. Now, 25 pounds of gaseous products for the combustion of one pound of anthracite coal containing only five-sixths of a pound of carbon, and producing, with no excess of air, only 10.5 pounds of gases, is equal to ($\frac{25}{10.5} = 2.38$) 138 per cent. surplus air. Experiments to ascertain the composition, volume, and temperature of the gases from 17 boilers, burning good anthracite coal at a known rate, with great care, and under most favorable conditions of draft, grate area, rate of combustion, area of heating surface, and general management, gave, by analysis, carbon dioxide (no monoxide), nitrogen, and free atmospheric air—the latter being one-half of the whole. A check upon the accuracy of these results was found in the temperature of the furnace. This should be, with double supply of air, about 2,600° Fahrenheit. It was found to be a little less, about 2,400°. In my opinion, it is understating rather than overstating the matter to say that the average of good practice would show a double supply of air.

If we take as the most common boiler pressure in stationary boilers 80 pounds per square inch above the atmosphere—say 95 pounds absolute—its temperature, 324° Fahrenheit, will be that of the *cooling* surface to which the hot gases are exposed. In strictness, the temperature of the outside of the boiler plates will be higher than this, as 324° must be about their temperature inside, and the transmission of heat from without implies a higher temperature on the outer surface. Data exist for the computation of this exterior temperature under given conditions; but the computation is unnecessary here. It is probable that there can be no active transmission of heat from the gases without to the water within a boiler, with less than 75° difference of temperature within and without, which will include the difference in the two sides of

the plates. Professor Dwelshauvers-Dery, in an article published in the *Revue Industrielle des Mines*, of which a translation appears in Van Nostrand's *Engineering Magazine* for February, 1880, estimates this difference at 91° C. = 164° Fahrenheit, which seems to me excessive; but 75° is probably quite within the mark. Observation of a pyrometer in the smoke-box of a return-tubular boiler at all stages of the fire has satisfied me that in excellent boilers, well fired, having a ratio of heating surface to grate area as large as 36, the temperature of the escaping gases rarely, if ever, falls lower than 75° above the temperature due to the steam pressure, except when the fire-doors are open, and there is great and unusual excess of air admitted. Adding 75° to the temperature corresponding to 80 pounds steam-gauge pressure, 324°, we have, say, 400° as the lowest practicable temperature of escaping gases. This will be confirmed by the best practice under favorable conditions; and the actual temperature will range through a low average of 500° and a high average of 600° up to 800° or over; in some extreme cases going up to high incandescence, or over 1,000°.

How much of this loss can be saved and returned to the fire? By the Marland plan of passing the gases after their escape from the boiler through thin passages, the thin walls of which are in contact on their opposite sides with air for supplying combustion, entering with a current flowing in a direction opposite to that of the gases, the final temperature of the cooling surfaces becomes that of the external air, say, as an approximate mean, 60° Fahrenheit, to which the temperature of the gases may be made to approximate as closely as to the temperature of the water in the boiler, say within 75°, making their ultimate temperature, on release, 60 + 75 = 135°. This is not too hot for discharge through a Root blower, while it is too cool to give efficient draft in a chimney. At this temperature the ratio of irrevocable loss becomes one-fourth as much as at 300° above outside air, say, for double supply of air (100 per cent. surplus), 2.97 per cent.

I have set the several ratios in an additional column at the right hand of Table II., column 8. Taking now the ratios of loss, with 100 per cent. surplus air, from Table II., and subtracting from each one this final loss, we have

TABLE III.

RATIO, PER CENT., OF SAVING TO BE EFFECTED BY O. MARLAND'S SMOKE-COOLING AIR-HEATER, AT 100% SURPLUS AIR SUPPLY.

	TEMPERATURES OF GASES ON ESCAPING FROM BOILER ABOVE EXTERNAL AIR.					
	300°	400°	500°	600°	700°	800°
1	2	3	4	5	6	7
First loss	11.86	15.81	19.77	23.72	27.67	31.63
Final loss	2.97	2.97	2.97	2.97	2.97	2.97
Actual saving	8.89	12.84	16.80	20.75	24.70	28.66

It appears, then, that under ordinary circumstances from 16 to 20 per cent. of the total quantity of heat produced by the combustion of anthracite coal can certainly be saved and returned to the furnace by the Marland apparatus, judiciously arranged and proportioned; that in no circumstances can such saving fall so low as 10 per cent.; and that it will often be 25 per cent., and may, in extreme cases, reach 30 per cent.

The rate of evaporation per pound of coal from feed water at 60°, under 80 pounds steam-gauge pressure, say 324°, is certainly, in general, below 8 pounds. Indeed, 8 pounds of dry steam is a fair result, 8.25 pounds a good result, 8.5 pounds very good, and 9.0 pounds about the best usually attainable, being rather over 10,000 thermal units, which corresponds to 69 per cent. of the full calorific power of carbon, and is, for coal of five-sixths carbon, a high result.

If we take, as we properly may, 8.5 pounds of water evaporated into dry steam of 80 pounds steam-gauge pressure from feed water of 60°, with one pound of anthracite coal of five-sixths carbon, as corresponding to an air supply of 100 per cent. surplus, and escaping temperature of gases of 400° above external air, the apparatus, in effecting a saving of 12.84 per cent., would add to the evaporation, say, 12.84 per cent. of 10.8 = 1.4 pounds, making (8.5 + 1.4) 9.9 pounds; 10.8 pounds being the *full* evaporating power of such coal under the given conditions. To about this degree of efficiency, or to nearly or quite 10 pounds of water per pound of five-sixths coal from water of 60° to steam of 324° (80 pounds steam-gauge), this apparatus should be able to bring all good boilers, with whatever excess

of air, or at whatever (reasonable) degree of heat, the gases were allowed to escape from the boiler. Not only will this apparatus restore to the furnace a large part—from four-fifths to eight-ninths of the heat otherwise inevitably lost; not only will it serve as a "heat-trap" to arrest and restore the loss otherwise inevitable by admission of cold air at the doors while firing and clearing out fires, and by the neglect or unskillfulness of firemen—it will also, I have no doubt, increase the rapidity of combustion, and so enable complete combustion to be carried on with a smaller quantity of air, *i. e.*, with less excess over the quantity chemically necessary.

It is true that by heating air from 60° up to 385° (that is, up to 400° above the temperature of external air less 75° of final difference), or from 521° to 846° absolute temperature, its volume will be increased in the ratio of these latter numbers, as 1 to 1.624, or about one to one and five-eighths: eight (8) cubic feet of air in the atmosphere will occupy thirteen (13) cubic feet in the pipes conducting it to the fire, whether above or below the grates.

Of course its density is in the same inverse ratio. Thirteen cubic feet of the heated air (385°) must be admitted to the fire and to contact with glowing fuel, in order to introduce as much oxygen as would be contained in eight cubic feet of the cold air (60°).

Equally, of course, the entering velocity must be greater in the some proportion, since the aggregate area of all the orifices through the grates and fuel may be regarded as constant.

This has been urged, sometimes most strenuously, as an objection to heating air before its introduction to the fire. The objection seems to me to rest on a partial view of the conditions of air-admission. It may be conceded that cold air in necessary quantity will enter the ash-pit, and will pass through the interstices of the grates, with less velocity than will the same quantity of heated air. But in these passages the area is (or always may be) amply large, and the velocity moderate. It is also true that, on entering the lower stratum of fuel, the velocity of the heated air will be the greater. But the very first effect of the chemical union of any part of the oxygen with any part of the carbon is to heat the gases associated with such oxygen—that is, its associated nitrogen and the atmospheric air yet containing its oxygen, together with the carbon dioxide resulting from such union or combustion—to the full extent to which the entire heat of combustion can raise the given mass of gases. This will be, approximately, the temperature of the furnace, a little modified, probably a little increased, by the

subsequent union of further portions of oxygen with new portions of carbon encountered during the farther progress of the mixed gases through the fuel, until they emerge, further de-oxygenated and further loaded with carbon dioxide, at the surface of the fire. If there is not, as there need not be, any carbon monoxide, the gases will be at their hottest and at their greatest volume on emerging from the surface of the fire.

Any further admission of air will only cool them by dilution. If their temperature be now 2,500° Fahrenheit = 2,961° absolute, their volume will be $\frac{2961}{521} = 5.7$ times that of air of temperature 60°, and $\frac{2961}{846} = 3.5$ times that of air of temperature 385°.

Now it is the volume of the gases at their final emergence from the interstices of the fuel that determines their flow—determines the force of draft or blower required to produce that flow. The expansion, which of necessity takes place in the most confined space—namely, in the interstices of the fuel—acts equally in all directions. Although all in motion upward through the fire, its upward portion, being most expanded, is moving more rapidly than its less expanded lower portion; and its expansive force, acting downwardly, simply retards the upward flow of entering air. Lateral expansion aids in bringing fresh oxygen into contact with unconsumed carbon. Upward expansion aids, and downward expansion retards, the draft. Now it is plain that this effect must be the greater, the greater the degree of expansion which takes place within the interstices of the fuel.

With air supply at 60°, it is 5.7-fold. With equal air supply (by weight), at 385°, it is 3.5-fold.

This difference is in the right direction to compensate, as far as it goes, for the greater force required to introduce the heated air with its greater volume and higher velocity, and certainly does compensate for it to some extent. My impression is, that it exactly balances the initial resistance; that the diminution of resistance to final expansion is an exact equivalent for the resistance encountered on entering; but this opinion is based on general dynamic considerations, and is not the result of special investigation. Certainly it cannot be far wrong.

Of the higher resulting temperature of the gases, there can be no question.

Nor can it be questioned that combustion will be more rapid.

Carbon (a solid) and oxygen (a gas) unite at all temperatures usually met. Anthracite coal wastes, in the open air, by slow combustion—so slow that the resulting heat, which is exactly the same as if the combustion were more rapid, is dissipated by radiation and the convection of the air. The rapidity of combustion is augmented with the rise of temperature, and is very great at high incandescence. Now, the temperature of the oxygen is no less important than that of the carbon: the higher the sum of their temperatures, the more rapid their union. So far as the associated gases are concerned, their higher temperature only serves to communicate more heat to the mass, or (which comes to the same thing) to abstract less from it.

Combustion being more rapid—being carried on with greater avidity—it seems certain that a smaller excess of air will be practically required; and, although the Marland apparatus diminishes the final loss from excessive air supply, it does not entirely remove it, since it must release the gases about 75° above the temperature of surrounding air. It also costs something to pass air through a blower or a chimney; and the less of it necessary, the better.

Grates of ordinary form could not endure a temperature of 400° or 500° in the ash-pit; but water grates are well known, and entirely available.

This device of Mr. Marland is an application of the well-known and firmly established principles of the Siemens regenerating furnace, by means of appropriate apparatus, to the conditions of such furnaces as those of steam boilers, and, if judiciously applied and worked out, should be as successful in its sphere as the Siemens furnace is in metallurgy.

This apparatus is particularly well adapted to the combustion of anthracite coal. Prideaux justly says (*The Economy of Fuel*, edited by D. K. Clark, New York, D. Van Nostrand, 1879, Part II., p. 211), "The less the quantity of hydrogen present, as with anthracite, the greater will be the chance of being able to seize the economic advantages attendant upon the increased quantity of heat attainable by the use of hot air, without having this heat so diluted as to make the temperature inefficient."

There can be no doubt that the heat to be returned to the furnace would several times exceed that necessary to make the power required to drive the exhausting fan, to the operation of which the final temperature of the gases presents no objection. No damage would probably be done to the plates of the air passages of this

apparatus by the heat of the entering gases in any admissible circumstances. Such gases are usually received from the flues of return-flue or return-tubular boilers, in plate-iron smoke-boxes, which prove as durable as other parts of the boiler and its appurtenances.

The passages are so divided that each one is thin, and the exposed surface is large, so that the temperature would fall rapidly, and thin plates must prove durable. The use of an exhaust fan will produce an inward draft at all orifices or leaks, which will merely increase, in some small but probably insensible degree, the load on the blower, but will, on the other hand, keep the incoming air free from carbon dioxide and nitrogen, and the fire-room free from noxious gases.

In the construction of new works the outlay for the Marland apparatus will be, or at least *may* be, largely offset by saving in the cost of a chimney.

If this apparatus can be successfully applied to marine engines, the gain by reduction of coal cargo, and by the increase of paying-freight carrying-capacity, is too obvious to require comment.

The arrangements for cleaning out the smoke passages seem to be convenient and efficient. The whole apparatus bears marks of thoughtful study, and seems to me to promise results worth some effort and expense to put to the proof of practical working.

CLXXXIV.

TOPICAL DISCUSSIONS AND INTERCHANGE OF DATA.

[NOTE.—At the XIth meeting of the Society the plan was inaugurated of presenting topics for discussion without the formality of a previous paper. This feature of the meetings is in the hands of a special committee, and by them the following topics were introduced.]

No. 184—1.

Are welded boilers and flues stronger and stiffer than riveted work, and have they merits and defects which are not found in riveted work?

DISCUSSION.

Mr. Root.—I do not know that I have much to say if I confine myself to the terms laid down. I have not had very much practical experience in that matter. The welding is now done in a different manner from the way it was formerly done, by welding a short space at a time. It is a continuous operation by which the seam is subjected to a steady heat, the conditions kept uniform, and the seam closed continuously. All I have to say is, that it bids fair to be a very useful process in the production of cylindrical work of all kinds. I think it can be applied with great advantage to the construction of steam boilers and shells for boilers. But so far as practical knowledge of the thing is concerned, I must say I am deficient in that. That is a matter that is to come in future.

The President.—Was the welding done in a gas flame or open fire?

Mr. Root.—It was done by gas jets.

Mr. Kent.—I have had no experience in welding work of that kind. Experiments are now being made at Mr. Rowland's works, at Greenpoint, in welding boilers. They have built a furnace and apparatus for welding, but I have not heard that they have put it into operation. I might ask Mr. Holloway if we cannot dispense with both riveting and welding in making boilers and make them seamless as they are trying to do in Cleveland.

The President.—Perhaps at some other meeting I will say more about it.

Mr. Green.—Does steel possess proper welding properties to enable it to be used to replace iron for boilers? A great many of us believe that steel is to be the material to make boilers of. But is not the difficulty of welding it something against it?

The President.—Of course it is understood that in the new process of making steel, it is intended that the material shall be of a character to weld with all the freedom of iron. Whether that shall prove true or not, is a matter for the future to illustrate. The more recent steel makers claim that they are producing a material which they can weld with ease. But I believe that the tubes Mr. Kent refers to will be made out of plate, so there will be no welding required.

Mr. Root.—I have tried several brands of steel, and while I have been able to weld them all successfully, some of them would crack. I used the pincer for closing the seam, which had to be kept cool with water, and a certain cooling would crack the steel when it had much carbon in it. I tried brands of steel very low in carbon, and it seems to me to weld much better than iron. It stands the heat better. Heating injures it less, I think, than the iron. Almost all low grades of iron, when you put heat on them, have a certain amount of slag in them, which is burned out and leaves the metal rather open and not so strong. But the low steel seems to possess full strength even after it is heated, and it does not seem to harden at all.

The President.—Did Mr. Root have the scarf weld?

Mr. Root.—No, sir.

The President.—It is known that in welding iron the edges are upset for the purpose Mr. Root mentions, of allowing a wasting away of the material; and it is due to the fact that the material is not pure, that there is a wastage whenever the weld is made, and that it is necessary to get an upset edge for the purpose of having the full thickness of the metal after the weld is made. As I understand it, this making of shells without riveting is carried on now in England very largely, but I did not know myself of any place in this country where it had been adopted.

Mr. Sweet.—It may not be generally known that the shells of the air tanks under the cars in the Westinghouse air-brake system are welded up and the heads welded in. The welding in of the heads, as they do it, is an operation worth seeing. The head with the flange turned out is forced in place and the tank suspended in a vertical position over the fire. When the two

exposed edges, which are to be welded, are brought to a welding heat, they are grasped between two rollers, which being set in motion make the weld; the tank spinning around as the welding takes place.

Mr. Babcock.—While I was in Manchester last year I visited an establishment there—Sterne & Co.—which has recently been fitted up in quite an extensive way for the purpose of making welded boilers. A sheet is rolled into a hoop, perhaps thirty-six inches in diameter, and is then placed in their machine, and with one motion the longitudinal seam is welded, making a complete cylinder. Mr. Sterne told me that it was a very great success, but I have no means of knowing exactly what the difference in strength is between the welded and riveted seams. The roundabout seams were to be riveted, his plan being to weld up rings which were to be riveted together to make the shell. By welding a short length of the seams at a time, he could make rings five, or even six feet long. I know that welded boilers are being used considerably in England. A large maker of upright boilers in Oldham, told me that they welded all their shells and welded in one head. The fire-boxes were welded complete, and the only riveting done was the bottom seam where the fire-box joins the shell. He said they found it much cheaper than riveting.

As for the strength of welded joints on large sheets I have no definite data; but some experiments were made a short time since—this spring, in Jersey City—on some large welded tubes. They were twelve inches in diameter and about eighteen feet long, five-eighths thick, with heads welded in. There were sixteen of them made, and half of them gave out at eighteen hundred pounds pressure, which was less than half the supposed tensile strength of the iron. Many of them gave out in the weld, and some gave out elsewhere, but I am not able to state the particulars.

The President.—We have in Cleveland a man who has invented a machine for piecing out boiler tubes. He has an apparatus by which he scarfs them and welds them under rollers, similar to the method that Prof. Sweet speaks of, and makes an admirable weld. Can Mr. Walker tell us anything about the practice in England in regard to this matter?

Mr. Walker.—So far as I know, in England they have been trying for some years with fair success in welding flues and boilers. I cannot speak of the recent improvements, but can say, that

eighteen years ago William and John Yates, of Blackburn, Lancashire, England made some very successful work with their flues for Lancashire boilers, which Figure 230 will illustrate. The sections *a a*, were made thirty to thirty-six inches in length, the longitudinal seam was welded and ends flanged outward as shown, with a large round corner for expansion and contraction. The stiffening ring *b* was introduced at each flange securely riveted and caulked in the usual way. It is evident that by using short sections and consequently more flanges, the flue would be stiffer and less liable to collapse, and at the same time there would be provided more points for expansion and contraction.

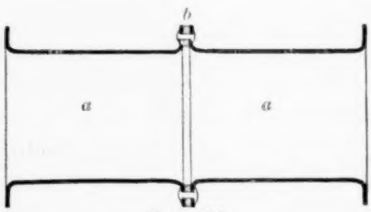


FIG. 230.

With reference to manholes, Mr. Babcock no doubt saw, when he was in England, that they made manholes of seamless wrought iron. A wrought-iron manhole is a very admirable thing, and ought to be taken up in this country. There have been many boiler explosions caused by cast-iron manholes cracking. Cast iron will not stand shrinking and expanding like wrought iron, and English boiler makers have adopted the wrought-iron manhole, and found it a great success. I am sorry to say that I am not posted on welded flues and boilers up to the present date.

Mr. Partridge.—I am unable to speak definitely in regard to this matter, but I know that some parties, who have had the superintendence of extensive boiler contracts, and who have experimented with long welded flues, found that they seemed to be too elastic and needed stiffening. They found it necessary to have rings riveted around the welded flues, or else specified a riveted rather than a welded flue, in order to secure the necessary stiffness. They could give me no information in regard to the question of strength; but they were inclined to think that continuous sheets for flues or boilers were not unalloyed blessings, even though no welds were employed to weaken them, and uniform strength was secured. They thought it possible that rings would have to be riveted or welded upon boilers made from single sheets in order to secure stiffness. They regarded long welded flues, especially those of large size, as dangerous, unless stiffened by suitable rings.

The President.—It has been the practice on the western rivers for a great many years—the Ohio and Mississippi—that is, the former practice; I cannot speak of late years—in making their two-flue boilers in which they carry steam from 175 to 180 pounds, to make the boiler tubes of short sheets, and in that way get in a great many riveted joints. I can remember when they were about twenty-six inches from center to center of rivets, and they all preferred to make the tubes of short sheets, and get the double thickness at as many places as possible on the boiler, for the purpose of getting additional thickness to sustain them against the tendency to collapse.

Mr. Kent.—It was kept up till two or three years ago, so far as I know. I thought it was an old-fashioned prejudice. On the Mississippi they still stick to antiquated engines, pumps and boilers, and everything else.

The President.—What to most of us may seem to be antiquated in some places, as Mr. Kent remarks, is after all the result of a great many years' experience. I know that it is the feeling of a great many eastern engineers who go west, and see a western steamboat, that they do not know how to make steamboats out there. But the fact is, that the boilers, and the boats in use there, have grown up as the very thing best adapted to the country and the circumstances. Of course in the first place it was difficult to make large sheets, and get sound sheets; but I think the point was, to get short sheets and plenty of joints for the purposes stated.

Mr. Babcock.—As is well known, many seams in shells exposed to rupturing stress are a weakness and not a strength, but in flues exposed to a collapsing pressure, the presence of roundabout seams adds stiffness, by virtue of the double thickness at the seams serving as stiffening rings. Doubtless, therefore, the practice on western rivers, of multiplying such seams, has grown out of experience in the use of such flues, and is correct in respect to strength against collapsing pressure. Lap joints are, however, very objectionable in the fire-box portion of a boiler, because of the increased thickness for the heat to pass through, and consequent danger of burning the sheet. The desired stiffness is obtained in the best recent practice by the Fox corrugated tubes, which are largely used for furnaces in marine boilers. These are welded and then corrugated, so as to give great stiffness with no extra thickness, and the joints are made as few as possible.

Mr. Root.—Mr. Thos. F. Rowland is a neighbor of mine at Greenpoint, and I very often see his process in operation—and, by the way, he said he would be here to-night, but I do not see him. He makes some very fine work there in the way of torpedoes. They are made by turning out a flange and pinching the flange together in front of the furnace, and he also welds in all the bushings, rings and connections in a very ingenious way. He makes very perfect work. He has also put up there a machine which some one alluded to for welding corrugated fire-boxes. I don't think that has been in operation; I don't think he has started it yet. I saw the machine there; but I do not think they have welded anything with it. But he tells me that he is going into the welding business extensively. That is the only place in this country that I know of where they seem to be taking it up with very much vim, or seem to be doing much at it. He has put up very fine gas works there, and he is using water gas for getting his heat. He is welding his torpedoes now with water gas. Speaking of the strength of the longitudinal seams, it always seemed to me very hard to get a welded seam as strong as the iron itself, and in the pipe that I know most about the seam, instead of running longitudinally, runs spirally around the pipe; and even though the seam was not quite as strong as the iron, it being in a circular form would enable the seam to stand very much more than it would if it was longitudinal.

The President.—Is Mr. Rowland going to make tubes under the Fox patent?

Mr. Root.—That is what I understand.

The President.—The plates are welded, and corrugated afterward?

Mr. Root.—Yes.

Mr. Oberlin Smith.—Does not that stretch the iron?

Mr. Root.—Yes.

Mr. Smith.—It does not draw the tube shorter?

Mr. Root.—No, sir; I understand they are the same length.

Mr. Stratton.—What thickness of metal is used in this circular welded pipe?

Mr. Root.—I have not made anything very heavy yet; about No. 12 is the heaviest I have made; but the difficulty diminishes as you go up in thickness. I have welded as low as No. 28 and made a weld that stood.

No. 184-2.

What is the relation existing, as determined by experience, between the surface speed of a bearing and the pressure which it will sustain?

DISCUSSION.

Mr. Babcock.—So far as my experience goes, this is one of those things that no fellow can find out; it depends upon so many other things. The same bearing will heat at one time, it will not at another. Sometimes it will heat when it is well fitted, and sometimes it will not heat when it is ill fitted. It depends also upon the way in which the pressure comes upon the bearing. A crank-pin will bear a great deal more pressure than a main journal, for the reason that the pressure comes upon it alternately, first upon one side and then upon the other, in such a way that it allows the lubricant to get in between the surfaces. I have found a very marked difference between the crank-pin and the main journal. The condition of the surfaces has a great deal to do with it. I have known a journal which was fitted most perfectly, with the utmost care, to heat, so that it was almost impossible to run it, and then a man who went entirely by the rule of thumb would put in a box fitted with a file, and not half fitted at that, and the journal would never heat afterward. I once had an engine in Milwaukee, a 24 × 48, running 75 revolutions with a high pressure of steam, and the cross-head would heat. It had a very large surface of brass working on cast iron, much larger than is usually allowed, and still it would get very hot. The engineer thought there was nothing so good as rawhide for such a place as that, so he put on a rawhide face. He took the gib out, and with a cold chisel made it rough as a rasp that it might hold the rawhide. It did not run many revolutions before the rawhide was out, and, as the mill could not stop he screwed the gib down to its place without the rawhide and it never heated afterward. There are so many things entering into the conditions that I think it would be impossible for anybody to state a rule which would work under all circumstances, or even a reasonable number of circumstances.

Mr. Towne.—Possibly this question comes to us by reason of our proximity to the ocean, where we have heard a good deal lately of a certain sea-monster [the U. S. S. *Dolphin*], with heated journals and other difficulties. But it seems to me that the trouble

with the question is that it covers too broad ground. In the first place, the behavior of a journal is dependent more directly upon the lubricant used, and the conditions under which that lubricant is introduced, than upon almost any other one condition; and the practice that may be right for light shafting, running at high velocities, will not apply without modification to marine engines. Locomotive practice, again, is quite different, and so on.

Mr. Hamilton.—In two or three instances I have had experience with well-fitted journals heating. After trying every other experiment I could think of, I have draw-filed them a little and the file marks served as buckets to carry the lubricant under the journal, and there was no trouble afterward.

Mr. Bancroft.—I have had the same experience as Mr. Hamilton. With bearings very finely fitted I found they could be made to run cool by taking fine emery paper and laying the grain lengthwise of the journal, so that these slight scratches would carry the oil over the surface.

Mr. Durfee.—I have used with great satisfaction, in a number of instances, for shafts from 10 to 14 inches in diameter, bearings whose "brasses" were hollow, and through which water circulated; the "brasses" being connected to an elevated reservoir by means of a suitable arrangement of pipes. If there was any disposition to heat, the water was turned on the bearings, and in that way they were kept cool and the water did not mix with the lubricating material. In a number of heavy rolling-mill engines I fitted the main shafts with bearings of this kind, and in every case they gave great satisfaction.

Mr. Towne.—I would ask Mr. Hamilton if the grooves he made were longitudinal or circumferential.

Mr. Hamilton.—Longitudinal. It seems that a journal can be so well fitted that it would be better if it was not quite so well turned, so that the lubricant can get between the two surfaces.

Mr. Towne.—There is another kind of tight fitting we all find trouble with occasionally, and that is the too close fit of the engine shaft lengthwise in its journals. In one case that came under my notice, by turning off the collars of the shaft so as to give about one-eighth of an inch end-play, and obtaining actually about one-sixteenth of an inch with corresponding lap-motion in the journals, the heating entirely disappeared, and the journals became beautifully polished and ran with entire satisfaction.

Mr. Oberlin Smith.—I have found that an excellent thing for

keeping journals from heating and cutting is to give them a *scraped fit*, just the same as we give to slides of various kinds. I have used a good many steel-casting shafts for heavy pressures, and sometimes when the boxes have been smoothly bored, and the shafts nicely turned, they have heated and got to cutting when not scraped. Of course the principal advantage of scraping is that we get a bearing all over the surfaces, and the pressure is therefore distributed throughout the whole journal instead of happening on one-half or one-quarter of the surface, as is often the case. But it just occurred to me, since the gentleman spoke of draw-filing his shafts, that perhaps there is virtue in the little "spots" of which the final bearing consists. The rest of the surface does not really touch, and in this case the general bearing is very nice, but at the same time there are all the cavities between the spots, caused by the scraping, which very probably serve to hold the oil better than would a perfectly smooth surface.

Mr. Kent.—The question may be answered in this way. The surface speed allowable for a given pressure will increase with the pressure, or it will decrease with the pressure, or it is independent of the pressure. It must be answered in one of those three ways—either the pressure must be decreased as the speed is increased, or the pressure must be increased as the speed is increased, or the pressure is independent of the speed; and I do not think we have had any answer to-night to show which of these is true.

Mr. Walker.—I wish to call the attention of the members to a bearing which was gotten up some years ago in England to overcome a great difficulty with a very heavy upright shaft. Figure 231 is an illustration of the bearing; *a* is the base or footstep; *b* is the vertical shaft, 9″ in diameter; *c* is a steel toe; *d*, *d*, *d*, are bronze discs; *e*, *e*, are steel discs; *g* is an inlet pipe, and *h* an outlet pipe for a steady flow of water through the annular space shown; *f* is a hydraulic pressure pipe which has a constant pressure applied through pumps and accumulator.

This bearing was constructed for a large cotton mill in Darwen, Lancashire, England, called India Mill. It was six or seven stories high, and like most of the mills in that country, was geared with massive gearing, a vertical shaft being employed with a pair of bevel gears at each floor. This shaft was 9″ diameter at bottom, and finished with a five-inch diameter shaft at top. Altogether the weight of shaft and gears was very excessive. They

tried several plans to make a footstep that would not heat, and finally adopted this one. The idea was to get hydraulic pressure on the bottom of the shaft by means of hydraulic pumps and accumulator, and provide a stream of cold water in the annular space so as to cause circulation and to cool the bearing. Whatever pressure was needed on the end of shaft was got by regulating accumulator and pumps, the shaft being relieved from the bearing to that extent. The use of the discs of different metals was, as you will understand, to prevent the surfaces from cutting, and at the same time, if one disc should cut, any one of the others could begin to operate. I know of no bearing of this kind so large and at the same time so successfully applied. The work for this mill was made by William and John Yates, of Blackburn,

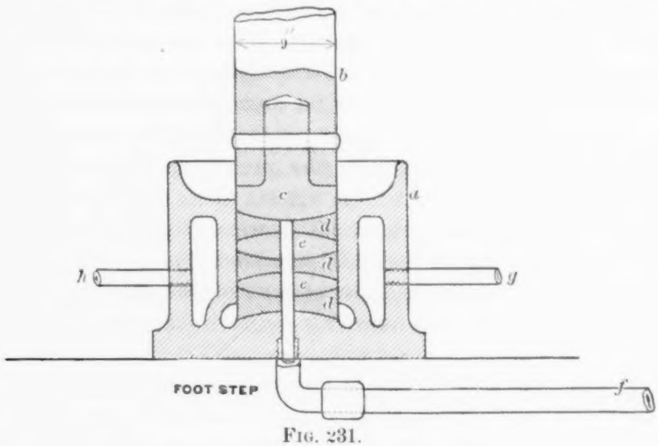


FIG. 231.

Lancashire, England, and was superior in all its appointments. I think this is a very good example of a large piece of work, and although not to the point in the query, seems a very good example of what has been accomplished in bearings.

Mr. A. H. Emery.—The intricacy of this question has been mentioned by one or two members, and to show how much there is in the subject I will mention one instance that occurred in my experiments, and that is, in regard to the condition of the surface. This was a case where I was using hardened steel on non-hardened steel. By merely changing the condition of one of those surfaces which was running on the other, the co-efficient of friction was changed from thirty per cent. to less than three per cent. That was under very high pressure. It shows how very extensive

is the range of the answers that may be given. By changing the surface that was ground on a fine solid emery wheel to the fine finish produced by a polishing wheel, the co-efficient of friction, with the same pieces of steel, and the same pressure per square inch, was reduced with same lubricant from over thirty per cent. to less than three per cent. But I should add that this was with extreme pressures, some of which were more than 50,000 pounds to the square inch.

I might relate a similar experience in regard to the friction of a step bearing, which came under my notice during the course of some experiments upon the efficiency of worm-gearing.

The thrust of the worm was carried by the end of its shaft on hardened steel faces, about three inches diameter, carefully ground.

The end of the shaft had cross grooves, and oil was supplied at the center of the step from a large reservoir through which a circulation was kept up. But these surfaces cut repeatedly, and especially at high speeds, so that for a time no satisfactory results could be obtained. Finally it was suggested that a brass washer should be interposed between these faces, and this proved to be an effectual remedy.

At the end of a long course of experiments, this washer was found to be in perfect condition, having been subjected to pressures ranging from 200 to 6,000 pounds, and to speeds from 3 to 900 revolutions per minute.

I may add in regard to the worm itself, which was 4 in. diameter, and immersed in oil, that its liability to cut appeared to depend on the speed and pressure, but to a greater extent on the speed than on the pressure. The highest efficiencies were obtained at a surface velocity of about 200 feet per minute.

Below this speed there was no evidence of cutting at any pressure, but at 900 feet per minute it was found that the worm was liable to seize under the lightest load, and the conclusion was reached that a certain maximum speed existed, at which the greatest amount of power could be transmitted without danger of cutting.

Mr. Hawkins.—While not exactly germane to the question as formulated, I notice there has been a tendency in the remarks of several gentlemen to show that journals do a little better when they are not well fitted. I would like to mention our experience a little in that direction. In the manufacture of cylinder printing presses, our company have adopted the plan for many years of

making the impression-cylinder shafts of cast iron, cast solid with the cylinders. We have found by experience that a cast-iron journal in a cast-iron box, for this purpose, gives the very finest results; but we find also that the more accurately they are fitted the better is the result in every case. I doubt very much if draw-filing of such a journal would be of any value. In fact, I am quite certain it would be a detriment to it. The only case in my knowledge in ten years where these journals have been abraded is where they have been ill-fitted; and we have never had a case where the journals have been properly fitted but that they have given good satisfaction.

Another point, perhaps, in connection with that is the point made by Mr. Babcock, that these cylinders run under much the same conditions as the crank-pin of a steam engine; that is, they receive intermittent pressure, and to that may be due their success to some extent. They may be large in diameter, but the pressure while it does last is very severe in some classes of these machines. But it has been invariably the case that the better they are fitted the cooler they run and the less trouble they give. I think that this may be almost considered as an axiom in regard to any bearing, notwithstanding the rawhide instance.

Prof. Sweet.—One who is discussing this question lays himself open to the query whether he knows what "well fitted" means. If it is to be believed that cast-iron boxes and steel journals, and cast-iron boxes and cast-iron journals will run well if only fitted with ordinary care, I would advise any one, if he wishes to profit by my experience, not to try it on a steam engine. Get two or three hundred revolutions a minute, and I think it will not work. I have tried it.

Mr. Stratton.—The question presented suggests to me some practical experience on this subject. Up to a very few years ago it was a custom with marine engineers to run brasses loose, with some play between the boxes, and key them up according as you wanted them to fit more closely to the crank-pin. Under those conditions trouble was frequently had from hot crank-pins and hot boxes. The later practice is to run the brasses bound; or "brass and brass," which renders it impossible for them to be keyed too tight on the pin if properattention is given in fitting them. The practice now is when a ship comes in from sea the main journal is disconnected and a piece of pure tin or lead is put in the journal, and the bearing is keyed up to see just what the

clearance is or what the wear has been for the voyage, and a certain minute amount of clearance is then left in the journal for expansion of the pin, and this I believe is the practice with all high speed compound engines we now have running on the coasts. As has been previously stated the condition of the pin is a very important matter. In a certain instance in a large marine engine we had trouble with a hot crank-pin, and I found the pin was originally poorly forged; there were a number of slight surface flaws in it. In this case we went to work and hammered it well with a flat faced hammer, and then went over it with a file and filed off the high spots made at the edge of the hammer marks. The hammering had the effect of closing up the pores and flaws of the iron, and the flat spots made with the hammer allowed the

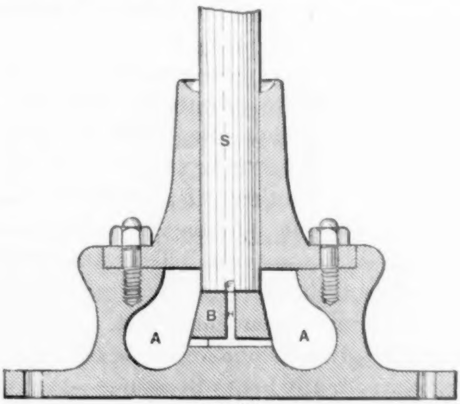


FIG. 221

circulation of the oil to overcome the difficulty we had previously experienced from heating.

Mr. Durfee.—I desire to call the attention of the Society to the construction of a bearing for sustaining the weight of a vertical revolving shaft, which has been found to be very satisfactory in practice. The construction is shown in Fig. 221, which represents a vertical section of the shaft and the bearing. The bearing and footstep are supposed to be formed of cast iron, with an oil well. Directly through the center of the support B, on which the shaft S revolves, a vertical hole H is drilled, intersecting a horizontal hole I. On the end of the vertical revolving shaft S there is a groove F, cut right across the center of the shaft. The oil well being full of oil, as the shaft S revolves the centrifugal force tends to throw every particle of oil out of the groove F. The

consequence is, that there is a continual circulation of oil through the holes H, I, and the groove F, being cut across the shaft, distributes the oil over the upper surface of the shaft support B, at every revolution.

There was another method used in a mill in Boston, many years ago, for supporting a vertical shaft which carried a horizontal fly-wheel weighing, I think, about twenty tons. The rim of that wheel was supported by a number of tie-rods that were attached to the upper portion of the shaft. The rim of the wheel revolved horizontally over a train of rolls. The weight of the fly-wheel and its shaft was sustained on an hydraulic bearing, using oil as the fluid, and if there was any evidence of settlement more oil was pumped into the bearing. The fly wheel ran at a very high speed. It was in use for many years—in fact, until the mill was removed—and worked with entire success.

Mr. Babcock.—This is one of those questions in which it is very difficult, if at all possible, for one to fix a limit of pressure without also establishing a corresponding limit of speed. I mentioned the instance I did, simply to show that what would work at one time would not work at another. I think it has been sufficiently demonstrated here to-night, that the important element is to get the lubricant between the surfaces. That was the whole secret of the rawhide arrangement, of the draw filing, and of the various other methods which have been mentioned. The instance referred to by Mr. Hawkins gives us one example of a very slow running bearing under occasionally heavy pressures—that is, heavy pressure for a portion of the time and pressure reversed for the rest of the time—working very well with a perfect fit; the same bearing, if it had a constant pressure in one direction with the same perfection of fitting, would not last anything like the same time, because the oil would be squeezed out from between the surfaces. I have made many experiments, years ago, as to the pressure per square inch allowable on a bearing. I found that in crank-pins with good fitting, I could allow as high as 1,200 pounds maximum, to the square inch; pins, perhaps four to six inches in diameter, running up to 60 or 70 revolutions, would stand that continuously, without getting warm. The main journal of the same engine would not stand over 300 pounds to the square inch without getting warm. And I repeat again, that it is a question of keeping the oil between the surfaces; if that is secured we can carry a much higher pressure than if it is not.

Mr. Walker.—Did Mr. Babcock in his calculations use a circle or less than a circle?

Mr. Babcock.—I figured it on a section of the pin; the diameter multiplied into the length. I considered that the area.

Mr. Hamilton.—There is one element that we are all omitting, which should be considered with the practical working of a journal—the ability the parts possess for absorbing or carrying off the heat that is being constantly produced while in motion. This is an additional factor and a very important one.

The President.—Mr. Towne states that where the journal is just fitted in between the collars of the shaft, that the least amount of heat imparted to the box, would immediately expand it lengthwise, and nothing starts a crank-pin to heating quicker than to be tight on the collars. Mr. Hawkins, perhaps unintentionally, carried out the same idea advocated by these other gentlemen. Cast iron is more cellular in its structure, and you cannot get as smooth a surface on cast iron as on wrought iron. The cast iron will have cells in it, in which the oil would be carried around with the shaft.

No. 184.—3.

Can any one give data about steam heating of buildings from heating plants in successful operation, including amount of boiler capacity, sizes of mains and returns, amount of radiating surface, its arrangement if indirect, amount and nature of ventilation, temperature maintained in zero weather, coal consumption, water evaporated, and amount of space heated?

DISCUSSION.

Mr. Towne.—There are two or three of these questions to which I can give a brief answer. Some four or five years ago, in designing with my friend, the late Robert Briggs, the heating system for a large group of new buildings, these questions came up, and they were treated in the following general way as to the points inquired of.

The system of heating was by exhaust steam and live steam, employed in two separate series of pipes carried together throughout the buildings. The design contemplated the heating of the rooms to from 60 to 70° F. in the coldest weather, and it was assumed that one square foot of steam surface, one-half of it live and one-

half exhaust steam, in horizontal pipes, would heat 150 cubic feet of space on an average, making judicious allowance for wall and window exposure and other variable conditions. The general rule adopted empirically for determining the sizes of pipe for steam mains and returns was that for each 100 square feet of heating surface in coils or radiators, the cross-sectional area of the pipes should be:

For exhaust steam .24 of a square inch plus a constant of .25 of a square inch;

For live steam .12 square inch plus .25 square inch;

And for the condensed water, .06 square inch plus .25 square inch.

The President.—The time is up.

Mr. Green.—I will give Mr. Towne my time if the President is willing.

Mr. Towne.—The extent of heating surfaces shall be determined, where they are other than horizontal pipe, as follows: It shall refer to the actual measurement of the external surface of any steam-heated pipe, tablet, fitting, or hollow object, and of any pins, ribs or plates solidly attached thereto, when it shall be accepted that 85 per cent. of the vertical surface and 80 per cent. of all surfaces having pins, ribs or plates as a part thereof shall be estimated as equivalent to plain horizontal tubular surface, which is deemed the standard unit of surface to be furnished under this specification.

Mr. Kent.—I think Mr. Towne should be censured for having kept that valuable fact in his possession for four or five years before he presented it. Mr. Babcock has a page in a pamphlet which he has just issued in which he has condensed into the smallest possible form the existing knowledge concerning heating by steam. I know that he spent months on that subject trying to get data from every source, and he did not have Mr. Towne's data, but if he had it, he might have made some change; as it is, he arrives at this new rule—the radiating surface may be calculated by the rule: "Add together the square feet of glass in the window, the number of cubic feet of air required to be changed per minute and $\frac{1}{20}$ of the external surface of wall and roof, multiply this by [the speaker read the rest of the rule]. In regard to cubic feet of space to be heated, Mr. Babcock says cubic feet of space has little to do with the amount of surface required.

Mr. Barrus.—I would like to ask Mr. Towne if I understood him rightly to say that for 100 square feet of radiating surface *the*

return should have an area of .06 of a square inch plus .25 of a square inch?

Mr. Towne.—Yes, for each 100 square feet of surface the multiplier is the first stated decimal, and the constant, to be added, is the second.

Mr. Barrus.—I would like to ask in regard to 80 or 85 per cent. of radiating surface?

Mr. Towne.—80 or 85 per cent. of the radiating surfaces other than plain horizontal tubes. I will read the specification again. "When it shall be accepted that 85 per cent. of vertical surface, and 80 per cent. of surface having pins, ribs, etc., shall be estimated as equivalent to plain horizontal tubular surface." That is to say, if you have one hundred square feet of vertical tube, you only estimate 85 feet of that as available for heating.

I wish to add just one word more, viz.: that in the range of buildings covered by this specification the overhead system of heating was adopted throughout. Three years' trial of that proved so absolutely satisfactory that at considerable expense the old system of heating has been taken out of other buildings and replaced with the overhead system.

Mr. Babcock.—I have in this pamphlet, that Mr. Kent referred to, a rule which I got from Mr. Briggs which is very simple—the diameter of mains leading from the boiler to the radiating surface should be equal in inches to $\frac{1}{10}$ of the square root of the radiating surface in square feet, mains included; which is equivalent to saying that the area of the mains in square inches should be about 8 per cent. of the radiating surface in square feet.

Mr. Partridge.—It is very easy to destroy entirely the efficiency of the overhead method of heating, by an improper arrangement of the pipes. In a factory recently erected on Long Island the steam fitter thought he understood the whole subject perfectly, and he put the overhead pipes so close to the upper corners of the room that the heat was mostly expended in warming the external air. So effectually did they expend their heat in that direction that the building was cold, and the whole system was taken down and rearranged under the windows. The pipes were first put up about 18 inches from the roof and as many from the outside wall.

Mr. Durfee.—I would like to ask Mr. Towne where that system of overhead steam heating was first applied to any great extent?

Mr. Towne.—I think it was in the cotton and woolen mills of

New England twenty-five years ago and more, but with cast-iron pipes. The system that I refer to is entirely of wrought iron, the unit for radiating surface being one and a quarter-inch pipe.

Mr. Walker.—The Cummer Engine Company, of Cleveland, tried the small pipes (say one and a quarter inch diameter) system overhead; it did not prove successful, as the first floor was never warm. The overhead system was abandoned last year and pipes put under work benches near the floor. The cotton mills in England are heated with five-inch thin cast-iron pipes overhead. I would like to know if the overhead system can be made a thorough success, that is, if the ground-floor can be made comfortable.

Mr. Towne.—Most certainly. The subject seems to elicit so much interest, that I will state this: I adopted the overhead system in heating a one-story building, eighty feet in width by three hundred feet in length, with a center bay having side sashes its whole length, and with an unusual amount of glass and exposure; these conditions and its great height making it very doubtful, indeed, whether the overhead system would so fill the upper part of the space as to overflow down into the lower part and properly heat it, and as it was an experiment that I ventured on with a great deal of diffidence, I made provision in my plans for putting in floor radiators in case the overhead system failed to operate fully. It has now been in use two winters, and the floor radiators have not been put in and will not be.

Mr. Geer.—I would state that we have at the Cambria Works, in our machine shops, a floor sixty-five feet in length by thirty feet in width, and it is heated by this system, and it has worked very successfully for two years.

Mr. Durfee.—As an evidence of the efficiency of overhead heating, I know of a shop in Philadelphia where it has been in operation with entire success for at least thirteen years, and I do not know how much longer.

Mr. Partridge.—The success of the heating arrangements in Mr. Towne's shop I tested very satisfactorily to myself three winters ago. I took the temperature at the floor level and six feet above. In the crane shop six feet from the floor the temperature was 70°; at the floor 60°. In the packing room six feet up 69°; at floor 65°. Drawing room six feet up 72°; at floor 72°. In one case through some change of condition I found that the thermometer stood at 68°, two feet from the floor, but promptly rose to 69° at the floor level. In most of the rooms the difference

in temperature of the room between the level of a workman's head and his feet was only one degree. The greatest difference which I found in any room was 4°. In one case the temperature was the same at the floor and anywhere in the room up to six feet. One of the rooms had no machinery in it, and would have been considered extraordinarily difficult to heat by any other system.

Mr. Woodbury.—In the practical application of overhead heating the pipes are hung upon horizontal racks, generally three feet from the walls and three or four feet below the ceiling, varying according to the height of the room. As far as I can learn in early times the first steam-heating pipes were of cast iron and about six inches in diameter. That gave a concentrated amount of heat upon the heads of the help, which was injurious and did not have any redeeming features. The amount of inch and a quarter pipe required for heating mills is about one foot in length for about every ninety cubic feet, although there is a wide range of differences caused by the character of the machinery, both in respect to the amount of the air circulated by the machinery, and also the aid to warming the room by the friction of the journals. In frame spinning rooms in cotton mills the rooms are heated with about one-third the pipe required in some other rooms. At one time, in the desire of learning the measure of success of these overhead pipes in their practical application, letters of inquiry were sent to those using elevated steam-heating pipes, the southern one being in Maryland and the northern one in Canada. Of the forty answers received thirty-eight were favorable; two were unfavorable. Of those unfavorable answers one was where large cast-iron steam pipes were used in a rather low attic room. At the present time they are using them even in the rag-sorting rooms of paper mills, where there is no forced circulation of air by machinery.

Mr. Babcock.—There is a point that might be of interest in this connection, which is, that steam heating is not new. When I was at Pompeii I found the old Roman baths there were heated by steam, and heated in a better and more scientific manner than we use at the present time. The walls were double, and the steam, of course not above atmospheric pressure, was carried up through these walls all around the room—the hot room—the walls being thereby heated to a temperature approximating that of steam, and the occupants of the room were exposed to a radiation from all directions upon their persons. That is the true theory of

heating, and the reason why overhead pipes are successful. Every body—when I say "body" I do not necessarily mean human persons—every *body* is radiating and receiving heat at all times. Heating or cooling is only a question whether or not it radiates more heat than it receives. If you radiate more than you receive, you feel cold and are growing colder; if you receive more heat than you radiate, you are warm and growing warmer. So if you put yourself in a position where you receive ample radiant heat from all sides, then you are comfortable, even if the surrounding air be very cold. This is the reason old-fashioned fire-places make a room so comfortable. The system of steam heating by "indirect radiation" or heating the enveloping air only, the method commonly in vogue, is unscientific, expensive, and uncomfortable.

Mr. Durfee.—Some years ago I heard the late Joseph Harrison, Jr., of Philadelphia, deliver a lecture before the Franklin Institute descriptive of his own boiler, and in the course of his remarks he said, that he had seen in the museum at Naples a boiler substantially of the same construction as our modern upright tubular boiler; this boiler was found at Pompeii, and was made of copper.

No. 184—4.

What is the linear speed at which toothed gears may be driven, and what is the relation thereto of pitch of teeth and their workmanship?

DISCUSSION.

Mr. Walker.—The highest speed in gearing, that I have recently heard of, is that of several large gears built by George H. Corliss. In one case the main gear was nearly 30 feet in diameter, 216 teeth, 5.183 inches pitch, 24 inches face, making 45 revolutions in a pinion of nearly 10 feet in diameter, 72 teeth. The speed on pitch line of these gears will be about 4,200 feet per minute.

Gears of the same size are being built for another place, with face 30 inches wide and 50 revolutions for main wheel. The speed on pitch line of these gears will be about 4,666 feet per minute, a speed I think surpassing anything of the kind attempted before.

Mr. Kent.—I would like to know if the engine at Pullman, Illinois, is not run at a higher speed than that?

Mr. Walker.—The engine at Pullman, Illinois, makes 36 revolutions per minute, making speed on pitch line of gear 3,360 feet per minute. This engine, with same gears, while at the Centennial Exhibition was run at same speed.

Mr. Davis.—I suppose this gearing was cut gearing, was it not?

Mr. Walker.—Yes, sir; and both gears were iron.

Mr. Davis.—I will mention some gearing that our shops have put up at Mahanoy Plains. The wheels are about 15 feet in diameter, and 6 inches pitch, and 18 inches face, dry sand castings, and they are running at 2,700 feet a minute. They are transmitting about 1,600 indicated horse power, and have a very severe strain on them from the fact that driving is done by a figure-eight arrangement of wire rope, two and a half inches in diameter.

Mr. Sweet.—In the attempt to run a train of experimental rolls for rolling wire, by belting direct, it was found that when the belts attained a speed of about 6,000 feet per minute, they would leave the pulleys, and increasing the speed did not increase the speed of the pulleys. Although the belts were of the best and put on at a tension closely approaching their ultimate strength, they would be thrown out by centrifugal force so as to leave the pulleys entirely, and the rolls come to rest.

By the substitution of gears with a driver 2 feet in diameter, driving a pinion $3\frac{1}{2}$ inches diameter, 2 inches face and 4 pitch, the rolls were driven without trouble at the speed of fully 6,000 feet per minute.

It was estimated that the belts to do the work must run upon pulleys twice the diameter of the rolls, while in the case of the gear-wheels the size of gear, on pitch line, is equal to the diameter of the rolls only; the gear-wheels are of steel castings and the pinions machinery steel.

No. 184–5.—No. 184–6.

What is the best method of keeping catalogues and pamphlets for ready reference?

What is the best method of arranging contents of note-books and making data readily accessible?

DISCUSSION.

Mr. Thompson.—I would say that I proposed the fifth question because I have had considerable trouble in trying to preserve

catalogues. I tried the method of binding them up into volumes irrespective of subject, but classified according to size—I found that that does not work very well. I have some ten or twelve volumes that way, and when the manufacturers issue later editions the other editions are of no sort of value. I have tried keeping them in separate pamphlet cases, but they get lost and misplaced, and altogether it is a very serious question to a man who wants to keep posted.

Mr. Kent.—I think the best method is the Amberg letter file, or something of the sort; there are other files of the same make, such as the B. B. file.

Mr. Davis.—To overcome one of the difficulties in the system, instead of binding them in a permanent binder, you can take such a binding as *Engineering* is put in. You take your pamphlet and tie it in with the cords, and you can take out or insert a pamphlet in that way at any time from the back or front or middle. It gives you the advantage of keeping pamphlets of the same size in one volume. You have got to have a card index in order to find them under any system.

Mr. Partridge.—I recently came across a little dodge for keeping files of pamphlets, papers, manuscripts and similar things, which, while it does not call for elaborate preparation, enables one to pile up on a shelf any number of pamphlets and to take any one out which may be wanted and put it back in its place, and to know at a glance whether any one is missing or out of place. A tag is made of the form and size shown. They are cut from cloth-covered envelope paper. Tracing linen also answers very well. It is made of this peculiar form so that it can be pasted in the middle of the pamphlet or circular without interfering with the reading matter. When it is put on the pamphlet it is bent down so that the number is in sight and the pamphlets are piled up then as many as convenient. If you wish to classify, on the lower pamphlet in the pile, a broad tag of the same general form is placed with the name of the class upon it, and that is called one, then the others are numbered right up. I have a stack at home two feet long and twenty inches deep containing twenty-two subjects. It is perfectly easy to take out the whole or any one of them by putting your hands in top and bottom, so that it is equally easy by referring to the card catalogue to take out any particular pamphlet, even though it be as thin as three sheets of paper. Maps go in there and are quite as easily re-

ferred to as Brown & Sharpe's catalogue, which is half an inch in thickness.

Mr. Thompson.—I would like to ask Mr. Partridge if those tags will not lap over each other and hide the numbers?

Mr. Partridge.—You simply run your finger up and you can see every number in the pile. It is also well to put tags on the opposite corners; that is, turn some of the pamphlets upside down and bring the tags on the opposite side of each pile.

Mr. Towne.—I presume I am heretical in being an opponent of the card system, and in saying so notwithstanding that we have a member of the firm of William Sellers & Co. here. I have found in my own experience nothing better than to have a reasonable number of books, of moderate size, with a title on the back, and to have these books indexed alphabetically *through their whole thickness*, the letter A having perhaps ten pages, B twenty, and so on, so that you can run your finger down the edge and pick out any letter. Under the alphabetical head in the proper book I enter the particular report. Occasionally one may be a little at a loss to know which letter you have put a given topic under, but at the most there are but two or three to look to and you very quickly get the one you want.

Mr. Smith.—I would like to ask Mr. Towne if he writes all his memoranda in these books; that is, temporary memoranda that he may have have taken outside of his office.

Mr. Towne.—If they are brief I write them; if they are newspaper cuttings I paste them in.

Mr. Woodbury.—Every person can logically consider that his system of keeping memoranda is the best, because it is the best for his needs. My plan consists of a number of letter-file cases, some of them containing letter files, and also a set of letter files filling several cases for the purpose of holding general correspondence. Most of these boxes are used as pamphlet cases for material bearing upon a certain subject; when a pamphlet refers to more than one subject, a reference slip is placed in the other case. I have a number of sheets of stiff Manila paper cut to fit these boxes, and upon them I make notes and stick clippings after the manner of a scrap book. In this manner I am enabled to have correspondence, notes and pamphlets bearing upon a subject in convenient relations to each other. For a pocket notebook, I use one (illustrating) which contains interchangeable

sheets, which are either cut out, or copied on the large Manila paper sheets referred to previously.

Mr. Kent.—One of the worst troubles in this whole business of keeping notes is the accumulation of them and the different methods and shapes we get them in. Sometimes you get them printed, sometimes on a thin piece of paper and sometimes in a large book. Scraps of paper and small pamphlets had better be kept in self-indexing letter files, but for original notes in writing, about as good a way as any is to have one book called a blotter; write them in that, in chronological order, then go through that book, page by page, at any convenient opportunity, and index whatever is worth indexing. Tod's *Index Rerum* gives us an excellent system of indexing.

Mr. Partridge.—The difficulty in using a blotter or docket of that sort is the same which has prevented the *Index Rerum* from coming into use. The clerical labor involved is too great to be undertaken by a busy man, and it is of a kind that cannot be done by an assistant. It seems to me that the card catalogue is the only feasible method by which one can render his whole fund of information available, including memoranda in books, papers in envelopes, catalogues, circulars, etc., and make the whole practically available. A pile of envelopes, or a bundle of papers can be made perfectly accessible by putting these tags upon them, and in this way I have made my card catalogue an index to all the information which I have, whether it is contained in books or is found on sheets of tissue paper. The use of the tags enables a great deal of matter to be indexed which is usually stowed away in boxes or bundles, and is only found by organizing an exploring expedition.

No. 184-7.

How fast will steel springs open and shut?

DISCUSSION.

Mr. Oberlin Smith.—Mr. President, if any gentleman wants to offer me his minute and a half on this question, after I've used up mine, I shall not refuse it.

The speed of a spring is of very little practical value in itself, without considering connected mechanisms. Of course, it is merely a mathematical matter to work out the average velocity of

a spring and to determine, as far as inertia is concerned, how fast its free end should go, the other end being fixed. The theory of spring action undoubtedly is that they will move at an infinite speed, if not retarded. Molecular friction, the friction of the parts they slide upon, the friction of the air and their own inertia, especially, are the retarding tendencies. As before said, their *inertia* is a matter for calculation and their approximate speed may be found, on the same principle as in the case of falling

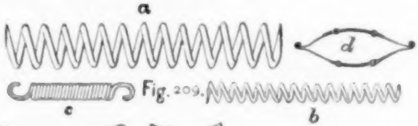


Fig. 209.

Fig. 210.

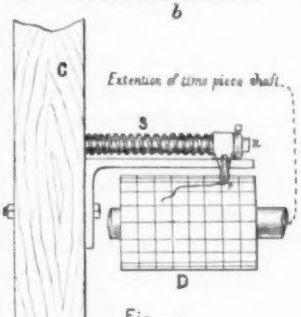


Fig. 211.

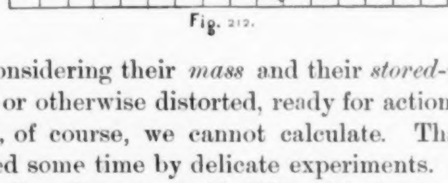


Fig. 212.

bodies, by considering their *mass* and their *stored-up force* when compressed, or otherwise distorted, ready for action. The molecular friction, of course, we cannot calculate. That will have to be ascertained some time by delicate experiments. If these were made with pull-springs, to avoid sliding friction, and *in vacuo*, to eliminate the retardation of the air, some interesting results might be obtained.

Noticing, only the day before yesterday, that this question was to come up, I made a few hasty experiments. These were neces-

sarily not accurate and their results are only of interest to show you as a matter of curiosity, just how fast certain springs I happened to have on hand did move. I had one buggy spring of which *d*, Fig. 209 [referring to sketch on blackboard], is a diagram. This was compressed $2\frac{5}{10}$ inches, and when released executed a movement through that distance in $\frac{7}{1000}$ of a second. I have here in my hand spring *c*, Fig. 209, which was pulled out a distance of $6\frac{6}{10}$ inches and flew back to its normal position in $\frac{32}{1000}$ of a second.

The President.—Has anybody got a minute and a half for Mr. Smith?

Mr. Partridge.—I will give him mine.

Mr. Smith.—Thanks! And please, gentlemen, excuse such rapid talking. This spring [holding it up], *b*, Fig. 209, was compressed $4\frac{4}{10}$ inches: it sprung back in $\frac{66}{1000}$ of a second. This spring [exhibiting it] *a*, Fig. 209, was compressed $3\frac{7}{10}$ inches, and it sprang back in $\frac{28}{1000}$ of a second. I have the weights and number of lbs. compression of these respective springs, but there is not time to mention them now. I here hold up in my hands the chart upon which I recorded automatically the speeds mentioned. It is shown in Fig. 212 and is about $15\frac{1}{4}$ inches long by 8 wide, cross-ruled in inches and tenths thereof. Its vertical scale is $\frac{1}{1}$ and shows *distance*. Its horizontal scale is for *time*, and is 50 inches per second. The chart's length was made to suit the speed at which my "time-piece" happened to be running, viz.: 196 revolutions per minute.

An end view of my extemporized apparatus is shown in Fig. 210, and a side view in Fig. 211. Into a column C was stuck a rod R, on which the spring to be tested, S, pushed a light wooden block, carrying a pencil P. On the time-piece shaft extension was placed the drum D, around which the paper chart was wrapped. The spring was compressed by a bar of iron pulled against the block and let go by slipping the bar off suddenly, the pencil drawing a diagram on the drum as you see here. The carriage spring was not unlimited in its motion. If it had been allowed to go on, it would have shown a gradually slower vibration by a zigzag mark crossing the zero line. It was only allowed, however, to move $2\frac{5}{10}$, plus $\frac{7}{10}$ of an inch, and then minus $\frac{7}{10}$. The other springs were stopped by a collar on rod R. Spring *c* was of course not on the rod, but was hooked to the block and over to a post at the right.

The President.—Has Mr. Smith another friend in the audience?

A Member.—Yes.

Mr. Smith.—My time-piece, I may say, was a "Straight-Line" engine. I requested my assistant to ascertain the exact speed at which it was running, by averaging a series of observations. He told me it was running 196. I said I would like to know the speed respectively of all the observations. Upon receiving this record I found that the first was 196, the second was 196, the third was 196, the fourth was 196, and the fifth was 196. Thus I couldn't possibly get a series of varying numbers from which to strike an average. I will venture to say that I think the Straight-Line engine a perfectly worthless affair for the purpose of getting a series of variable observations of speed.

Regarding the object of speed tests for springs, it seems to me that their chief practical use (aside from musical instrument devices, such as tuning-forks and reeds, which of course are nothing but springs, loaded to obtain proper inertia) is in relation to springs which actuate levers and slides operated by single-acting cams. In such cases *time* is a factor of importance—that is, if the cam rotates rapidly and if its inclines are steep. The spring in question must be strong enough to overcome its own inertia, *plus* that of its *load*, with such rapidity as to make the lever or slide forming the load "follow" the cam smoothly.

Mr. Babcock.—The question admits of two answers—the number of vibrations, and the number of feet per second which a spring can make, and I am not sure which is meant. If the first, it depends upon the size and stiffness, as in the notes of a music box. Some twenty-three years ago I was called upon to design a spring motor for sewing machines, and made a large number of experiments to determine the power of springs. I found this varied greatly with their form and size. The weight which would bend the spring to its elastic limit was ascertained, which, assuming the elasticity to be uniform, was just twice the force the spring would exert through the distance moved. From this was deduced the number of feet which the spring would lift its own weight. I have no data of those experiments at hand, but from memory should say that this varied from 25 to 150 feet, the lowest being a heavy flat spring, and the highest a coiled clock spring. A watch spring would doubtless give higher results. These figures give an initial velocity of from 40 to 100 feet per second, but as one end of a spring is stationary, and when moving itself only

it moves but half its weight, it would lift that half twice as much, or 50 to 300 feet, giving an initial velocity at the center of motion of from 56 to 138 feet, while the free end of a coil or straight spring would move at twice that velocity, or, say, 112 to 276 feet per second.

Mr. Bancroft.—Some experiments were made last year by Mr. Wilfred Lewis, who was at this meeting, to ascertain the foot-pounds of energy that could be stored in steel springs of any given kind. The experiment was in reference to the spring motor which was talked of in Philadelphia for passenger cars, and the results of his investigations would seem to show that with the weight and kind of spring that was proposed, the motor would not be able to ascend a hill more than 20 feet in height, but with the springs that he experimented with, coming down to a very much smaller size, he succeeded in getting a much higher duty.

In one experiment a torsion door-spring, $\frac{3}{16}''$ diameter, $3\frac{1}{2}$ feet long, was twisted through an angle of 180° by a weight of 8 lbs., acting at a radius of 13″, taking a permanent set of 5°. The greatest shearing stress was 80,000 lbs. per square inch, and the resilience about 43 foot-pounds per pound of spring. A clock spring, $\frac{3}{8}''$ wide, .014″ thick, weighed 605 grs., and was consequently 60″ long; this spring was in the usual coiled form and, when wound up 12 turns from a state of rest, supported one pound at a radius of 3″; the tension was in all cases proportional to the number of turns, and no set was apparent after unwinding.

This load gives a resilience of 108 foot-pounds per pound, and assuming the neutral axis to be in the middle of the ribbon, there must have been a transverse resistance of 240,000 lbs. per square inch upon the outside fibers.

A short piece of this spring supported, when fixed at one end as a cantilever, one pound at a radius of 4″, when an apparent set took place. The transverse strength of the steel was thus found to be over 320,000 lbs., with an elastic limit closely approaching that amount.

Another spring, $\frac{1}{2}''$ wide by .022″ thick, weighing 2,040 grs., showed even better results, giving 154 foot-pounds per pound, and showing a transverse elastic resistance of 300,000 lbs. per square inch.

With reference to the use of springs in a car motor, he says: "Assuming 154 foot-pounds per pound, which is probably far in excess of what can be obtained on a large scale, and allowing $\frac{1}{4}$ of

the total weight for the weight of the springs, and 50 per cent. for the efficiency of the driving mechanism, we have about 20 foot-pounds of available energy per pound of load moved." Evidently this would move a car but a very short distance over ordinary street tracks.

Those who may be interested in the subject will find further data in a paper on the "Resilience of Steel," read by Mr. Lewis before the Engineers' Club of Philadelphia, and published in their proceedings, November, 1884.

Mr. Babcock.—Some thirty years ago or more a Frenchman proposed to put a spring in front of a locomotive, so that when two locomotives ran together the spring would take up the momentum and no harm would result. I forget how many millions of tons of steel it required to produce the effect, but it was something enormous.

APPENDIX V.

MEMORIAL NOTICES OF MEMBERS OF THE SOCIETY

DECEASED DURING THE YEAR.

J. H. BURNETT

was born at Cazenovia, N. Y., August 21, 1826. He learned the machinist trade with Wright & Smith, of Newark, N. J., completing his apprenticeship in 1872. After that time he was engaged in the practical side of mechanical engineering, having made several inventions in use in connection with car building, machine tools, etc., and for rubber manufacture. He entered the Society at the Philadelphia meeting in 1882, and at that time was manager of the Usudurian Steam Packing Department of the Woonsocket Rubber Company, with offices in New York City.

His death was due to consumption, and took place on the 31st of January, 1885, after an illness of six months.

HORACE LORD

was born in Windsor, Conn., in 1815, and died in the city of Hartford, February 2, 1885. He began to learn his trade as machinist in Springfield with Zelotes Lombard in 1832, and in 1836 the latter secured a chance for him in the Springfield Armory, where he was engaged six years designing and building tools and machinery for special uses. On the suspension of the Armory in 1842, Mr. Lord became foreman of Whitneyville Armory, under Mr. Eli Whitney, where he remained four years until he moved to Hartford to become connected with Colt's Armory. He was eight years foreman of Colt's Armory, and for upwards of twenty years until the time of his death had been Superintendent of Colt's Patent Fire-arms Manufacturing Company.

He joined the American Society of Mechanical Engineers at the first annual meeting in 1880. A friend says of him: "Those who knew Mr. Lord the most intimately appreciated him the most. Honest, energetic, kind-hearted and generous, active in every good work, respected and beloved by neighbors and by those under as well as over him, his loss is felt especially by the mechanics and laboring classes with whom he was associated more than with any other class of his fellow citizens."

DAVID HOWARD HOTCHKISS

was born in the city of Syracuse, January 8, 1856, and died in that city April 29, 1885. From the public schools of his native city he passed at the age of fourteen to a preparatory school at Laurenceville, N. Y., with a view to entering Harvard College. At the solicitation of his parents this plan was given up, and he matriculated at Syracuse University with the class of 1876. In Sophomore year he showed so evident a disinclination to the professions of either law or divinity, that he left college and entered mercantile life in the jewelry store of C. S. Ball. His strong inclination for mechanics had been early shown, and the commercial side of his business was very distasteful to him, so that after much persuasion his guardian finally allowed him to enter the factory of William Duncan to learn the trade of a manufacturing jeweler. On the death of his father, in 1874, he re-entered the University as he had hoped to do, and graduated in 1880.

Mr. Hotchkiss took one-fifth of the stock of the Straight-Line Engine Company when it was formed on February 1, 1880, and at first acted as draftsman, and afterward as assistant superintendent and in charge of the accounts. On February 5, 1885, he was elected Secretary of the company, and having grown up with the business he was thoroughly conversant with it, and was practically second only to its projector in influence and interest. He was elected one of the first junior members of the American Society of Mechanical Engineers at the meeting at Altoona in August, 1880. A friend writes of him: "He was one of the few young men who, left an orphan with a surplus of worldly goods, was making a worthy application of it. He had a life of usefulness before him, and there are many who have occasion to mourn his loss."

HENRI TRESCA,

honorary member of the American Society of Mechanical Engineers, was born at Dunquerque in the year 1814, and was therefore about seventy-one years of age at the time of his death, June 24, 1885. He evinced a fondness for scientific and engineering pursuits at an early age, and, following his natural bent, was educated at L'Ecole Polytechnique, graduating with a high reputation for scholarship and ability. Nearly thirty years ago he became connected with the Conservatoire des Arts et Metiers as its sub-direc-

tor, and remained attached to that institution until his death. The director, General Morin, was his friend as well as colleague, until the death of the latter, when M. Tresca succeeded him. Many of the investigations and researches made and published by General Morin, were made with the aid, and often mainly by the work of M. Tresca, who also made a very considerable number of independent researches. He was Professor of Industrial Mechanics at the Polytechnic, and of Applied Mechanics at L'Ecole Centrale des Arts et Manufactures. He became a member of the Institute in 1872, upon the death of M. Combes, the distinguished scientific writer, investigator, and author, especially of works upon thermodynamics, when the science was in its infancy. M. Tresca was not a prolific writer, and is best known by his papers on the flow of metals, and upon the working of metals, in which two directions he made extended investigations, and reached some very interesting and important results. These two works are published in small quarto; all other writings seem to have been only published as papers before societies, and in technical periodicals. Among the latter are a number describing some of the earliest trials of air and gas engines. These trials were usually made at the Conservatoire for his own satisfaction, and contain some of the earliest illustrations of the scientific method applied in engineering. The earliest information in relation to the efficiency of the Lenoir, and of the Hugon gas engines, is obtained from the reports of M. Tresca.

M. Tresca was almost invariably a delegate from France to the International Industrial Exhibitions, and was always placed upon those sections of the juries which had cognizance of the various prime motors, and more important machinery of the arts. In the discussions preceding the making of awards, he always gave evidence of a very thorough appreciation of the skill and ingenuity of American mechanics. He was always ready, not merely to admit their claims to consideration, but would usually very gladly lead the way in moving the awards given them. In the exhibition of 1873, at Vienna, as an illustration, Mr. Geo. H. Corliss made no exhibit. It was proposed by the American commissioner in that "Group" to award to Mr. Corliss the highest award, a "Diploma of Honor," as the most distinguished inventor of the steam engine. It was objected that Mr. Corliss was not an exhibitor; the American commissioner rejoined that he was an exhibitor, as he was represented in every department of the ex-

hibition, and that whatever of merit was exhibited in that class—steam engines—was the result of the skill, ingenuity, and pluck of Mr. Corliss. M. Tresca was the first to rise, and to second the claims of the American inventor, and his remarks were so earnest and so thoroughly to the point that the award was made without another dissenting voice being raised. The presence of M. Tresca was always welcomed by the exhibitors of worthy inventions and of good machinery, as that of a good and a fair judge, and his expression of opinion always had very great weight with his colleagues. His later work has included very interesting and valuable investigations of the efficiency of electro-dynamic transmission of power, especially over considerable distances. He was elected an honorary member of the American Society of Mechanical Engineers at the annual meeting in 1882. The death of M. Tresca is a very great loss to the profession.

HENRY H. GORRINGE

was born in the West Indies in 1840, and died in New York City, July 6, 1885. He came to New York in his youth, and was appointed from this State to the navy in July, 1862. He served through the war in the Mississippi squadron, taking part in nearly all the important battles of that part of the fleet. Beginning as master's mate he was three times promoted for gallantry in battle. After the close of the war, he was assigned for two years as commander of the S.S. *Memphis* of the Atlantic squadron, and in 1868, while on duty at the Brooklyn Navy Yard, he was duly commissioned a lieutenant-commander. From 1869 to 1871 he commanded the S.S. *Portsmouth* in the South Atlantic, from 1872 to 1876 he was at the Hydrographic Office at Washington, and from 1876 to 1878 he was on special service with the S.S. *Gettysburg* on survey duty on the south coast of the Mediterranean. It was at the close of this assignment that Mr. Gorringe obtained a long furlough from the Navy Department to undertake the removal of the obelisk known as Cleopatra's Needle from Alexandria to New York City. New York capital having furnished the necessary means, to Mr. Gorringe was entrusted the responsibility of effecting the shipment and erection of the monolith. Improving on what had been done in similar efforts before, the removal was satisfactorily carried out, and on July 20, 1880, the S.S. *Dessoug* returned to New York with the obelisk on board. He published an account of this work (for which he had undertaken considera-

ble preliminary study) in book form, and reference may be made to that for the engineering details of the work. Trunnions were bolted to the vertical shaft on an axis through its center of gravity, these trunnions turning in bearings built up under them. The obelisk being lifted off from its bearings by jacks under the trunnion journals, the shaft was turned over to a horizontal position and lowered by the jacks and blocking to the ground. Plates were removed from the fore-quarter of the ship, and the obelisk was thus put on board in the hold while the vessel was in dry dock. The shaft was re-erected by reversing the method used in getting it down. After it had been duly erected and presented to the city, Mr. Gorringe had opportunity for remunerative expert work during the continuance of his furlough, but as the result of certain correspondence with the Secretary of the Navy, he severed his connection with the navy in February, 1883. Since that time he had been manager of the American Shipbuilding Co., with yards near Philadelphia.

Mr. Gorringe entered the Society at the Altoona (IIId) meeting in 1881. His death was the result of an accident in jumping from a moving train early in the winter. The fall accelerated a cancerous affection near the spinal cord, from which death finally ensued.

APPENDIX VI.

DISCUSSION

ON THE ADOPTION BY A LETTER BALLOT OF THE REPORT OF A COMMITTEE OF THE SOCIETY ON A STANDARD METHOD OF CONDUCTING STEAM-BOILER TRIALS.

[*Note.—It has been thought advisable to print this discussion in full, as an appendix to the volume, in order to put upon record, as a precedent, the decision reached by the Society in the matter of official adoption of a Report of one of its Committees. The discussion took place at the XIth meeting of the Society, held in May,* 1885, *at Atlantic City, N. J.*]

The Report of the Committee of the Society on a Standard Method of Conducting Steam-Boiler Trials had been sent to every member before the meeting. Certain gentlemen had sent on their views in advance of the meeting, and these papers had been printed for circulation as Discussion of the Report, and headed with the following note:

"NOTE.—The Report of the Committee was formally presented at the New York Meeting of the Society, November, 1884. On account of its thoroughness and magnitude it was not discussed at that meeting, but it was ordered that it should be printed and sent to all the members before the spring meeting, that it might receive their careful examination. The discussion of that Report being made a special order for the second session of the meeting of May, 1885, the following suggestions were presented. The council had directed that the final vote on the adoption of the Report should be made by letter ballot, to the end that all the members of the Society might have a voice in so important a matter. The discussion was as follows:"

The debate on this branch of the subject was opened by the reading of the above note by the Chairman of the Committee, who proceeded as follows:

Mr. Kent.—I understand that the action of the Council to which the Secretary refers in the above note was taken in view of the suggestion made by Mr. A. F. Nagle, of Chicago, that such a letter ballot ought to be taken by the Society, for the purpose of giving greater weight to the code than could be given to it by the Committee themselves.

Mr. Nagle's position was as follows, quoted from his letter: "* * * I wish to express to you the importance of a letter ballot on the adoption of the Report on the Standard Method of Conducting Boiler Tests. In order to give said Report the value it merits, particularly in lawsuits where boiler capacities are in dispute, it would be important to quote the vote of the Society—as 714 for it, 62 against, 90 not voting; or as the case may be. The adoption by a *viva voce* vote would not be of sufficient force in a suit to give it the weight it merits."

It is easy to see that, especially if the names and addresses were given of those who voted affirmatively or negatively, it would be possible for a court to decide on the standing and weight of the Report. It seems that the Council agreed with these suggestions of Mr. Nagle, and ordered a letter ballot, according to the note placed at the head of the pamphlet. The Secretary then wrote to the individual members of the Committee, and asked what disposition should be made of the suggestions and amendments proposed to the Committee by Prof.

Trowbridge and others, and I think the opinion of some of the members of the Committee is given in a letter which I addressed to the Secretary, which I will read.

"I think that the discussion should be printed in the 'Transactions' with the Report, after it has been revised by the authors in the light of the discussion at the Atlantic City meeting, for the purpose of keeping a permanent record of the whole matter. I would insist on the discussion being pulled to pieces as thoroughly as the Report was, before it is finally printed, and that authors have full liberty to withdraw their remarks, if they find they have been based upon misconception of the meaning of the Report. Such discussion can certainly not be considered as an amendment to the Report, or even as a proposed amendment, unless it is written out in the form of an amendment, distinctly specifying the words to be omitted in the Report, and the words to be substituted.

The Committee may accept such an amendment if they choose, and modify their Report accordingly, if they find they have made any mistake, but if they reject the proposed amendment, the Report as a Report must stand as it is printed.

The submission of the Report for adoption by the whole Society is another matter. If proposers of amendments insist upon them after rejection by the Committee, they may propose them to the Society at the Atlantic City meeting, and obtain a vote upon the question: Shall Mr. Blank's amendment be submitted for letter ballot?

If decided in the affirmative, the letter ballot may be had on several questions. Thus:

1. Shall proposed amendment number 1 be added to (or substituted for) paragraph so and so of the Report?

2. 3. Same for other proposed amendments.

4. Shall the Report be adopted by the Society as modified by amendments 1, 2, 3, or such of them as receive a majority vote of the Society?

5. Shall the Report be adopted without amendment?

Yours truly, WM. KENT."

The whole question was then further complicated by a letter from Prof. Thurston to the Secretary.

* * * * * * * * * *

"My own personal opinion in regard to the action advisable in relation to the Report of the Committee on Boiler Trials is, that no other action is needed or desirable than the usual vote of thanks to the Committee, and the acceptance of the Report—provided the Report be acceptable. I do not think it well for the Society to make a precedent of formally adopting and making the Society as a body responsible for a specific system, like that proposed by the Committee. It should be taken, in my opinion, as simply the expression of the views of the Committee, and it will have a weight (as it ought) simply proportional to the weight of the Committee. If the Committee are to accept their task as one of representing the aggregate opinion of a society, or if the Society can in any possible way be brought to promulgate the reports of its committees as if representing a received platform of the Society itself, I fear it may involve us at some future time in serious difficulties. It might even lead to attempts to use the Society through the action of small committees.

I would therefore say, simply accept the report and discharge the Committee

in the usual way, and let any debate on the Report take the usual course. If the Committee is discharged it would be satisfactory to the Committee itself, no doubt. Very respectfully yours,

R. H. THURSTON.

P. S.—If the Report be right, it will be accepted by the profession, whether indorsed by the Society or not; if wrong, the Society cannot help it, and will only be itself injured by its formal indorsement."

I think that letter was written by Prof. Thurston without hearing the arguments in favor of the other side, raised by Mr. Nagle and others; but the letter appealed so strongly to the later views of the Council that the Council, I am officially informed, have rescinded their action ordering a letter ballot, since the Secretary's note was printed, and the matter now stands open for action by the Society. I think the whole question may therefore be submitted to the Society whether a letter ballot ought to be taken on this Report, and I want to bring up that topic for discussion at some session of this meeting.

Mr. Charles E. Emery.—I think it is well to try and dispose of this subject while we have it in mind. Mr. Kent does not seem to recollect the suggestion I made to him at the meeting of the Committee—that it be stated that an exception is made in this particular case. I believe myself that a general rule should be adopted that the Society is not responsible for the opinions of its members or its committees. In this particular case there are important reasons why an exception should be made, and I think in drafting a resolution in reference to the subject that it should be expressly stated that this *is* an exception, and that it is not the policy of the Society to indorse the opinions of its committees as a general rule. It is thought that this plan will overcome the difficulties which Professor Thurston has so ably stated, and at the same time give this boiler report a basis of authority greater, as Mr. Nagle has stated, than that of the members of the Committee simply. I would be pleased if some member, not on the Committee, would move that this proposition be submitted to letter ballot; I think that action the proper one to take if the qualification referred to be embodied in the resolution.

Mr. Sweet.—While I was hearing the discussion on the Report, I made up my mind to make that motion, not knowing that anything had been proposed in that direction. I make a motion that the code, after copies of it have been received and the discussion has been amended, be submitted to all members of the Society by letter ballot, and with this statement that this is a special case, and that it is not to be considered hereafter as a precedent.

Mr. Kent.—I would ask, then, what shall be done with the suggestions of Mr. Barrus and Professor Trowbridge? I understand that the Committee, so far as I know its feelings in the matter, will not accept the amendments of either. No other proposed amendments have been made, and I think it would be questionable whether these should be submitted to the Society.

Mr. Root.—I would like to ask the Committee if their work is not substantially a corroboration of the Report of the Centennial Committee. Their unit is about the same thing.

Mr. Kent.—It is just the same.

Mr. Root.—Then the Report of this Committee being the same as the Report of the Centennial Committee, I should think would carry very great weight with it. Two committees investigating a subject of that kind and reporting in the same manner would in itself have considerable weight. I do not think that the

letter ballot could have much effect one way or the other, but I see how it might compromise the Society and lead to forming a precedent which might afterward be embarrassing. It seems to me that it is only necessary to have the Report duly brought before us, and then to let the Society make that Report as prominent as they choose. The Report of the Committee coinciding with that of the Centennial Committee it seems to me that the whole matter has received a sufficient indorsement, and it would have just as much weight as though the Society voted upon it.

Mr. Green.—I second Professor Sweet's motion.

Mr. Kent.—The Committee of Judges of the Centennial did not make any code or any proposition or statement of what should be considered in future; but merely in their rep rt in speaking of the different things about the boilers they rated a horse-power on the basis of thirty pounds, etc., but I do not believe that they put it in the form of a code or as a proposition for future use, or anything of that kind. That Report is published by Lippincott & Co. at $2.00, and unfortunately few people know of its existence, so it has not been widely enough published in that form.

Mr. Durfee.—While I recognize the great value of the work of this Committee, and am disposed to agree with their conclusions, I will ask the Society, before they submit this Report to a letter ballot, to consider the possibility of a negative conclusion. It is possible that the Society, by a very small majority, might vote in opposition to establishing this report as a standard, and in that case the Committee and the Society also would be in an unpleasant position, and I think myself it had better be left as it is.

Mr. Kent.—I think the Committee are willing to take that risk.

Mr. Stratton.—As I understand it, at present there is no adopted standard of horse-power in this direction except that established for its own guidance by the Centennial Commission. It seems to me that it is desirable that such a standard should be adopted by some organization, and as I know of none better qualified to establish such standards than our own, I certainly hope the motion will prevail.

Mr. Babcock.—I do not exactly know what would be the benefit of such a vote (if this matter is to be discussed at the present time), whether the adoption of a standard of this kind by this Society would have any special force outside of this Society. I presume it would not have any legal force, but it might have some moral force. It is undoubtedly desirable that there should be a recognized standard for boiler horse-power. The Franklin Institute in Philadelphia saw the necessity of it some years ago, and appointed a committee for the purpose of fixing a standard which the Institute might adopt and cause to become a standard for the country, the same as they adopted a standard for screw threads. That Committee reported in favor of the old Watt standard of a cubic foot of water to the horse-power, but the Society refused to adopt it. If the adoption of this standard by this Society would secure its adoption by the world, it would certainly be a desirable thing to do. If it would not give it any more force than would the publication of the Report of the Committee in our Transactions, then it would not be worth while for us to adopt it and so establish a precedent which might afterward involve us in difficulty. It certainly is desirable, as has been expressed, that if this question is submitted to a ballot, it be expressly stated in the resolution and put upon the record that this shall not in any way be a precedent for the future. We do not want to be obliged to vote upon such questions always as they arise, and put ourselves upon record that a certain thing is just right, when subsequent investigations might show that we were mistaken.

Mr. Kent.—I am surprised that Mr. Babcock thinks that the decision would not have any legal effect. I think he knows that he himself in times past has had lawsuits in which this very question has come up, and he was obliged to employ experts to swear that a horse-power was thirty pounds, against the contrary evidence of another expert. Some of his experts said thirty pounds under one condition and some said thirty pounds under another, but the question did have legal weight, and I believe the court decided that thirty pounds was the horse-power according to the weight of evidence; that was the Superior Court of New York City. In Cincinnati, also, I know the same question was raised in a case. No member of the Mechanical Engineers, when summoned in court and asked what a horse-power was, could say that the Society's report did not amount to anything, and that a horse-power was still sixty-two pounds, after he had voted that it should be called thirty pounds.

Mr. Green.—I would suggest that while the decision of this Society might not have legal force, it would be a mile-stone on a path that has not been traveled very far yet. It is a white stone set up by which a man can find the place he has got to.

Mr. Durfee.—I do not think Mr. Kent quite understood the full force of my suggestion. I think with him that a majority of this Society would be in favor of the Report of the Committee; but the vote may turn entirely upon the question of the policy of indorsing it by letter ballot. If the vote should go in the negative it will accomplish a very unpleasant result. It will say to the public, who do not understand the true inwardness of this matter, that the Society are not in favor of the Report of the Committee; whereas the majority of the Society may be in favor of the Report, believing in its value as I do myself, but having a decided doubt as to the policy of committing the Society as a whole to its adoption.

Mr. Kent.—Those people can refrain from voting.

Mr. Couch.—I do not think there is any doubt, after the discussion that has taken place, that the Society will sanction the standard of horse-power, stated by the Committee, and I think it certainly very desirable that Professor Sweet's motion should prevail, and that each member should have opportunity to give his sanction to it. I think it is very desirable, however, that the point should be distinctly kept in mind (and perhaps more distinctly expressed in the Committee's Report) that, whereas the ability to evaporate 30 pounds per hour at 70 pounds pressure from feed water at 100 degrees Fahrenheit is equivalent to an actual horse-power, a boiler, for each commercial horse-power, should be capable of evaporating a quantity one-third greater; because, if it is simply required that the boiler should be capable of evaporating 30 pounds, the purchaser and manufacturer may differ greatly as to the amount of forcing admissible. Of course our decision cannot have any legal force except where it is made the basis of a contract.

Mr. Chas. E. Emery.—I will add some remarks as to the way the courts will probably look at this matter. I have been called upon several times to testify in boiler cases, and the first question in court is, what do you yourself know about the matter? The statements of others are in the nature of hear-say evidence, and are ruled out at once. In relation to the proposed standard of 30 pounds of feed-water per horse-power per hour, I personally ascertained in the year 1869, at the Fair of the American Institute, that standard high pressure engines of medium size (60 to 80 horse-power) could be operated for 28 pounds of feed water per horse-power per hour, and that those a little smaller would require 30 pounds. Pro-

fessor Thurston, another member of the Committee, ascertained similar facts the year after, at the same place, during a trial of one of the engines of Mr. Porter, a third member of the Committee. These facts ascertained almost in coincidence by three members of the Committee, are in addition to and independent of other similar facts, coming under their notice, and the other members of the Committee have had similar opportunities personally to ascertain the cost of steam power. Personally, therefore, all may feel more complimented to have the report stand on the opinion of the members of the Committee without any action of the Society. The majority of the people throughout the country do not, however, know the members of the Committee. The judges of the courts do not, and it might be well if the subject be voted upon, that the Secretary mention in the accompanying circular, that one object of the letter ballot was to show that the Society as a whole had confidence in the judgment of the members who have been appointed on this Committee. The special statement should also be made that this action of the Society is an exceptional case, and that the Society does not, as a rule, act upon individual opinions or the reports of Committees If the subject were acted upon and promulgated in this shape, I think the indorsement of the Society would be valuable. It occurs to me to suggest further that the letter ballot be in such form that the names and residences of those who vote for and against the adoption of the Report, may be published in the Transactions of the Society. I will ask Professor Sweet to embody that in his resolution.

Professor Sweet.—I was going to suggest that the vote on my resolution be postponed until we have the resolution written out, as it will appear on the ballot, and presented in its entirety to the Society.

Mr. Durfee.—I submit, that if the action of this Society is to be brought into court, the only way that it can be legally offered and placed before the judge or the jury, is for the officers of this Society to come into court and make oath that on such an occasion, and under such circumstances, a particular action of the Society was taken. We are going to establish an expert business for the President and Secretary of the Society, if we adopt this report in this way. The mere testimony of a single engineer would not be evidence of the action of the Society. He could only say, that *he believed* that the Society took certain action; but the legal evidence of that action is the records of the Society itself, and the persons to bring these before the court are the officers of the Society.

The President.—The suggestion of Professor Sweet is a good one. But for the purposes of the discussion of the matter now, perhaps it is well enough understood, and we may discuss it without reference to the official adoption of the resolution which will take place afterward, after it has been written out with somewhat careful attention.

Mr. Partridge.—In view of the fact that this is one of the most important resolutions that have ever come before this Society, it seems to me that we need more time for deliberation over Professor Sweet's motion. I think that the publication of the names of those who vote on this subject would be an exceeding injustice to those members who vote solely with reference to it as a precedent for the Society, without any regard whatever to the report itself. For myself, as I feel at present, I should vote against Professor Sweet's resolution, while I should most heartily indorse the report of the Committee. I presume there are many others who feel in the same way, and to publish the names of those voting in the negative would be to say that they do not believe that the Committee were right, while the real significance of the vote would be that they do not wish to put the

Society in the position of indorsing any Report of a similar character. No matter how we may word our motion, it will be regarded hereafter as a precedent. We cannot avoid that, and while I do not foresee definitely the circumstances likely to embarrass us, I can see that they may come up in the immediate future, and that we may find ourselves in an unfortunate predicament.

Mr. Oberlin Smith.—Although I do not know that I am wholly in favor of having this ballot, I must say that if we do take it I agree with Mr. Partridge in thinking it to be unfair to publish the voters' names. I cannot see how a thing can be called a ballot if the names are all to be published. The inference regarding a ballot is that the thing is to be secret. I must say that I disagree with two things that Mr. Durfee said; one is that it would put the Society in a bad light if they voted against this measure. I do not think so. They would simply say that they did not agree with the Committee. It would simply show that several hundred verdicts overpowered the five verdicts. About its not being legal evidence unless the Secretary and President appeared at court, I do not see that there is anything in it. I do not see why a published volume of transactions, which it is perfectly evident was published at a certain date, is not as good legal evidence as any other facts which have been printed and made known to the world. The presentation of a volume of "Transactions" would, I think, be perfectly legal evidence that the Society had adopted this Report.

Mr. Kent.—As to the legal matter I will say there is no necessity whatever for so identifying the Transactions as a basis of evidence. I have had some experience in court in which the transactions of societies have come in. A man would swear that a certain piece of metal, because it contained only ten per cent. of carbon and would not harden, was not steel but iron, and I would hand him a volume of Transactions and say to him: "Did you write this paper that has your name to it, in which you described such a product?" "Yes." "Did you call it iron or steel?" "Well, I called it steel, there," he would say. The question whether steel has been used for boiler plate has been brought in in the same way. I would bring a member of the Master Mechanics' Association and ask him to identify a volume of the Transactions of that association. I would ask him: "Were you present at that meeting?" "Yes." "Did you hear the question of boiler plate discussed?" "Yes." "Was anything said about steel boiler plate being used in this country?" "Yes." That would refresh his memory completely. And that is the way the question might be raised about this standard. Every one here and every one that takes part in the vote might be brought as a witness to testify with regard to it, and the Transactions be used to refresh his memory.

The President.—I think the difficulty of the matter comes in in this way. The feeling prevailing in the Society would favor a vote agreeing with the suggestions of the Committee; but when you bring the matter before the membership in a letter ballot, the question will come in this form: Shall I vote for this thing of which I approve and yet to adopt which is contrary to my conviction of what the Society should do as a matter of policy and precedent? The chances are that he will vote against adopting the Report, even though he agrees with it and thinks it is right. The difficulty is to divorce these two points in one vote.

Mr. Kent.—I think that can be got over by letting a man have three votes. He can vote for the Report, or against it, or decline to vote at all. With regard to the negative votes they should be divided into two classes, one negativing the Report of the Committee, and one negativing the action of the Society in indorsing anything.

The President.—Professor Sweet, I suppose, will take that point in view in writing out the resolution.

Mr. Partridge.—In the New England town-meeting there was formerly a peculiar custom which might well be adopted in this case. It was called "taking the sense of the meeting." If we could put our vote into this form we might divorce the question of adopting the code from that of the individual sanction of the members. A resolution in this form would remove most of the objections which have been made to the letter ballot and to the motion as made by Professor Sweet. Taking the sense of the meeting would bring out the feeling of the members in regard to the Report without the necessity of a formal ballot, to which many of us object purely on grounds connected with the policy of the Society.

Mr. Babcock.—I would suggest making two ballots:—first, are you in favor of this Society indorsing or adopting a standard of this character? Then, second, if it be decided by the other ballot that the Society are in favor of adopting a standard, would you vote to adopt this or not? That seems to me would divorce the question from the two difficulties under which Mr. Partridge finds himself laboring. He says he should vote against it on the ground that he is not in favor of the Society's adopting any standard, but he could thus express his views on both questions at issue.

Mr. Oberlin Smith.—Has this Committee conferred with kindred societies in this country or in Europe with reference to this matter?

Mr. Kent.—They have not, and I do not think they will. I think we are proud enough and conceited enough to believe that there is no society in this country which is as capable of deciding this question as the American Society of Mechanical Engineers.

Mr. Smith.—I agree entirely with Mr. Kent. At the same time, we could be backed up by the little fellows around us. Would it not be better to leave this whole question until the Fall Meeting, and see if we can get the indorsement, as some gentlemen suggested, of the Franklin Institute and others, and then vote on the question by letter ballot?

Mr. Kent.—I did not mean any disrespect to the other societies by what I said. But the American Society of Mechanical Engineers is a Society of Mechanical Engineers. The Civil Engineers' Society has little or nothing to do with boilers. The Franklin Institute is interested in science and philosophy, and the like, and is not specially interested in mechanical engineering, and there is no other society that fulfills the function that this Society does. Any one who votes on this subject should be a member of this Society.

The Secretary.—In connection with the suggestion of Mr. Smith about deferring action till fall I would explain that, as a matter of convenience, it would be well to finish this subject now, because the sixth volume of the "Transactions," which will close with the papers of this meeting, could thus contain all the action of the Society concerning the Report and the Standard.

Mr. Chas. E. Emery.—The general feeling of the members of the Society present appears to be in favor of the Report, criticisms being directed to minor points. I think it important, if the Society adopts the Report, to have the discussion seem to favor it. I hope that the members will reason with those who have expressed dissatisfaction with special features, and urge them to put their views in such shape that they may not be misunderstood. It is particularly desirable that experts in boiler trials hereafter, who are interested in opposing the Report, will not find a basis of opposition in the language used by those who

really favor its principal features. I expressed myself a few moments ago as thinking it might be better to have the Society vote on the adoption of the Report. It appears to me now, that, if the discussion be put in proper shape, it will add more weight to the Report than its adoption by the Society, inasmuch as many who then vote upon it are not expert in this particular subject. If all who have discussed the matter simply show that they have views as to details, but are willing to yield to the opinions of the majority, their views will add to, rather than detract from, the conclusions presented. I understand that Mr. Babcock, for instance, favors the Report and is not opposing it, but simply stating his views of details. I am almost converted to the view of dispensing with the letter ballot, as a matter of minor importance, and letting the discussion indicate the views of members. Those who are expert in the matter have views, of course, but the discussion can be made to show that they are willing to yield to the majority. This course was necessary in the Committee. While there were no wide differences in opinion on important questions, compromises were necessary as to details.

Mr. Durfee.—I want to reply to Mr. Kent in regard to the legal evidence of any action of this Society. The evidence of any member who is here present will only suffice to establish his own memory and belief relative to the opinion of the Society. He can say what he *thought* was the opinion. He can say what he *believes* was the opinion. But if the court wants the *official opinion* of this Society, it has got to have its records attested by its officers, and there is no other way to get at it.

Mr. Babcock.—I certainly do not wish to go upon record as objecting to this Report. I did not so intend my remarks. I personally would have been much better pleased if the Committee had confined themselves to one definite well-known unit instead of taking three; but I have no fault to find with the Committee, or other criticism on this Report. I am satisfied with their Standard, and would certainly like to see it become the standard of the country; whether I would like this Society to put itself upon record by formally adopting the Report, is another question, which I am not prepared to decide.

Mr. Partridge.—There is one phase of the discussion perhaps to which no one has called attention, which is as important as any, bearing as it does on the feeling of the Society toward the whole Report. Here is a Report covering a code for boiler testing, sixty-eight pages in length. In our discussions here, twenty-two lines only of this Report have been unfavorably criticised, and the remainder has been accepted by every man present, I presume, as entirely harmonizing with his own views. Now it is well for us to remember, that less than one-half of one page, or not quite one per cent., has been made the subject of any adverse remarks whatever, and if the feeling of the Society in the discussion confers any value upon a Report, this one has had a most hearty commendation. There are eighteen lines on the eleventh page, I believe, which have been mentioned, and Mr. Emery's paragraph in the Appendix, on the calorimeter, is, I think, six or seven lines more.

Mr. Emery.—That would hardly be considered part of the Report.

Mr. Partridge.—This makes my position so much stronger. We find but eighteen lines out of the whole Report have been objected to by any of the members in the whole Society. This shows that the proposed code is considered suitable for adoption in all cases where members have to make boiler tests.

Mr. Root.—I would ask if there is any motion before the Society in regard to the acceptance of this Report?

The President.—It has been accepted.

Mr. Root.—And the question now is merely upon its adoption by a letter ballot?

The President.—As the official code approved and indorsed by the Society.

Mr. Root.—Then the matter is finished with the exception of this letter ballot indorsing the action of the Committee.

The President.—I think the Committee may feel highly complimented by the fact that the Society has recognized not only their great labor in the matter and the great thought that has been bestowed upon it, but they recognize the truth and value of everything that is in that report. I think that is very apparent. Has Professor Sweet any suggestion to make as to the time when this matter shall come up?

Professor Sweet.—I am almost disposed to withdraw the resolution. Those who were at first disposed to favor submitting the Report to the whole Society seem now pretty well satisfied to submit it to the members present only. So far as I am personally concerned I should be willing to leave it in that way.

[At this point the morning session adjourned].

AT THE AFTERNOON SESSION.

The President.—The first thing in order is Professor Sweet's resolution.

Professor Sweet.—I wish to withdraw the resolution that I offered before, as I understand there are some amendments that gentlemen present have written out which they wish to present.

These amendments were then presented as they appear in No. clxviii—A.

Mr. Kent.—I second the proposed amendments for the purpose of having them brought properly before us and discussed. But the Report itself not having been adopted, I do not see how the Society can proceed to adopt amendments to it. The only course I see that these amendments can possibly take is to refer them to the Committee. The Committee having been thanked for its labors and discharged no longer exists. I do not see what can be done unless the Secretary might take these amendments and write a circular letter to the individual members of the Committee, and if a majority of them consent to having them substituted for the report, it might be done in the printed Transactions in that way. If they decide not to do that, these proposed amendments might be printed in the Transactions as proposed, and the fact stated that the Committee did not accept them. But it is questionable whether even that should be done, because there were perhaps fifty such amendments proposed during the meetings of the Committee, by the different members thereof, which have been suppressed.

Mr. Partridge.—Perhaps it would aid the consideration of these things to make a statement of the condition in which the question now is before the Society. The Committee has nothing further to do with its Report, since this Report has been accepted by the Society, and by a vote the Committee has been discharged. It is now before us simply as a code for our guidance. We are now proposing to decide what form it shall have, and whether, as a Society, we will adopt it as a code for our guidance. Therefore amendments to this code, and not to the Report, are in order and can be considered like amendments to a resolution before the Society.

Mr. Kent.—I beg leave to differ. The code is not before the Society for adoption; we settled that, I think, by the discussion this morning and by the withdrawal of Professor Sweet's motion. As it stands now, the Report is received, the Committee discharged, and the Report accepted to be published in the Transactions as it stands. The Society cannot change the Report. The Report is

finished when we hand it to the Society and sign our names to it, and they can either accept it or reject it. They have accepted it, and the only persons who could amend it now would be the individual members of the Committee by their meeting and requesting that it be amended.

Mr. Partridge.—There is a little misunderstanding. This Society and many others fall into the habit of making a motion to accept a Report after it has been read. Such a thing as that, according to the best Parliamentary usage, has been decided to be an absurdity. We accept a Report when it is read and the Committee has nothing further to do with it, and when the motion to discharge the Committee is made and carried, that Committee ceases to exist, and, except as individual members of the Society, can have no further concern in that Report. This Report is in the form of a code proposed for adoption. The Report being made, the Committee have no more to do with it than they had before they were organized. In other words they are now individual members of this Society. We have only handled this so far as we have discharged that Committee. Their Report is before us and we can do, according to Parliamentary usage, three things with it. We could refer it back to the Committee if we had not discharged them, or if we had, to another Committee. We can lay it on the table, or, as it recommends something to be done, we can adopt it and thus do what has been recommended. Now that code is essentially a motion to do, and is open like any other motion to amendments or to changes. That, however, does not, as Mr. Kent suggests, make it possible for us to alter the Report. This is made and finished. Neither Society nor Committee can make any change in that. But the code we can change; we can adopt or reject it as a rule for our guidance, and, therefore, when that question comes up, as it must in due course, the amendments are perfectly proper and in due Parliamentary course.

Mr. Stratton.—As I understand it, this Report is now before the meeting, and according to my understanding of Parliamentary usage, it is proper for us to determine as to whether we will adopt this Report as a whole, or adopt it section by section or paragraph by paragraph. Therefore, I would offer a motion that we adopt this Report as offered by the Committee.

Mr. Kent.—I think both the gentlemen are led astray by their conception of Parliamentary rules. This Society does not exist as a Parliamentary body for the purpose of passing resolutions. Certainly by the withdrawal of Professor Sweet's resolution, and by our permitting him to do so we have expressed our refusal to submit this code to letter ballot. We have taken the stand that the Society does not adopt anything; that it will adopt the Report of no individual or no Committee; that it will not put itself on record as adopting anything. It has no creed and no platform, except such as are in our Constitution and By-laws. This code is not now before the Society for adoption at all. It is not proposed by this Committee, or by any individual of the Committee, that it should be adopted. The general trend of the discussion, as it was summed up in the withdrawal of the motion, was that this Society shall not adopt anything. If it cannot adopt the code, how can it adopt an amendment to the code?

Mr. Sweet.—I have prepared a resolution to present in place of my former one offered this forenoon, and I think it will put the matter in the correct light and to the satisfaction of all concerned. The resolution I wish to offer is as follows:
Resolved, That we *recommend* that members of this Society adopt in their practice the code for conducting boiler tests, which has been presented by the Committee, and that the standard adopted by the Committee as to what shall constitute a horse-power, be accepted as a standard in cases of litigation.

It seems to me this opens it for amendments. This presents the code to the Society, and leaves it so that if any one has an amendment to propose to the code he can do so.

Mr. Green.—I second the motion of Mr. Sweet.

Mr. Towne.—With all due respect to Professor Sweet and the other speakers, I sincerely trust that this motion will not prevail. It is in fact the same as the one which was presented this morning. It commits the Society as a body to the indorsement of this Report, as embodying a proper method of making boiler tests. So far as I know, that method is probably the best mode of making such tests that we have, but if this Society, as such, is to be committed to that indorsement, it should be by a vote of the whole membership (by letter ballot, in my opinion) as has been proposed—not merely by a vote of this meeting, at which there is but a very small fraction of the membership present. But a vote by letter ballot, or even a vote taken while the whole membership is present at any one time, on a subject of this kind, would not, in my opinion, carry with it the force that such a vote ought to carry to the outside public. The Report deals with a special branch of engineering practice ; one with which the members of the Society who made that Report are intimately familiar, and about which they probably know more than any other five members of the Society. It is proposed to present that Report to the other members, to vote yes or no upon its adoption. If each individual member who is going to vote yes or no upon that question has a training and experience which qualifies him to vote intelligently upon it, then his vote means something and is desirable, provided that he has also been able and willing to read the Report carefully. But I venture to say that not more than ten per cent. of our membership is competent at the present time to pass upon such a question. Not all of us are practicing steam engineering, and even those of us who have had occasion to undertake boiler tests—as for instance I did at one time—have not necessarily been able or obliged actively to keep up that branch of engineering practice, and to that extent are disqualified, at present, to pass any judgment upon these questions without first bringing themselves up to the state of the art at the present date, and also fully studying and comprehending what the Committee has done. Granting all this, it follows that when a member, not having this intimate knowledge of the subject, has this question to vote yes or no upon, he has either got to say frankly, I don't know anything on the subject, and votes yes or no—whichever way takes his fancy (in which case his vote is worse than useless) ; or, he has to qualify himself to vote intelligently by doing an amount of work which it is not to be supposed he will do for the present purpose. Besides all this, if the Society takes the ground of officially adopting the Report now before us, it follows that other subjects will come up for official adoption, and, sooner or later, it will be sought to give the weight of the Society, as a body, to the enforcement of certain theories and certain rules, which some of us may be sorry to see done, and I think the proper course, this being the first time the question has come up, would be not to act upon it now and in this way. But if the contrary opinion prevails, that the Society should as a body take some action upon this matter, then I think that the first step should be to present this question to the Society first, namely : Shall the Society, as such, by a vote of its members give its indorsement to *any* Report of this kind, so that it shall go forth to the world that such and such a method of procedure (as it happens to be in this case), or, that such and such theories and opinions are those *officially* of the American Society of Mechanical Engineers. That question should be put to the Society, and a vote by letter ballot had on that first. If the

result of that vote is affirmative, then the question of the indorsement of this Report should be submitted to the membership.

But can we not avoid all of this difficulty in the present case without trouble? A vote was passed this morning by which the Report was accepted and the Committee discharged with the thanks of the Society. Now if any members present think that the Report can be improved in certain directions, let them embody their suggestions in a brief statement, and have that filed and incorporated as part of the record of this meeting. It goes out to the world then, together with the Report, as the opinion of such individual member, as to certain improvements or modifications in the methods suggested by the Committee. As Mr. Kent has said, that Report is accepted, and amendments to it are not in order. It is not competent for this meeting, as the matter stands at present, to discuss anything in the nature of amendments to the Report. They can consider, if it is brought up in proper form, any views or opinions differing from those set forth in the Report, but we cannot act upon such opinions presented as amendments to the Report. I think that before taking any vote upon the motion of Professor Sweet, as just read, this fact should be carefully considered, that an affirmative vote contemplates the commitment of the Society to the official indorsement of this Report, so that it shall go out to the world as a programme approved and indorsed by this Society, and inferentially by every member of the Society. If we take that course in this matter, we shall be called upon to take it in many others.

Mr. Durfee.—For the purpose of getting at the sense of this meeting in regard to what is the best course to be pursued, I move that the Report of this Committee and the discussion thus far thereon be printed in the next volume of the transactions, and that further discussion of the Report be dispensed with, and the whole subject of its adoption be laid on the table.

Mr. Partridge.—I second the motion.

Mr. C. E. Emery.—I merely suggest as an amendment to that last motion that parties have permission to correct their discussion. I do not know that it is necessary.

Mr. Durfee.—That goes as a matter of course under the Rules. All discussions are sent to the parties taking part in them.

Mr. Sweet.—If it is the will of the meeting, I will withdraw the original motion, with the permission of my seconder, and let Mr. Durfee's go as the one before the house.

Mr. Durfee's motion was carried, and the discussion closed.

Index photographed at the beginning for the convenience of the microfilm user.